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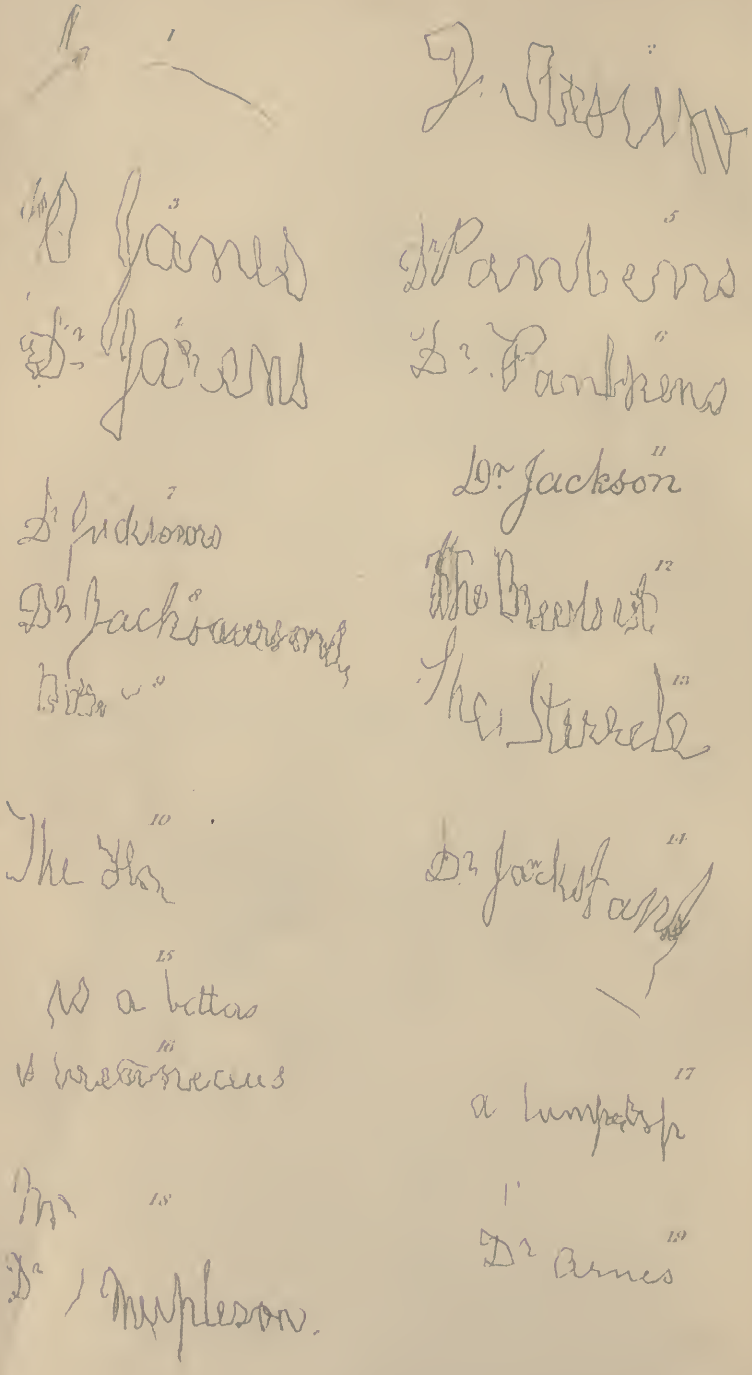

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In this fair world of ours
Calling forth the skylark's note
And wakening ~~iop~~ the flowers

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On forth the skylark's note
And wakening up the ~~st~~ saplowers

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The woods are aprouke in deeper
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view from thence is [109] very striking, commanding a
d wide & ~~x~~ range of the European and Asiatic
ores, and of that gloomy and ~~too~~ turbulent
a so celebrated in the songs of the sunny
chipelago.

Dear [110]

I am

very much obliged to you
for your kind present

I am
Dear Sir.
Yours truly.

Part First.

ORIGINAL COMMUNICATIONS.

Article I.—*Case of Typhus Fever, followed by Right Hemiplegia and Loss of Intellectual Language, both Articulate and Written* (Amnesic Aphasia). By R. E. Scoresby-Jackson, M.D., F.R.S.E., Fellow of the Royal College of Physicians; Physician to the Royal Infirmary; Lecturer on Clinical Medicine and on Materia Medica, Edinburgh.

It would be simply a waste of time and space to repeat what has already been so ably written in the various journals respecting that condition of the body which is now commonly expressed by the word *Aphasia.* All that is at present known about it has been published, and what is now required for its further elucidation is a record of carefully-observed facts. In relating the following narrative, therefore, I shall not enter upon the consideration of its physiological or pathological bearings, but merely describe the facts as they occurred.

As a contribution to the subject, this case seems to be interesting chiefly from three circumstances,—namely, that it is that of a well-educated gentleman, and therefore the contrast between his former and subsequent condition is the more striking; that hemiplegia and loss of intellectual language occurred in connexion with typhus fever, in which respect, so far as I can learn, the case is unique; and that all the facts that are to be mentioned were carefully recorded in my note-book in presence of the patient, and therefore the narrative is not in any respect modified by tricks of memory.

Typhus Fever.

Mr A. B. X., age 21, etc.,[1] had been invited to meet a few friends at dinner at my house on Friday the 5th of January 1866, but an hour before dinner-time I received a note from him excusing himself on the plea of a slight indisposition. On the 8th (Monday) I called upon him at his lodgings, and found him in bed. He told me that on the evening of Wednesday the 3d of January he had been seized with shivering and headache, but believing that these were merely the symptoms of a common cold, he had treated himself accordingly. He had been freely purged with Henry's solution of salts, and had taken one or more doses of James's powder. On examination, I told him that I believed he had contracted typhus fever, and that, reckoning from the date of the rigors, he had arrived at the close of the fifth day. From this date, at his own request, I set myself to watch him through the fever, and obtained, from the Sick Nurses' Institution, a nurse who proved to be in all respects a most desirable attendant.

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[1] Both the patient and his friends have kindly given their cordial permission to the publication of this narrative. They have done so in the interests of the profession, and it would be very unbecoming if I were to publish any facts which would lead to the discovery of the patient. With reference to the previous history of the case, therefore, I feel at liberty only to mention a few circumstances—which were communicated to me by Mr X.'s father subsequent to the hemiplegic attack—and these only very briefly. Mr X., when a child, was not strong; he had the common diseases of childhood; in October 1863 he had small-pox; in August 1864 he had typhoid fever. His moral character is irreproachable. His mother died of phthisis. His maternal grandmother

On the following day (Tuesday 9th), the typhus eruption showed itself, and from that day, until the 20th, he gradually passed through the fever, without exhibiting any peculiar symptoms. A decided improvement occurred on Tuesday the 16th, when the pulse fell for the first time.

	Day of Fever.	Pulse at Morning.	Pulse at Mid-day.	Pulse at Evening.
Jan. 9.	6th,	93	...	93
„ 10.	7th,	92	...	93
„ 11.	8th,	106	...	106
„ 12.	9th,	112	...	110
„ 13.	10th,	118	...	118
„ 14.	11th,	118	...	120
„ 15.	12th,	122	120	120
„ 16.	13th,	116	116	114
„ 17.	14th,	90	...	84
„ 18.	15th,	74	...	...
„ 19.	16th,	65	...	...
„ 20.	17th,	64	...	...

When, at my first visit, he requested me to attend him, I asked the address of his friends, in order that I might communicate with them. This he gave me, but added, "Don't write to them; I don't wish them to know about it until it is over." I readily promised not to write to his parents, but I immediately wrote to Dr ——, physician to one of the largest provincial hospitals, their medical adviser, and corresponded with him afterwards. In a note to Dr ——, on the 13th of January, I wrote,—"Last night he was quite conscious, and I am altogether quite satisfied with his progress hitherto. I hope he will get well over the next three days, and if so, all immediate danger will probably be past; but he will have to be very cautious during convalescence, which, I apprehend, will be slow; and, of course, there is always the risk of developing latent constitutional disease."

On the 17th, I wrote,—"I sent you a telegram yesterday, which I hope would relieve you and Mr ——'s parents of some anxiety.

was also phthisical, but died hemiplegic (of which side is not remembered). Another near relative died of disease of the brain, at a little under 50 years of age. Another near relative (still living) has, since an accident in early life, been affected in his speech, his hearing, and his right side.

Mr —— is doing nicely. On Monday night, the pulse gradually fell from 122 to 114, and last night, from 114 to 90. I hope and believe he will make a good recovery. I need not write again unless something untoward should occur."

From Wednesday till Saturday (17th to 20th), Mr X. made a good recovery. The fever was at an end, and there seemed to be every prospect of a favourable convalescence. The tongue cleaned steadily after Tuesday the 16th; and on Saturday the 20th, it was clean, moist, and in all respects as well as could be desired. The bowels acted spontaneously twice between the 17th and 20th,—the motions, so the nurse said, being both copious and natural in appearance. On the 20th, he was still very weak, but could turn in bed with the smallest assistance. He had the free and full use of all his limbs, and spoke cheerfully.

I need not encroach upon the space of the Journal by publishing the routine of an ordinary case of typhus; but a sentence or two may be necessary to give an idea of the character of the fever. It was an average case,—neither so mild as to cause no apprehension, nor so severe as to cause much alarm. The typhus which prevailed at the time was generally mild, and the mortality comparatively low.[1]

During the early part of the fever, the headache was pretty severe, and he was sleepless and restless; but on the 11th, the headache disappeared, there was much less restlessness, and a tendency to sleep. Throughout the attack, however, there was neither marked vigilance nor somnolence. There was no delirium, neither violent nor muttering; but after the headache left him, his remarks were somewhat incoherent, and, although he was not deaf, he did not answer questions so promptly as he had previously done. He seemed to be confused, but still always answered my questions respecting his state in a tolerably distinct manner. He never professed to be better than he really was, and, on the other hand, he made no moaning complaints. There was no paralysis; his movements were generally slow; but he protruded his tongue, and moved the muscles of his face and limbs without difficulty; there was neither tremor nor twitching of the muscles; there was no involuntary action of the bowels or bladder, nor retention of urine. The urine was tested two or three times for albumen, but there was none. There was a tendency to constipation, which was a little troublesome,—causing some uneasiness of the bowels, which were probably rendered more obstinate by the free purging which had been produced at the outset of the fever by means of Henry's solution of salts. The only other point that it is necessary to refer to is the muscular prostration, which was almost extreme. It was the symptom about which I was most anxious. On the 14th and 15th, the first sound of the heart was quite inaudible at the base. But he took his nourishment readily, and, although he disliked stimulants, he had about four ounces of sherry daily from the 13th. He took beef-tea and milk pretty freely, but what he most liked was cold tea. The eruption was neither scanty nor copious. I have no record of the temperature. There was no pulmonary complication.

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[1] To give an idea of the mortality, I may mention that, from 1st October 1865, to 31st March 1866, inclusive of both days, there were admitted into the Infirmary under my care 326 cases of distinct typhus, of which 206 were males, and 120 were females. Of these, 32 died, or 1 in 10. The average age of all the cases was 23·7 years; the average of the ages of those who died was 36·7. Thus, over a period of six months, equally divided before and after the 5th of January, the day on which I first heard of Mr X 's indisposition, the mortality was 10 per cent., which, considering that no deduction is made of those cases which died soon after entering the Infirmary, nor of others which were complicated with delirium tremens, or engrafted upon other diseases, is an evidence of a somewhat mild epidemic.

At my visit on Saturday, everything promised well. There was not, if I except the muscular prostration, a single bad symptom, nor anything whatever to indicate the advent of an alarming change. He had been taking a little calves' feet jelly, the pulp of a few grapes, and a little toast, in addition to the diet already mentioned; but the next day he was to have something like a dinner. We spoke of fish and a chicken, and he chose the latter. When I asked him how he was at this visit, with a beaming, hopeful countenance, so expressive of convalescence from fever, which none can wear but one who, after a hard exhausting fight, is enjoying complete repose, he replied, with increasing emphasis, "*Oh! I feel charming.*" As I was leaving his room I said, "Well, you'll enjoy your dinner to-morrow?" He replied, "Oh! *shan't* I." He never spoke to me distinctly again.

HEMIPLEGIA.

On Monday the 22d of January, I wrote to Dr ——: "It is with great regret that I have to announce to you a serious change in Mr ——'s state, which, although by no means rendering his case hopeless, makes his recovery a matter of the deepest anxiety. Day by day, he improved so steadily and satisfactorily, that on Saturday I requested the nurse to get a chicken, with the view of his having a little of it as a first dinner on Sunday. But when I visited him yesterday (Sunday) morning, I found a serious change. The tongue, from being moist and clean, had become dry and brown, and there was a very slight appearance of facial palsy of the right side, with a difficulty of articulation, evidently incipient aphasia. I saw him again at night, and thought him a little better, the bowels having been in the meantime freely opened by a dose of castor oil. This morning I was sorry to find him *hemiplegic*, the right being the affected side. No cause of an external kind can be given for the change in his state, and I have no doubt it has arisen either from embolism or from slight effusion, but probably the former."

To give the history of the case at this stage more fully, I will transcribe my notes taken at the bedside:—

"*Sunday, January 21st* (*eighteenth day of fever*), 10.30 A.M.—Great change in his appearance; tongue dry and brown; pulse 60, of fair strength, and quite regular; speech affected, can scarcely articulate; when the face is quiet there is no appearance of paralysis, but when he attempts to speak the left side of the mouth does most of the work, and when he smiles—which he does at his imperfect speech—it is the left side of the face that is lighted up, though the right side has some expression. He appears to be quite conscious, and understands the questions that are put to him, but cannot give a distinct reply; he can get two or three words out, which are indistinct, and then, after a short effort to complete the sentence, he fails, and the attempt to give utterance changes into a broad smile at his inability. He nods or shakes his head in reply to questions, and yawns a good deal. The nurse noticed the change at 8 P.M., but she thought it was due to sleepiness. The movements of the eyeballs are quite normal, there is no squint, and the eye follows the finger held before it in all directions, the same on both sides. The pupils are neither dilated nor contracted, they are both alike, and equally sensitive to light, that is, they both contract and dilate rapidly on alternately exposing and covering them. There is no loss of voluntary power, except on the right side of the face, where one feature is more remarkably altered than the rest, namely, his inability to completely close the right eyelids; the left eyelids close perfectly, but the right leave a chink of about one-eighth of an inch between them in the middle, through which is to be seen the sclerotic with a very small portion of the lower arc of the cornea, the pupil being quite covered. He moves the eyelids of both eyes in winking, and when aroused, but the upper eyelid of the right eye does not drop so smartly nor so far as that of the left eye. He has perfect control over the limbs of both sides, and moves them in obedience to my request in all directions; he grasps equally well with both hands, and protrudes the tongue evenly to the extent of three-quarters of an inch beyond the lips.

"His attempts at speaking are like those of a man who has something important to communicate in a language in which he has the command of but few words, with perhaps this exception, that he is amused and not irritated by his inability to proceed. The few words which he has the use of are indistinctly uttered, as, for example, when I ask him how he is to-day? with an effort he replies, 'Mo mush bessa,' for 'not much better.' When I repeat a question which he is unable to answer, he does not endeavour to make me understand by signs, but either closes his eyes or turns his head away. He does not care for food, takes what is offered, but has not the relish for it which he has evinced during the last few days."

After calmly reviewing the case, and carefully considering the symptoms which I had then noted down, I felt convinced that it was not one for active interference, and that whatever was done should be with the view of supporting the patient through this critical juncture. The only point about which I had any doubt was as to the propriety of making a direct counter-irritant application to the head or neck, and I concluded that it would be unwise, both in consideration of his present condition, and in the view of future consequences, to make any application which would cause discomfort in these parts. The only derivative applications that were made consisted of two large mustard poultices which were wrapped round the calves of the legs, and the application of iodine liniment to the left side of the head. Enemata were offensive to him; and as I did not think it advisable to give him a severe purge, I gave him half-an-ounce of castor oil. I left strict injunctions that he should be kept perfectly quiet, with the room as airy but as dim as possible, so as to encourage sleep; that he was to be cautiously nourished with beef-tea, milk, and wine (the latter to the extent of between two and three glasses in the twenty-four hours), and that he was to be moved as little as possible, either in feeding or other circumstances.

"8.30 P.M.—Pulse 63; tongue dry and brown; still nearly speechless, but quite sensible; has taken about half a pint of beef-tea, wine, and a morsel of toast since morning. The bowels acted at one o'clock. He seems rather better. To be kept perfectly quiet."

At my early visit on Monday morning (22d), I found him hemiplegic, the right being the affected side. The following notes were taken at the time:—"His face when quiet is not remarkably altered, but when he smiles in recognition of me, it is done by the left side, the right half remaining completely placid. He winks with both eyes, but cannot quite close the right eyelids. The right arm and right leg are utterly powerless; when raised they drop like lead, and when he is asked to move them he has no power to do so in the least degree. The tongue can scarcely be protruded beyond the lips; it is flat, is not drawn to one side, and is a little cleaner and moister than last night; but still brownish and dryish. The bowels acted copiously once during the night. He is quite speechless, but nods and shakes his head feebly when interrogated. When he smiles, it is with the left side of the face; but when he moves the jaw, as in mastication, both sides of the face seem to act, at least there does not appear to be any loss of fulness of the muscles of the right side, nor anything to indicate that the right side is not working, although no muscular contraction is visible. But he slobbers out of the right side of the mouth when offered a drink. He is still intelligent; but it is more difficult to arrest his attention than heretofore, and he sooner becomes abstracted. He has no difficulty in the act of deglutition; but it is performed by a series of gulps. He breathes calmly; there is no snoring, no stertor, and no puffing out of the cheeks or lips. He yawns frequently, but seems more to stretch his mouth as in the act of yawning than to take a deep inspiration; it reminds me at this moment of the yawning of a cat more than of a human being—a wide gaping without inspiration; this he repeats every two or three minutes. Pulse 55. Has slept a good deal during the night; has had beef-tea and wine occasionally, and for breakfast a lightly boiled egg, tea, and half-a-slice of toast."

At mid-day I asked my friend and colleague Dr Sanders, senior ordinary physician to the Royal Infirmary, to see the case with me. We made the visit at 1.45 P.M. Dr Sanders carefully examined the patient, and the result of our deliberations was that Mr X. was to be kept quiet, to be suitably nourished, and that we were to wait patiently, and without any meddlesome interference.

It would be tedious to transcribe my notes in full respecting this part of the case; I shall merely give a few extracts to show the progress of the hemiplegia. But before doing so, there is one incident of importance which I must allude to. I called again at 4.30 P.M. (22d), when the nurse mentioned to me that he had not passed water since morning. The bladder was not distended, and in all other respects he was precisely in the same state as when I had seen him with Dr Sanders three hours before. At my evening visit I took a catheter with me, and, as he had still not passed any urine since morning, I introduced it into the bladder. Not more than an ounce of urine flowed through it, and as the bed was quite dry none had passed involuntarily. I ordered hot linseed-meal poultices to the region of the kidneys. In all other respects he was the same as before; there was no aggravation of any of the symptoms. At my morning visit on the 23d, the nurse told me that he had involuntarily passed water in the bed once during the early part of the night, but that at 6 A.M. he had passed about ten ounces into the vessel. This was muddy-looking, and when examined at the Infirmary at noon, presented these characters: acid; sp. gr. 1025; became transparent on the application of heat; contained no albumen; chlorides slightly diminished. With this exception, there was neither suppression nor retention of urine throughout the case.

Dr Sanders visited frequently with me from this date, and occasionally Professor Christison also saw the case with me. The progress of the hemiplegia will be seen from the following brief extracts:—

"*23d.*—Right side completely paralyzed. . . . . *24th.*—Closure of right eyelids still a little less perfect than left. . . . . Still smiles with face distorted to left side. . . . . Speechless. . . . . Moves right leg a little, and gives a very feeble squeeze with the right hand, but cannot move it nor the arm. . . . . *25th.*—Can protrude tongue further; can move right arm a little; and can grasp my hand with his right hand so firmly that when I raise my hand his arm clings to it, and does not fall. . . . . When the fore-arm is raised it does not fall so suddenly as before, but gradually droops. . . . . Moves both legs equally well when told to do so. . . . . *26th.*—He can now lift his right fore-arm a very little, and can grasp pretty firmly with the right hand, and can also offer considerable resistance to movements which I make with the hand, wrist, and fore-arm; the elbow rests always on the bed; right eyelids scarcely meet; protrudes tongue well; moves the right leg freely and forcibly; nods or shakes his head in reply to questions. . . . . *27th.*—Cannot raise the right arm fully off the bed, but when it is raised for him he can hold the fore-arm up for a moment or two, and then it gradually droops; firm grip with right hand, and considerable power in fore-arm. . . . . 28*th.*—Can raise his right fore-arm, and can give a firm grip with right hand. . . . . 29*th.*—Moves fore-arm much more readily, but cannot raise elbow from bed; closes both eyes completely; moves legs well and strongly; smiles with left side only." And so on, gradually gaining muscular power; on the 4th of February he sat up in bed for a little while, and also rose to the night-stool; on the 7th he walked to his bedroom door and back to bed again (about twelve feet). "18*th.*—This morning he dried his face with the towel in his right hand for the first time. . . . . 22*d.*—Walked about the room for about five or six minutes this morning whilst the bed was making. . . . . 27*th.*—He is in the sitting-room in his ordinary walking attire."

On the 10th of March he walked outside, and from that time he went about freely, walking without assistance, firmly, and without catching the ground, or otherwise showing symptoms of paralysis in the right limbs, except in the handling of small objects, which he continued for some time to do in a *fumbling* manner. The face also continued to be distorted to the left side in smiling.

The following is a record of the pulse noted at each visit during the hemiplegic state; at one time it was very slow and feeble, but always regular:—

	Morning.	2 P.M.	Evening.
January 21. . . .	60	...	63
" 22. . . .	55	55	57
" 23. . . .	54	50	45
" 24. . . .	53	53	50
" 25. . . .	54	52	50
" 26. . . .	55	52	56
" 27. . . .	55	54	52
" 28. . . .	53	...	57
" 29. . . .	52	52	54
" 30. . . .	58	...	62
" 31. . .	55	...	63
February 1. . . .	66	64	64
" 2. . . .	60	...	61
" 3. . . .	55	...	57
" 4. . . .	56	...	60
" 5. . . .	62	...	56
" 6. . . .	59	68	...
" 7. . . .	65	...	66
" 8. . . .	57	78	...
" 9. . . .	60	...	60
" 10. . . .	68	70	...
" 11. . . .	66	...	68
" 12. . . .	63	...	70
" 13. . . .	70	...	70

From that date the pulse went on improving both in strength and frequency, until it became normal. The excited state of the circulation in the afternoon of the 8th was partly due to the arrival of his mother (his father and an aunt having previously been with him), and partly to his being in the sitting posture when the pulse was felt.

The treatment of the case in the way of medicine was very little indeed. At first he got nothing; after some time he took mild vegetable tonics, then small doses of *liquor strychniæ* for a time, and at a much later period he had occasional applications of galvanism. At first he passed his time almost entirely in sleeping, being aroused only for his meals. His diet was gradually increased until he took almost a double portion at each meal, his appetite becoming ravenous, until, after the lapse of a few weeks, he fell into his old habits. In these respects he resembled all typhus convalescents.

Loss of Intellectual Language.

As this part of the case will be regarded with greater interest than the others, I will transcribe my notes more fully; but as it would require far more space than I can reasonably ask in order to publish them without abbreviation, I must endeavour to make such a selection as will convey something more than a mere sketch of the case without entering into complete details.

I have already described the patient's condition on the 21st and 22d of January, and I find that until the 28th the chief records are that he was utterly speechless, and that he nodded or shook his head in reply to questions.

28*th.*—"Still no utterance whatever. . . . . I asked him if he could write his name in my note-book, and gave him the pencil; he took the book, but was too feeble in the right hand to hold the pencil up to the book, and as he appeared to wish not to be troubled, I did not press him."

29*th.*—"No utterance . . . . answers by nodding or shaking his head. . . . . I observe that when I request him to do something which does not task his judgment he responds promptly, as when I tell him to put out his tongue, shut his eyes, move his legs, etc.; but when I ask him a question which involves an opinion, he is slow to answer, and if he does not reply at once, he shuts his eyes and gives up. To-day I notice that his port wine is nearly done. . . . . I ask him, 'Do you like your wine?' Nods promptly. 'Do you like your port wine?' Nods promptly. 'Would you like the next bottle to be *port* or *sherry?*' No answer. 'Port?' 'Sherry?' No answer. I repeat the questions; no answer, looks confused and shuts his eyes. I ask, 'Would you like more port wine?' Nods promptly. . . . . His father tells me that he thinks him much better, and quite intelligent; 'For,' he says, 'although he cannot speak, I who know him can converse with him by the expressions of his countenance.' . . . . Tried to get him to write his name in my note-book. He managed to raise up both arms (forearm on right side), to hold the book in the left, and the pencil, with a pretty firm grip, in the right hand, and to bring the book and pencil together; but after an effort, sustained for about half a minute, he gave it up having made only one or two scratches.[1] When he dropped his hands he

[1] This and subsequent numbers refer to the illustrations.

laughed at his inability to do what was asked, and this laugh was the nearest approach to utterance that has been heard since he became speechless; it was simply a short hollow sound very like the sound emitted by persons with cleft palate."

30*th.*—"*Note*—I have frequently recorded that he answers some questions by prompt movements of the head or limbs according to the nature of the request, but I have never (nor has any one) seen him make any spontaneous sign, or give any indication of a desire for anything whatever. When Dr Christison was at the bedside this morning, I asked Mr X. if he knew who it was, and he nodded assent; but when I asked, 'Is it Dr Sanders?' he still nodded assent; and when I asked him, 'Is it Professor Christison?' he appeared a little confused, shut his eyes and gave up, although he had known Dr Christison before his illness. Another thing I have remarked, that whilst his assent is a sharp, short, decisive nod, his negation is a heavy, slow, and complete roll of the head, touching the pillow with each cheek alternately, when lying on his back, and even when lying on one side of his face, the roll extends from that position to one with the face directly upwards: it is not a sharp shake so much as a roll."

31*st.*—"Tried to write his name in my note-book; but the pencil, held in the right hand, was stationary, whilst the book, in the left, made the necessary movements."[2] (I think I may venture to publish this attempt, for it is not likely to be deciphered; but he very soon recovered the power of writing his own name with facility, and quite legibly.)

1*st February.*—"During the visit, when Dr Sanders was at the bedside, I asked, 'Do you know who this is?' Nods assent. 'Is it Professor Christison?' Nods assent. 'Is it Dr Sanders?' Turns his head away and shuts his eyes, as if tired of trying to identify Dr Sanders. He does not tire in this way when asked to perform any movements with his limbs, which he does promptly and pretty vigorously; indeed he seems rather proud of, or grateful for, his accomplishments, and would continue the movements longer than is necessary. When I sit down by his bedside and take out my watch, he at once extends his left arm to me without my making request either by sign or word for it. The left pulse is the one I have always used since I discovered an abnormality (high division) in the right radial artery."

3*d February.*—"I asked him to write my name in my note-book —I said, 'Write Dr Jackson.' He held the book and pencil pretty well, and moved his right hand in making the characters. After he had done it, and had examined it, he uttered a loud and prolonged laugh, and I thought he was going to speak; the noise was exactly like what would be made, and such as I have heard made, by a man with his jaw dislocated, and when I put a question to him he made a feeble attempt to speak, which was also like what a man with a dislocated jaw would utter. The laughter continued for about two minutes in occasional fits. The first attempt to write Dr Jackson is apparently '*O Janes*,'[3] the second '*Dr Jarens*.'"[4]

3d (evening).—"I asked him this evening, 'Did you enjoy your roast-beef?' Nods. 'Was it roast-beef that you had to dinner to-day?' Nods. 'Was it roast-mutton?' Nods again. It is curious to observe that he generally nods his head when his memory is taxed on a point of discrimination such as this, whilst he instantly fulfils any request made to him to move a limb, protrude his tongue, shut his eyes, or the like. When I ask him, 'Can you speak?' or 'Can you say, Yes?' he always shakes his head and smiles."

4th February.—"I asked him to write my name in my note-book, and pronounced twice or three times very slowly, 'Dr Jackson.' I then walked to the window, in order that he might not be disturbed. When I returned, the book was on the bed, and I found that he had written 'Dr Panbens.'[5] I then sat down beside him and spelled my name, and pronounced it deliberately four times; he wrote 'Dr Panbkens.'[6] As he wrote he stopped from time to time and looked to the ceiling, like a person trying to recall something to mind."

5th February.—"I asked him to write my name in my note-book. I pronounced my name very deliberately, the upper one[7] of these is the result. I then asked him to write it again, in which he seems to have repeated the last syllable.[8] I then asked him to write the word 'better,' and spelled it for him. He wrote '*Bitte*.'[9] I asked him to write the word 'better' again: he wrote what appears to be '*The H*.'[10] Thinking that perhaps although he could not write my name from memory, even when slowly and frequently repeated to him, he might be able to copy it if it were written for him, I wrote my name plainly,[11] and asked him to copy it; the results are these.[12] [13] I rather think, however, that from the manner in which he pointed to what he had written, and followed it with his finger, that he intended it to be a spontaneous communication, and not a copy of my name, although I urged him to copy what I had written."

6th February (morning).—"He made a strong effort to speak, and tried to get the lips into the right position, but just when I thought the word was coming the mouth broke away into a laugh, and he did not succeed. I then asked him to write my name in the note-book. He took the pencil more readily, and has not the same difficulty in getting it fitted into the right hand as he had; but still he always takes it first in the left, and carefully puts it into position in the right. This is his attempt at my name,[14] which I spelled to him. I then asked him to write the word 'better,' which I pronounced slowly, and spelled several times to him. I only mentioned the single word 'better,' but he wrote *is a betters*.[15] I then asked him to write whatever he pleased, when he wrote the following, which I cannot make out."[16] [17]

6th, 1.50 P.M.—"Visit with Dr Sanders. I ask Dr Sanders (who has not seen him since the 1st) to mention what improvement he observes: I write to his dictation—'I notice a great improvement in the right arm; he has all the movements of it, though they are not so strong as the left, and he prefers to use the left. He still has the face drawn to the left side when he smiles, but at other times the paralysis is not noticeable. General appearance much improved; he still makes no utterance; considerable improvement in the strength of the pulse.' We then asked him to write Dr Sanders' name in the note-book. I mentioned the name to him and spelled it several times, and Dr Sanders also mentioned his name to him slowly. This[18] is the result of his attempt to write the name and title, which he first began as Mr."

7th February.—"Writing: I first tried him with pen and ink, but the pen stuck in the paper and he could not get on, so I gave him the note-book and pencil again, and asked him distinctly to write 'Dr Jackson,' but he wrote *Dr Arnes*.[19] I then told him to write whatever he pleased, when he wrote these sentences, the first of which is evidently intended for *This is Better*;[20] and the second, I think, has reference to his right arm, and possibly may be intended for *This arm cured*.[21] The next word was also written of his own accord. It is either *Felbow* or *Fellow*,[22] but I rather think he was referring to his right elbow. I next asked him to try to say something, and after a little consideration and a strong effort he repeated in a stammering manner the initial *st-st-st-st*. I then suggested the word 'strong,' when by another effort he got out *st-st-st-st-rrr-rrr-rrr-ong*, the first part with a hissing sound, the second with a loudish roll, and the last in a whisper. I then asked him to say 'better,' which he tried to do but failed; but on asking him to say 'strong' again, in presence of his aunt, he repeated it in the same way as before."

7th (evening).—"His aunt and the nurse tell me that he said *No* distinctly this afternoon, that he seemed to be startled at the sound of his voice, and then laughed."

8th February.—"Writing: I gave him my note-book and pencil, and asked him to write my name. He took the book, and immediately, and quite alertly, sat up in bed. He then took the pencil in the left hand, and fitted it between the thumb and index finger of the right: this he now does much more readily than formerly. I asked him to write 'Dr Jackson,' and repeated it very distinctly; he wrote *A. Jacksons*.[23] I said, 'I asked you to write Dr not A., try again:' he wrote *A. Jaksons*,[24] omitting the c. I then said, 'Never mind, write whatever you please,' and repeated this three times: after the first request he wrote *a very brilliant*[25] (the r of the last word he added after examining the sentence); after the second, *Yes, I have none*;[26] and after the third, *Mother she keeps, Mother cheeps*.[27] I had no clue to the meaning of the two first sentences, and at the time he wrote the third sentence I did not understand it, but I was afterwards told that his mother arrived this morning (I did not expect her until evening), and it was supposed that he was endeavouring to tell me of her arrival. During the whole of this visit his attempts to speak were almost unceasing; the efforts were of a straining kind, as if he could not get the words thrust out, and at last they died in the utterance without being sounded. After each effort he looked as if he had forgotten something, or what he would say, and looked to the ceiling as if trying to recall his ideas, and then made a fresh attempt to speak. Each attempt began with the letter d: thus, *d-d-d-d*, but he could not get beyond it. I suggested to him 'say don't,' when, after one or two attempts, he uttered *d-d-d-d-d-d-on-st-st-st-st-st*. Afterwards, he said *strong* in the same manner as yesterday."

8th (evening).—"His mother tells me that he mentioned my name to her in a manner that she distinctly understood. I asked him to say 'Dr,' which he did in this way *D—octa*, dwelling long and straining over the *d*, when the rest suddenly followed; there was no repetition of the *d* or any other part of the word. When he strains at speaking, he slowly moves his head from side to side as if to help in the expulsions of the words."

To save the space of the Journal, I now pass over a few days.

20th February.—"I offered him my note-book to write, but as he did not seem to be able to originate anything to write about, I said to him, 'Suppose you write this,—This is a very fine day, it is very like a spring day.' This he attempted,[28] but he required frequent promptings after the first few words. I afterwards asked him to write my name, which he did."[29]

February 23d.—"When I gave him my note-book he wrote promptly and without bidding, *Dr Sanders*,[30] *Dr Jackson*."[31]

February 24th (evening visit).—"Some days ago his mother spoke about buying a draught-board, and said that he was fond of backgammon and draughts. It was agreed that I should be present at the first trial. This evening the board was given to him, wrapped in paper just as it had been purchased. I was anxious to see how he would deal with it, and expected to see the same inability to fix the attention and to play the game as I had seen in the writing. To my surprise, however, he not only recognised the board, but at once selected the white draughtsmen and put them correctly into position. I sat by his bedside and played a game with him. To my great delight he played a very good game, and made several very clever moves; it was, in short, quite as much as I could do to beat him, which I fairly endeavoured to do in order to test his powers. The game was very equal until by an unfortunate move he lost three men at once. At one time the contest lay between six kings on my side and four on his. The game lasted about twenty minutes, and he was quite as lively at the end as at the beginning of it. The only utterances that he made during the game were well-modulated interjections, *ah!* and *oh!* which spoke all that was necessary. At the outset of the game, I 'huffed' him for declining to take a man, but he playfully seized the draughtsman, put it back again, reversed the move he had made, and apparently tried to say, 'I had forgotten that I must take you,' or 'I did not observe that.' But later in the game, when I overlooked an opportunity of taking a man, he 'huffed' me with great glee."

26*th*.—"I asked him to write the numerals up to ten, this he did twice, readily, and without prompting. I then said, 'Write 100,' this he did; 'Now, write 300,' for this he wrote 3000; 'Now, write 2718,' this he tried twice, but failed, he wrote first 27717, and then 277717.[32] He had a solitaire-board on the bed beside him. I asked him to play the game; he did so, and accomplished it in a very ingenious manner, without a single false move."

28*th February*.—"He wrote in ink to-day for the first time; he once attempted it before but failed. I showed him my watch, and asked him to write the name of it, but he could not. I then asked, 'Have you got one?' He took out his watch. 'Now write what it is.' He could not. I then said, 'It is a watch, write watch;' he wrote *a match*.[33] I did the same with the inkstand, but had to tell him the name of it, when he wrote *a inkspank;*[34] and for snowdrops, some of which were on the table, when told the name, he wrote *a spinhant*.[35] He then wrote the alphabet,[36] transposing h and g and omitting several letters; but his great difficulty was with the letter x, which he could not manage. After considerable effort, he left a space for it and went on to y and z; he then returned to the space and looked beseechingly at me for help. I did not speak, but crossed my index fingers, which he at once understood and filled up the gap. I then offered him a simple addition sum of two rows, which he did correctly.[37]

9*th March*.—"The nurse tells me, on inquiry, that he occasionally goes to the book-case and takes a book, or lifts a book or newspaper from the table, and looks at it for a few minutes, but, she adds, 'It's hard to say whether he reads any or not.' He is a most amiable patient; he never loses his temper when in difficulty either of speaking or writing, but generally smiles at his mistakes, and endeavours to correct himself or explain. When he is asked to repeat a word or sentence, he always emits a sound for each syllable; as, for example, when asked to say 'The Duke of Wellington,' he replies, *De—De—De—De-de-de*. His 'How dy'e do' has not improved, and he can scarcely be said to be able to articulate a single word, although his *home* and *Doctor* (in a singing tone) are intelligible to me. He frequently repeats the word 'Durner,' which sometimes seems to be intended for Doctor, but at other times it is unintelligible."

10*th March*.—"When Professor Christison entered the room, I asked, 'Do you know this gentleman?' Nods his head, and attempts to reply, but it is only *nn*. 'Write his name;' he writes his own initials well and correctly; 'Not your own name, this gentleman's; *Oh!* he says, and then writes *christison*.[38] 'Very good,' I said, 'but not very polite, should there not be something before the name?' meaning Dr or Professor; he looked at the word, and then, after exclaiming *Oh!* he increased the size of the initial letter."

12*th March*.—"I took with me the volume of the *Dublin Journal of Medical and Chemical Science*, for Nov. 1833, which contains a paper by Dr Osborne, 'On the Loss of the Faculty of Speech,' in order to test Mr X. by the sentence which Dr Osborne put to his patient, 'The Scholar of Trinity College.' The sentence is this:—'It shall be in the power of the College to examine or not examine any Licentiate, previously to his admission to a Fellowship, as they shall think fit.' I said to him, 'Now, I wish you to write a sentence which I will read to you from this book: I will first read it to you without interruption so that you may understand the tenor of it, and then slowly word by word as you write it.' This I did, and after pronouncing a word slowly and distinctly, I waited until he looked towards me inquiringly for another. I sat at a little distance from him, and did not interfere at all with his writing. When finished, he handed the paper to me, when I found that he had written as follows:[39]—*It shall be in the power of the college to examine organe not examine any the Licentiate, but to previously to high by admissionly to a Fellow to sopha as they those think fit.* I afterwards took him for half-an-hour's drive; the motion of the carriage did not inconvenience him; he looked about him with much interest as we went along the streets, and would fain have told me many things which were lost in a babble of unintelligible sounds."

13*th March*.—"At 1 P.M. I called for Mr X. and his mother, and drove them to my house to luncheon. When we arrived, I ran up before them towards the drawing-room, and when I turned at the top of the stair, I found that X. was smiling at a very slight difficulty he had in lifting his right leg at each step. It required but little effort, but still it was quite perceptible. There were no stairs at his lodgings. Dr Sanders called at two, and when we went to the drawing-room again we tried Mr X. at the piano. He sat down quite readily, put himself into the attitude for playing, and placed his fingers upon the keys so naturally that I expected to hear him play without difficulty, excepting such as might arise from the disuse of the piano for nearly three months; but, having come smartly to the starting point, the rest was a blank; he could not play a single bar, even when a piece of music with which he had been quite familiar before his illness was placed before him. Thinking it might be due to a defect in the right arm from the palsy, I asked him to try with the left hand alone; he then managed a bar or two very slowly, but transposed several notes. I then asked him to hum the tune, which he did pretty well. I should mention that he had had a piano at his lodgings up to the time of his illness, and I understand that he could play very fairly. At luncheon—and this is, I hope, a pardonable breach of the laws of hospitality—he behaved with the utmost propriety; there was nothing in the least degree childish in his manner, nor was there any necessity for assisting him in any of the proceedings. He did not appear to be at all embarrassed, and, although he could not speak, he appeared perfectly to understand the conversation. Afterwards, I wrote on a slip of paper, 'Would you like to take a walk with me?' and he wrote in reply, '*Very much.*'"[40]

14*th March.*—"I wrote, 'Where did you take luncheon yesterday?' He wrote in reply, *I take at Dr Jackson.*[41] I then dictated to him slowly and distinctly the following passage from 'The Crescent and the Cross':—'The view from thence is very striking, commanding a wide range of the European and Asiatic shores, and of that gloomy and turbulent sea, so celebrated in the songs of the sunny Archipelago.' He wrote as follows[42]:—'*The view fiew thence is t very the, is commanding a wide and dangre a of range of wide and it asiatic aside, and a of a afore the gloffy*'—here he broke down, not being able to pass the word gloomy. (Compare this with the same passage written on 16th of April."[109])

17*th March.*—"I wrote on a slip of paper as follows:—'I am going to touch the right side of your tongue with one or two different articles, and I wish you to write the name of each after you have tasted it; do you understand?' He nodded assent. I then told him to shut his eyes, and put out his tongue; and when he had done so I rubbed the right side of the tongue with a stick of barley-sugar. I then requested him to write what it was: he wrote promptly, *To keefshage.*[43] I said, 'I don't understand, write it again,' and he wrote, *To sheefsage.*[44] I then repeated the experiment with a little table salt, for which he wrote, *To alumen.*[45] I repeated the experiment with a glass rod previously dipped in dilute sulphuric acid, to which he wrote, *To albuminate.*[46] He did not seem to be satisfied with his written answers, and endeavoured to explain himself by articulation, but was not intelligible. I then wrote the words barley-sugar, salt, dilute sulphuric acid, and told him to point to the one which I applied to his tongue. I then repeated the experiment in a variety of ways, often repeating the same substance two or three times in succession, but he never failed to give a prompt and correct answer by pointing to the name of the substance used.

"I then took a pair of compasses, and, showing them to him, explained that I was going to apply the points to various parts of the right hand, and the right side of the face; that when he felt two points he was to hold up two fingers of the left hand, and when he only felt one point he was to hold up one finger. I applied this test to various parts of the right hand, including the back and front of all the fingers and thumb, and all over the back of the hand, and also to various parts of the right side of the face, from the neck to the temple and forehead, comparing also the same points on both sides of the face. The result was that he never made a mistake when the points of the compasses were half an inch or more apart, but when they were within half an inch of each other he generally made a mistake,—in fact, almost always held up only one finger; but the same result followed the application on both sides of the face. He seemed to be fully as sensitive on the right as on the left side, and the reply in every instance, by holding up either one or two fingers, was instantaneous. His eyes were shut the whole time."

I find that, even by omitting the reports of several days together, I cannot, by merely transcribing my notes, bring them within a reasonable limit. I must, therefore, sum up the remainder of the narrative more briefly. At this time, he was staying at Portobello, whither he had gone with Mrs X., his mother, for a change. On the 5th of April, he came to my house on a visit, and remained with me until the 16th, when he left Edinburgh for his own home. From the 10th of March he was out every day, walked a good deal, and conducted his mother (a stranger to Edinburgh) about the city and neighbourhood, explaining, in a very intelligible manner, the various objects of interest. Physically, he was, in all respects, save one, better than he was previous to his illness. The exception was the remains of the paralysis, which exhibited itself only in the distortion of the face when smiling. On the 17th of March he weighed 10 stones 4 pounds; on the 6th of April he weighed 10 stones 11 pounds. Before he left me, I made a careful examination of the various organs, and found nothing wrong. It is unnecessary to give the report, but it is satisfactory to know that Dr Sanders read it, and verified it also by a careful examination. The urine was never albuminous. After he had regained his strength, in addition to the treatment already mentioned, he had a small blister occasionally, either behind the left ear, or on the left temple, and once, a larger one, to the nape of the neck. He had also twice half-a-dozen leeches applied, once behind the left ear, and once to the nape of the neck. After the application of the leeches, there was a decided improvement in the face,—the right side of it acquiring much more expression.

During his stay with me he was scrupulously neat and tidy, both in his person and arrangements. He gave no trouble whatever, and required no assistance; in short, except that he did not speak, he was, in all respects, an ordinary and very agreeable guest. He played draughts, and, what was more, he taught me, who knew nothing of the game before, backgammon. I find this note recorded, whilst he was at Portobello :—"Mrs X. tells me that he arranges the things in his room as orderly as ever, and never forgets where he has put a thing. In his old lodgings he remembered quite well where each article was to be found, just as he had left it before he took the fever." His playing on the piano improved a little, but he did not care to practise much. He often sat before the piano, however, with the music before him, and hummed the tunes quite correctly and loudly. He frequently took long walks alone, and often called at the house of a very kind lady, who had shown him great attention during his illness, and who understood his gestures. He occasionally received notes of invitation to this lady's house; and when he had read them, he carefully put them in his pocket, and, without much difficulty, explained to me the nature and time of the engagement.

The following is from my notes of the 13th of April:—"When he came to stay with me, I placed a packet of paper and a pencil in a drawer, and told him that whenever he wanted to tell me anything he might write it. He has done so a great many times, but although I have generally made out his meaning, he always wrote a jumble of words, which, without an interpretation by signs, were incomprehensible. The odd thing is, that after writing a long sentence of this kind, he would carefully read it over, correct an error or two in spelling perhaps, and then seriously hand it to me, evidently expecting that I should understand him. His eye did not tell him that he had written nonsense. I generally made out what he would be at, however, and answered accordingly; but occasionally I said, 'Just let me read to you what you have written,' and when I had done so gravely, he invariably burst into laughter at the jargon."

From my notes of the 16th of April:—"Yesterday, Mr X. accompanied me twice to church, in the morning and in the afternoon. I noticed that he quickly turned to all the places in the prayer-book, and found the hymns readily enough. In the morning he did not turn up the text, he did not try; but when we got home, without any reference to the subject previously, I asked him if he could show me the text, when he took his Bible and found the place at once. In the afternoon he endeavoured to turn up the text in church, but I think he failed to find it; but, on arrival at home, he turned it up in his own Bible, though not so promptly as in the morning. Before going to bed at night, I asked him to turn up both texts again, and he did so readily. I should mention that in the afternoon it was as much as I could do to catch the text, so he must have been very alert. I asked him if he had understood the sermons, and he nodded assent."

"He has now several words which he can utter when they are previously spoken to him, but, except yes and no, he never utters them spontaneously. He can repeat after me, *better*, *bed*, *home*, *do*, *don't*, and a few others; and I have no doubt that there are many words which he could utter if first spoken to him. For negation he always says, *No!* with a shake of his head; but when very positive he says, *Oh, no!* For 'yes,' he says either *Det*, *Net*, or *Et*; and I think he sometimes replies *det-ta* for 'yes, sir.' I remarked previously that he often wrote an incoherent sentence without being apparently aware of the fact by looking at it. This is also the case in his attempts to speak; he utters a number of sounds without being at first apparently aware that he is conveying no meaning; but this is not so much so as in writing, for he quickly perceives that he is not conveying any information, and he then resorts to paper. He always utters a sound corresponding to each syllable which he intends to speak; this I have ascertained by requesting him to repeat sentences which I first spoke to him. He never repeats a single word, making it do duty for a sentence or part of a sentence; the only approach to that was the frequent repetition, for a short period, of the word 'Durner;' but he has not employed it for some time.

"With the view of catching the sounds of the names of the letters of the alphabet, I asked him first to run over the alphabet quickly; in doing so he made twenty-six distinct sounds, one for each letter. I then asked him to name the letters slowly; in several instances he could not come near the right sound; of some of the letters he could make nothing until he had examined the arrangement of my mouth when I uttered them, and then he endeavoured to get his own mouth into the same shape, and more or less succeeded in uttering the name of the letter. The following will give an idea of his pronunciation:—a, b, correctly; c, *tee;* d, correctly; e, correctly, or more like *a;* f, correctly occasionally, but sometimes only *ph;* g, sometimes nearly correctly, but more frequently *dee;* h, *ait* or *eight;* i, *ah;* j, *day;* k, sometimes *day*, sometimes *tay;* l, *en;* m, n, o, correctly; p, correctly, sometimes *b;* q, *too*, or *oo;* r, *ah;* s, *et;* t, correctly; u, cannot come near it; v, *phan* (several times repeated); w, laughs, as much as to say, 'that is quite beyond me,' cannot get near it; x, cannot get near it, *epba*, or *etta* were the nearest; y, *ban-na*, cannot get near it; z, *ed-da*."

Such are a few of the leading features of this very interesting case. I could have given much more, for my notes are certainly not half exhausted, but perhaps the selection I have made will be acceptable and useful. I shall close this part of the case with a few illustrations of the writing, and thereby also of the loss of the power of communication by means of intellectual language. I should explain that the questions which follow were not put verbally but in writing, so that he had as much time as he chose to take for the answers; the questions were also put singly, that is, he did not see the second until he had answered the first, nor the third until he had answered the second, and so on; therefore, there could be no confusion of ideas, such as might have been the case if several questions had been proposed at the same time. When simple arithmetical questions are put, the figures in the questions are mine, the answers only his. When he wrote to dictation I read the passage very slowly, pausing after each word until he looked towards me for the next.

He wrote the alphabet on the following, besides other days:—28th February,[36] 6th March,[47] 16th March,[48] 7th April,[49] 16th April.[50]

He attempted simple arithmetical questions on the following, besides other days:—28th February,[37] 3d March,[51] 14th March,[52] 31st March,[53] 7th April,[54] 12th April,[55] 13th April.[56]

On 1st March he copied (correctly)[57] from a printed book the verse, "It were well to be a sunbeam," etc. I then immediately took the book myself, gave him a fresh slip of paper, and dictated the verse.[58]

He wrote the following verse to dictation on 5th March[59] and 13th April:[60]—

> "The shades of eve came slowly down,
> The woods are wrapt in deeper brown,
> The owl awakens from her dell,
> The fox is heard upon the fell."

In answer to the questions, "What o'clock is it?" "Tell me the time," or "Write the time of day," he replied:—9th March (at nineteen minutes to four);[61] 16th March (at six minutes to three);[62] 22d March (at half-past two);[63] 24th March (at twenty minutes to three);[64] 30th March (at twenty minutes to three).[65] "What is that?" (pointing successively to inkstand,[66] draughtsmen,[67] hat,[68] penknife,[69])—9th March. "Name these articles?" (placing before him inkstand,[70] pocket-knife,[71] book,[72] draughtsman,[73] poker,[74] pair of scissors,[75])—13th April.

He wrote to dictation,—"It shall be in the power of the College to examine or not examine any Licentiate, previously to his admission to a Fellowship, as they shall think fit."—12th March,[39] 22d March,[76] 11th April.[77] "How did you sleep last night?"—16th March,[78] 6th April.[79] "What had you for breakfast to-day?"—16th March,[80] 27th March.[81]

"Where were you yesterday?"—17th March (means that he was at Portobello).[82]

"Write the months in their order."[83] "Write the days of the week in their order."[84]—19th March.

23*d March*.—"In what year was the battle of Waterloo fought?[85] Who conducted the battle on the part of England?[86] Who was the French General?[87] Who commanded the Prussian army?[88] In what country was the battle fought?[89] Where was the battle fought?[90] In what country is Waterloo?[91] Of what country is Brussels the capital?[92] In which of the following countries was the battle of Waterloo fought, France, Holland, Switzerland, Prussia, Belgium, Spain, Turkey?"[93]

17*th March*.—"What illness are you recovering from?[94] How long have you been ill?"[95]

6*th April*.—"Are you aware that you are not in the same state that you formerly were; what is the deficiency?[96] Write something of your own accord (tried, but could not). Tell me about the weather."[97]

7*th April*.—"Write the months, with the number of days in each.[98] Tell me what you saw at the Botanic Garden yesterday."[99] (He was alone at the gardens.)

9*th April*.—"Tell me what you saw at Rolands' *Assault of Arms* (Messrs Rolands' annual school competition for prizes in fencing, gymnastics, etc.)."[100]

11*th April*.—"Tell me how you are, and what progress you think you are making?[101] Tell me what kind of a day it is?"[102]

12*th April.*—" Write a verse of ' The Arrow ' ? " (a song which he used to sing before his illness).[103]

13*th April.*—" Who came with me to see you yesterday ?[104] Did he say he thought you better ?[105] And do you think so yourself ?[106] Do you think that you will soon be quite well ? "[107]

14*th April.*—" Tell me how you enjoyed the country yesterday ? "[108] (I understand him to mean that he enjoyed the afternoon, that we were entertained in the house, that we walked by the river, that we gathered primroses in the woods, and saw rabbits in the parks, and that we drove to the station in the pony carriage in the evening, all of which is correct ; but I do not understand the *two-legged* matter.)

On the same day, I wrote the first and third lines of the first verse of the " Busy Bee," and asked him to supply the second and fourth lines, but he could not write a single word. I asked him if he had ever known it, and he nodded assent. I then wrote another verse more fully, omitting only one word in each line, which I asked him to supply. The italics are the words which he placed in the vacant spaces.

" Let dogs delight to *Back* and bite,
For God hath *taught* them so ;
Let bears and lions *bear* and fight,
For 'tis their *let them* to."

On the 16th this was repeated, the ultimate words of the lines being omitted, which he supplied as in the following italics :—

" Let dogs delight to *bite and bite*,
For God hath *sight ;*
Let bears and lions *growly seemly*,
For 'tis *their natures feely*."

I felt sure that he must have been familiar with the verse before his illness ; but, as a still more certain test, I tried him with the Lord's Prayer, and found that he could not write the first dozen words correctly.

Mr X. left me on the 16th of April and travelled home by railway, in company with his mother. He was then placed under the care of Dr ——, with whom I at first corresponded. His father, who had not seen him for more than two months, wrote me on the 18th, —" Dear —— is much better than I expected to find him ; his intelligence and manner of expressing his meaning being far beyond my anticipation. I have no difficulty in holding a small conversation with him. Activity and thought appear to be fast returning, and with careful tuition, we may hope, speech will also."

On the 20th of May, Dr —— wrote to me, " —— is improving certainly, though very slowly. He walks well, and looks in robust health ; but the angle of the mouth is much ' drawn,' and his progress in speaking is but slow, though, I dare say, quite as rapid as you might expect. He can manage the labial and dental sounds pretty well, but has most difficulty with the gutturals. Improvement is also perceptible in the way in which he answers questions on paper." In the early part of the month of August, I accepted a kind invitation to visit at Mr X.'s, and spent a most delightful week with them. I need not enter into details of improvement, suffice it to say that he was much improved in all the respects in which he was formerly deficient. There was scarcely a trace of facial palsy left; he was in robust health, he enjoyed daily pic-nics or sight-seeing without fatigue, climbed over ruins where nobody else cared to follow, and had but little difficulty in making me understand all that he wished to explain. He had got many new words, and had become tolerably proficient in the use of the dumb alphabet. As a last specimen of his writing, I have a note which was written, during my stay, to a friend who had made him a present of a book.[110]

I find that I can scarcely allow myself that freedom of observation which would be necessary to make a commentary upon this case interesting; besides, I think that at the present time facts are more valuable than opinions. I prefer, therefore, to let the narrative stand alone, assured that within itself it contains a mine of physiological and pathological wealth that will amply repay a careful exploration. But although I do not enter upon a commentary, I am unwilling to let the paper go to press without making one or two remarks with reference to my own experience of the complications of typhus fever, at least of such of them as bear some resemblance to the present case. I have said that this case, as one of hemiplegia and loss of intellectual language following typhus fever, is unique; at least, I have never met with one of the kind either in my personal experience or in the accounts of cases published by other physicians. Most cases of typhus fever begin abruptly and pursue their course resolutely to the end, which is either a rapid convalescence or death. Enteric fever, on the other hand, is treacherous throughout. Grave nervous accidents, it is true, do occur in typhus fever, and it is by no means a rare thing to have a patient cut off by coma and convulsions just as he is on the eve of recovery and seems to be out of danger; but still, as a general rule, when a marked amelioration takes place about the thirteenth or fourteenth day of the fever, a favourable opinion of the result may be entertained. It was so in this case; there was no circumstance from the beginning to the end of it that created in my mind the slightest suspicion of even a transient interference with the intellectual powers, much less did I anticipate the sad change—the obviously deep-seated lesion—which has unfortunately attached so much interest to it. When I said in my letter of the 17th, "I hope and believe he will make a good recovery; I need not write again unless something untoward should occur," I made statements which were quite consistent. Looking to the general rule, there was no longer cause of anxiety, but looking to exceptional cases, an accident might still befall the case. No man could have reasonably anticipated such a result. It is true there was a family history of hemiplegia and aphasia; but that was not discovered until afterwards, and even had it been known before, I am not aware of anything that could have been done to avert the calamity more than was done to preserve the patient's life. There was no history of rheumatism, nor was there any valvular imperfection to cause apprehension of an accident in the vascular system.

Speechlessness, aphasia, partial paralysis, and other nervous affections are occasionally witnessed after continued fever—more frequently after enteric than typhus—but they are usually of short duration, and ultimately disappear entirely as convalescence advances. I have seen several cases of this kind, three of which—as they were marked by temporary loss of speech, and occurred after typhus fever—I will briefly mention. An Irishman, æt. 24, was admitted into Ward IX. with typhus fever. He passed safely through the fever, but on one of the early days of his convalescence he presented peculiar nervous symptoms. Amongst the rest, he told the nurse that he was dead, and then refused to speak. This state continued for a few hours, after which all the nervous symptoms disappeared, and he recovered. There was no paralysis.

P. L., æt. 18, passed through typhus fever in Ward IX., and was discharged after an ample convalescence. The following day he returned whimpering, and declaring that he had an eruption upon his hands which we could not discover. He was low-spirited, and wandered listlessly about the ward. On the fourth day after his re-admission—to use the nurse's words—"he hopped about from bed to bed like a roebuck;" but after one of these capers he suddenly tore out the sash of one of the windows, and attempted to throw himself out. The nurse was fortunately in time to save him. From that moment for several weeks he never uttered a word. He was sullen and obstinately contradictory in his manner; and twice, if not oftener, threw his meals at the nurse. After remaining in this condition for three weeks, having no bodily ailment, he was removed by his friends to the country, where he still continued speechless for some weeks longer. Nine months afterwards he called upon me in perfect health, active, and cheerful; and when, amongst other questions, I asked him why he did not speak, he replied, "It was not that I *could* not, but that I *would* not." When I asked him why he attempted to throw himself out of the window, and why he behaved so rudely to the nurses, he said he did so because he was under the impression that they wanted to kill him. There was no paralysis, and from the time of his leaving the Infirmary no treatment was attempted; in fact, he never saw a medical man, but "gradually came to himself."

The third case in some respects more closely, though but trivially, resembles that of Mr X. Mr ——, a gentleman of liberal education, æt. 22, passed through typhus fever under the care of Dr Peddie. He appeared to be progressing favourably, and was considered to be convalescent, when suddenly, on a Sunday afternoon, he became speechless. In the evening I saw him, with Dr Peddie and Dr Begbie, but by that time the symptoms had disappeared. There was no paralysis, the urine was not albuminous. I asked him why he could not speak; he replied, "I don't know why, but I could not if it would have saved my life." During the attack he made signs for writing materials, and when they were given to him he scrawled something which somewhat resembles the word *aphasia.* In the evening he said, "I wanted to write that I thought I was about to become an interesting case of aphasia; but they tell me that I wrote nonsense." He had two more attacks of the same kind, but still more transient; the second, on the following Wednesday; and the third, two or three days later. Since his recovery, he has furnished me with the following notes:—

"The advent of loss of speech was in my case gradual. I lost the power of articulation, and of phonation equally. So long as I was able to say anything audibly, I articulated those words which I wished to pronounce, and when quite unable to say a word, I was also (I think) unable to produce any sound. I never felt as if I did pronounce other words than those I wished, or as if words were suggested which had no reference to the thoughts present in my mind at the time. I never lost my consciousness of what was going on in the room, and believe that if I had had the power of speech, I could have named all the people in the room, recognising them by their voices. I had during all, but especially during the first of the attacks, great muscular prostration, and an intense aversion to make any movements. Thus, when Dr Peddie came into the room, I knew he was there—not because I saw him, but because I heard his voice. I was lying with my face to the wall at the time. I remember distinctly some of the questions which he put to me,—thus, he asked me if I knew him, which I answered in the affirmative by nodding my head.

"I was capable of thought, I firmly believe, because I began to question myself as to the cause of the 'aphasia,' when in the first attack; and having, as I thought, satisfied myself as to its cause, I next began to wonder how long I would be in that state. With the exception of two or three minutes when the first attack came on, I never had any dread as to the issue; but for a short time at the commencement of the first, I certainly did think that it would terminate fatally. I remember distinctly making signs for writing materials, and doing my best when they gave me a pencil, to write the word 'aphasia.' I also remember my aunt saying that it was all nonsense (*i.e.*, what I was writing), and the intense disappointment I felt when she said so. What I said afterwards, when Dr Begbie came, as to their having said that what I wrote was nonsense, was from recollection of what took place during the attack, *not* from anything said to me after I had emerged from it. I have no distinct recollection of how I felt on emerging from the attacks. My impression is that speech returned almost instantaneously, not gradually; but of this I cannot be sure. I have a remembrance of making signs during the second attack, which must have corresponded to the raving of delirium. I sought to communicate ideas which must have been neither more nor less than nonsense. The third attack lasted but a few minutes, and was accompanied by scarcely any of that muscular prostration to which I have referred. Premonitory symptoms of weakness of voice and disinclination to exertion were of longer continuance in the last two attacks. I think that, after the third attack, more than once I felt as if I was going to have another, but it never developed into anything, nor were those feelings even noticed by those attending me."

This was obviously not a case of aphasia, but merely the simulation of aphasia. Previous to his illness he had heard me speak of Mr X.'s case; in the state of mental prostration produced by the fever, the thought had taken possession of his mind that he also was to suffer in the same manner. His will fell into subjection to the prominent idea, it became enthralled and consequently inefficient as an originating cause of speech. It was not that the machinery was out of gear, but simply that the will was not strong enough to set it in motion. It is true that he believed that he was making a strong effort to speak, but the belief that he was, as he afterwards said, "about to become an interesting case of aphasia" overpowered his will, just as the "electro-biologist," by his simple dictum, can produce speechlessness in certain susceptible people even in health. That the thought of aphasia held his mind in captivity, is clear from the abortive attempt which he made to write the word; the true aphasiac would not even have remembered it. In this case there was merely a functional aberration; in Mr X.'s case, there was from the first moment of the attack a deep-seated grievous anatomical lesion.

Cases such as that of Mr X., if painful and full of anxiety to those in charge of them, are nevertheless pre-eminently interesting. They teach us practically to be cautious, even when there seems to be no danger, and to be prepared to act at a moment's warning, when we little expect to be called upon; lest by neglect on the one hand, or by a busy but mischievous interference on the other, a life, already so nearly ebbed out, should be rudely shaken into the grave. In a scientific point of view, they lead to studies of the highest importance, both with reference to the pathology of fevers, and the general physiology and pathology of the nervous system.

Postscript.—Just as the proofs are leaving me, I have a note from Dr ——, dated 5th December 1866, which contains the following passage respecting Mr X.:—"His power of speech improves notably. He seems to have got over most of his mechanical difficulties, but has still some about the gutturals and the j. For example, wishing to tell me, the other day, that his father was waiting for me in the surgeons' room, he said,—'father (something not understood), surton rooms.' When I told him to write the last two words, he wrote 'Surgeon rooms.' The mistake of putting the s at the end of the wrong word is a fair specimen of the kind of blunders he still commits in writing."

ARTICLE II.—*On the Etiology of Stricture of the Urethra.* By HENRY VEALE, M.D., Royal Artillery.

AMONGST the several causes which have been assigned for the production of Stricture of the Urethra, it is almost universally held that gonorrhœa is the chief. This doctrine has been so long current, and has obtained such an ascendency over our minds, that it may perhaps at first be difficult to see any reason for calling it in question. Whatever may be our conclusion, however, the meaning to be attached to the term Gonorrhœa must be defined before we proceed to examine the grounds upon which the doctrine is based.

Until between 30 and 40 years ago, gonorrhœa in the male signified any puriform discharge from the urethra; but when it was discovered that syphilitic ulcers might exist in the urethra, and occasion a purulent discharge, a limitation of the term became unavoidable; and thus, what most medical men now understand by it is, a specific inflammation of the mucous membrane of the urethra, attended with a purulent discharge, caused by, and capable of reproducing a similar disease, but not giving rise to a solution of continuity in the affected membrane. Notwithstanding, however, that this limitation of the term has been accepted, many traces of its original vagueness of meaning still remain; consequently, we may observe that, although gonorrhœa is acknowledged to be a specific disease in its acute stage, the distinction is not always kept in view with respect to the chronic discharges from the urethra. A man is still often said to have a gonorrhœal discharge, or a gleet, when he suffers from an urethral flux of long standing, no matter how it may have originated, or by what causes it may be maintained. This source of ambiguity I shall endeavour to avoid, by employing the term gonorrhœa only in the sense which I have defined above.

What proof is there, then, that gonorrhœa is the principal cause of stricture of the urethra? In most of the works on the subject which I have had an opportunity of consulting, it seems to have been taken for granted that proof of the assertion was not required. Yet, no one, so far as I know, has ever traced a gonorrhœa, step by step, until it produced a stricture; and most authorities acknowledge that there is no perceptible connexion between the intensity of the inflammation in gonorrhœa, and the liability to stricture. What is most frequently asserted is, that long-standing gonorrhœas are likely to give rise to the latter condition. Now, it may be readily admitted that nearly every man affected with stricture will answer in the affirmative if asked whether he had previously suffered from gonorrhœa; but such an admission is not sufficient for the inference that gonorrhœa is a frequent cause of the subsequent ailment. Its antecedence is not shown to be unconditional. The day invariably precedes the night; but we do not, therefore, suppose the day to cause the night. Something more, then, than frequency of succession is required to prove the dependence of stricture upon gonorrhœa.

We should be often misguided if we trusted to the statements of the patients themselves on such matters, for most of them employ very loosely such terms as a "running," a "clap," etc.; and there are hardly two men to be found who will ascribe the same importance to disorders apparently exactly similar. What the one will regard as a merely trifling irritation, scarcely deserving a moment's notice, another will believe to be a sure cause of all the ills described in certain quack advertisements.

Nevertheless, there is a relation between the condition called a gleet and that of stricture; and it appears to me that this relation is one, not of cause and effect, but of conjoint effects,—either of the same, or of separate causes. The assumption that every chronic discharge succeeding an acute gonorrhœa is the true pathological sequel of this disease, would neither be warranted by theory, nor established by observation. On the contrary, I think it will be found on inquiry that nearly all such chronic discharges have some other cause for their maintenance than that which originated them. Either in the part itself, or in the constitutional condition of the patient, there will almost always be discovered something, if it be sought for, which can be recognised as the condition which perpetuates the morbid action.

However much we may reason concerning a question of this nature, we must always refer to direct observation of facts if we wish to test the soundness of our conclusions. It will be understood that I do not assert that a gonorrhœa may not act as a cause of stricture, but that it frequently produces this condition is not at all confirmed by my observation. During the last eight years, with such opportunities as fall to the lot of an army medical officer, I have searched in vain for a case in which I could safely infer that a gonorrhœa, *pur et simple*, had given rise to a permanent or organic stricture. My attention was first particularly directed to the subject in 1858, by seeing a case in which a discharge from the urethra had been produced by an indurated syphilitic ulcer at the orifice. In course of time the ulcer healed, and the discharge ceased; but an induration followed soon after, about an inch further down the canal. So long as the patient lived quietly he had no urethral discharge, but his habits were very irregular, and as often as he indulged his propensities to sexual and other excesses, the discharge recurred. In 1859 and 1860, I saw him frequently again, and he then had a very tight stricture about an inch from the orifice, and two others, of less severity, between this spot and the prostate, which, he informed me, had commenced exactly like the first. At this time he was almost never free from urethral discharge. He could never deviate from the paths of chastity without, as he thought, contracting a fresh gonorrhœa; and I must confess that at first I also was inclined to attribute the discharge to successive infections. A more attentive consideration of the circumstances, however, convinced me that this was not a case of repeated gonorrhœa, but of urethritis, predisposed to by the presence of syphilitic deposits along the canal, and excited by over-exertion or irritation of the organ; and the dependence of the strictures upon the syphilitic disorder seemed to be sufficiently clear. I have since that time seen both primary and secondary syphilitic sores at the orifice of the urethra to be followed directly by stricture in the same locality, and I not long ago witnessed a case in which an induration of the submucous tissue gradually crept from the site of an old primary sore at the orifice, to nearly an inch and a-half down the canal, causing a very decided stricture, but, curiously enough, being exactly limited to the right side. In this instance, also, a puriform discharge frequently came on after sexual intercourse, and had been mistaken for gonorrhœa.

The narration of cases is, unfortunately, seldom sufficient proof of the correctness of an opinion. We so rarely see what we do not look for, and are so apt, on the other hand, to see exactly what we desire, that a theory having been once formed, or a doctrine espoused, we seldom fail to furnish ourselves with "facts" to support it. Such being the measure usually accorded in the world, I shall abstain from giving a detail of the cases which have satisfied me that syphilis is a more frequent cause than gonorrhœa of urethral stricture. I imagine that most surgeons in venereal hospitals have seen strictures produced by both primary and secondary syphilitic lesions, and I think it is reasonable to infer that what we see to take place at one spot, may also occur at another, where the tissues are almost exactly similar. A stricture at the orifice is, however, very easily cured, which is scarcely to be wondered at when we consider the early period at which the appropriate local treatment is then had recourse to, the greater facility and certainty with which it is applied, and the fact that, for such a case, constitutional as well as local treatment would almost always be adopted. Why it is that syphilis manifests itself more in certain parts of the body than in others it is for the present impossible to explain, but we are acquainted with the general law that parts which are not the ordinary channels for the elimination of a morbid poison are liable to be affected by it with extraordinary severity, if their functional activity be so unduly exercised as to lead to an impairment of their powers of nutrition. In conformity with this law, we may consider that sexual excesses, a gonorrhœa, or any such direct violence as the bruising of the perineum upon the pommel of a saddle, in a man affected with constitutional syphilis induce the peculiar mal-nutrition which is found in certain strictures.

By analogy, also, we might be led to regard syphilis as a cause of urethral stricture, for in the cachectic state which that disease produces, it is not uncommon to find stricture of the lachrymal canaliculus, of the Eustachian tube, or even of the rectum.

From the foregoing remarks it will be seen that I not only regard syphilis to be a common cause of stricture of the urethra, but that I also believe it to be a frequent cause of persistent puriform discharge from that canal. Certain other cachectic conditions, however, have a similar tendency. These are principally the rheumatic, the gouty, the scrofulous, and the scorbutic; but, so far as my observation goes, their influence is much more decided in prolonging the urethral flux than in giving rise to stricture.

If the opinions which I have expressed be accepted as correct, their importance in a therapeutical point of view will not be denied. A gonorrhœa, if not aggravated by injudicious food, exercise, or general management, will nearly always terminate spontaneously in a few weeks, under the influence of rest alone. When, however, the discharge persists beyond this period, we may be sure that there is some definite cause, either local or general, acting as an impediment to recovery. By appropriate treatment, the duration of a gonorrhœa admits of being greatly abbreviated, as I shall show. Some years since, I treated a considerable number of cases with full doses, either of copaiba, or cubeb pepper; and although, in the majority, the result was satisfactory, in other instances these drugs proved either inert or injurious. Consequently, for the last six years, I have only employed them when I have been unable to put in practice the more certain remedies, of which the application of blisters to the groin below Poupart's ligament is one. This mode of treatment was first suggested to me by my friend Mr Park, of the Royal Artillery, to whom belongs the merit of having systematized it, and communicated the plan to his colleagues. Dr H. C. Miles has since made public the results which he has witnessed from it, and I may add my testimony that, for soldiers and others similarly circumstanced, it will be found a most speedy and effectual one. In private life, patients will rarely submit to be treated in this way, because it necessitates the recumbent posture, and would thus interfere too much with their avocations. Nevertheless, circumstances occasionally transpire when the method may be had recourse to with advantage. I have known a man, for instance, to contract a gonorrhœa a week or two before the period appointed for his marriage, and to have been so successfully treated by the blister as to be enabled to fulfil his engagement, if not with a clear conscience, at least with a clean bill of health. But the application of a blister to the groin is not without its dangers. I have seen it on more than one occasion to be followed by considerable sloughing of the integument, and I therefore never allow the blister to remain in contact with the skin for more than four hours whenever I now employ it. After its removal, a poultice is applied, and the part is afterwards dressed in the ordinary way. This practice is perfectly satisfactory, and will often cure the patient in three or four days.

Under ordinary circumstances, however, no treatment is so simple and effectual as that by injection. Two grains of sulphate of zinc to the ounce of water is one of the best injections, and it should be used from three to six times in twenty-four hours, or oftener, if the symptoms are very severe. The injections should be begun at the earliest possible period, and the recumbent posture be maintained as much as circumstances will allow. In a series of observations, I found the average duration of the cases under this treatment to be nine days. The results which I have seen from blistering the penis itself, and injecting very strong solutions of nitrate of silver, etc., are such that I should not consider it wise, under any circumstances, to repeat either practice; and I may also remark, *en passant*, that, since I have relinquished the use of cubebs and copaiba, I have never met with a case of gonorrhœal rheumatism, as it is called.

Whenever a gonorrhœa is prolonged beyond the ordinary period, the condition of the urethra ought to be carefully examined, along its whole length, both by external manipulation, and by the passage of a bougie. The slightest nodule of induration, or the least diminution of the elasticity of the canal, should receive the most attentive consideration, for it is chiefly by these indications that we can infer the tendency to the formation of stricture. Should a syphilitic diathesis co-exist, the nature of the case will be tolerably clear; but it will be a matter of difficulty oftentimes to determine the right method of treatment. Mercury and iodide of potassium, as is well known, irritate the urethra so much that, for a time at least, their internal use would aggravate the symptoms; but mercurial inunction, or the vapour bath, do not act so prejudicially, and these appear to me to be the best remedies to employ when the constitutional condition is such as to offer no impediment to their use. The efficacy of purely mechanical means for the cure of stricture has, I think, been too much insisted on during the present century. For affording immediate and temporary relief, I am willing to concede to them all that can be demanded, but I am convinced that in strictures caused by, or complicated with, a syphilitic deposit, mere dilatation is not sufficient. It will not restore the canal to its original elasticity, and we ought not to consider the cure to be complete until this has been effected.

ARTICLE III.—*On Excision of the Knee-Joint, and the employment of a New Apparatus in the After-treatment.* By PATRICK HERON WATSON, M.D., F.R.S.E., F.R.C.S.E., Surgeon to the Royal Infirmary, and to Chalmers' Hospital for Sick and Hurt.

IT is with the after-treatment of excision of the knee-joint, more than with any question as to the mode of operation, or the selection of cases of disease of this articulation suited for the operation of excision, with which this paper has to deal.

I have no intention, for I cannot think now-a-days, after the admirable writings of Sir W. Fergusson, Jones, Mackenzie, Butcher, and Price, that it would serve any good purpose to discuss the question of how far excision of the knee-joint will serve as a substitute for amputation of the thigh, how far it will bear comparison and rank with other articular resections with which in this country we have long been favourably acquainted.

I should prefer, so far as I am concerned, to accept it as a settled point, that excision of the knee-joint is a good operation, one which has, in so many different hands, yielded successful results as to render its admission into the common list of operations, not only defensible as being something more than an experiment, but justifiable as being an accepted procedure of modern surgery.

I am perfectly aware that such a position is denied to it by some surgeons of repute. Their hostility, however, may be traced to different causes, in some to their ignorance of the procedure, as at present employed; in others, to their jealousy of the success which has crowned the efforts of the chief promoters of the operation; and in others, to an unfortunate want of success in its employment, which speculatively might be said as reasonably to indicate something blame-worthy in the unsuccessful operators themselves, as in the operation which they refuse to recognise.

The procedure, as revived at the hands of such men as Sir William Fergusson and Mr Jones of Jersey, has stood the test of a sixteen years' trial, during which it has been performed by the chief surgeons of every medical school, not only in this country and in its dependencies, but upon the continent of Europe, and has gained a verdict, if not upon all the issues which its adoption involves, yet upon so many of them as to render its future progress, in civil practice, no doubtful matter. Many of those who at first opposed its employment have changed sides in this controversy; some have passed away from this, as from all else, on this side of time; and those who still continue stout in their opposition to what they regard as a dangerous innovation, have probably done as much in diffusing a knowledge of the operation by attracting the attention of others who would never have concerned themselves about the matter had it not become a subject of dispute, as any ill which may have accrued to the operation from their energetic denunciatory efforts, or their abstinence from its practical employment.

There are two reasons, it seems to me, which may encourage any one whose experience of the operation has been satisfactory, to do what he can to extend his own favourable opinion of it. The one is, that it is still practically excluded from military surgery; the other, that even in civil life the substantive objections taken to the operation apply to what may be regarded as contingent and remediable evils.

Mr Park, the great originator of this procedure, with characteristic modesty, insists in the letter in which he describes his new operation to Mr Potts, "I am conscious that the mode of operating, which I have described, is by no means perfect, but still stands in need of the finishing hand of a more able master." Several such hands as he desiderated have been engaged in the process of perfecting it for more than half a century, even excepting the period of comparative oblivion into which it was permitted to fall, and still there are some who regard the procedure as far yet from the standard of perfected art. It may not therefore seem, I hope, a too ambitious effort which I would propose to myself, if I enter the field in which the greatest men of our age in surgery have been engaged, setting before me no greater work than that of gleaning after them, and endeavouring to add a little grain of practical experience to the great harvest wherewith their labours throughout a long day have been so deservedly crowned.

Shortly after the restoration of the operation of excision of the knee-joint to the list of creditable procedures by the precept and example of Sir William Fergusson, followed by the gratifying successes which attended upon its practice by Mr Jones of Jersey, the late Dr Richard Mackenzie of Edinburgh performed excision upon four patients suffering from disease of the knee-joint. Two of these were adults, two were adolescents. All these cases came more or less immediately under my charge as his house-surgeon. Thus my attention was early directed to the subject, and I confess that, notwithstanding excellent results in the adult patients, I then formed a very unfavourable opinion of the procedure chiefly on two grounds:—1. The extreme difficulty I experienced of maintaining the limb at rest, and in good position, during convalescence; 2. The risk encountered by the formation of extensive bed-sores during the necessarily long period of confinement to bed, required by the methods of after-treatment of the limb then in use. These very manifest practical difficulties certainly gave great countenance to the opinion, then somewhat strongly expressed by the greater part of the profession, against the adoption of the operation as a substitute for amputation, and led to its all but universal rejection by military surgeons. Even one of so fair and judicial a mind as Sir George Ballingall, while he taught the operation in his surgical class, and descanted upon it in glowing terms, as one of the greatest improvements in the surgical procedures of the day, spoke of it "as an operation unsuited for military surgery in the field, where the appliances of a fixed hospital could not be depended upon."

Mr Guthrie, also, speaking favourably of excision for gunshot wounds of the knee-joint, pointedly introduces the proviso, "provided every accommodation, and, particularly, absolute rest can be obtained for the sufferer."[1] With such eminent practical military medical authorities at one upon this point, it is no wonder the influence of their opinions showed itself, in the late campaign in the Crimea, by the rarity of the performance of this operation. In that campaign, excision of the knee-joint was only once had recourse to; then, as a secondary operation, and with a fatal issue.[2] The applicability of this operation to army practice, as regarded from a purely military point of view, may further be gathered from the retrospective remarks of Deputy-Inspector-General Matthew. They are all the more interesting as coming from a man brought up under the auspices of Mr Liston, and assuredly one of the foremost among the operators of that campaign. Though decidedly hostile to its employment, one is glad to see that the ground of objection is found to exist, not in the operation itself, but in the insurmountable difficulty of the after-treatment. "Gunshot wounds," says he, "of the knee-joint are generally looked upon as demanding either amputation or excision; and, were these operations attended with less danger, it might seem advisable to have recourse to one or other in almost all cases where the bone is implicated beyond the most trifling amount, for there can be no doubt that the risk to life from such a wound treated without operative interference is very considerable. Unfortunately, however, the experience of the past war demonstrates but too clearly that amputation of the thigh after gunshot injury is a very formidable operation, terminating fatally in a large proportion of cases, while the difficulties to be encountered in disposing of a case of resection of this joint during active service, and when the removal of the patient may at any time become necessary, will probably always render the success of the latter operation in field surgery very doubtful."[2] And again,—

"Resection of the knee-joint has only once been attempted, and then as a secondary operation; and considering how vitally essential to its success is the most complete immobility of the limb after its completion, it remains doubtful if the operation will ever be practised to any extent in the field hospitals; and before proceeding to its performance the surgeon will always require to weigh well the possible circumstances in which his patient may be placed during the progress of the case."[2]

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[1] Guthrie, "Commentaries on the Surgery of the War in Portugal, Spain, France, and the Netherlands." Fifth edition, p. 94.

[2] Medical and Surgical History of the British Army during the War against Russia, vol. ii. pp. 352, 379.—Macleod, Notes on the Surgery of the War in the Crimea, p. 349.

Macleod, too, referring to the circumstances of a campaign as experienced in the Crimea, expresses himself as unfavourable to the procedure; but that opinion is arrived at only because the difficulties in conducting the after-treatment seem insuperable. "Admiring, as I do, the brave attempts which have been made in civil practice to save limbs by excising the knee, I regret that it should not also be extended to military practice; but except in rare circumstances I fear that cannot be accomplished, from the careful after-treatment, and the long period of convalescence necessary to effect a cure."[1]

Although Professor Longmore does not enter into any consideration of the question of excision in military practice, his statement in cases suited for such an operation is positive against its employment. "When the fracture is comminuted, and affects the knee or ankle-joint, opening the capsule, amputation is necessary. The knee-joint was once excised in the Crimea, but the patient died: as was the case in the only instance where this operation is known to have been performed for gunshot injury in the Schleswig-Holstein campaign."[2]

The late Dr John Brown, in his Notes on the Surgery of the Indian Campaign of 1857-58, remarks: "If there is a class of cases in military practice which demands that the knife should follow the wound, it is this class in which the knee-joint is injured by a musket ball. Occasional recoveries do not affect the question at all; for operative measures should always be had recourse to when there is no chance of recovery without them; and that chance is too remote to be considered in comparison with the greater chance of a fatal result in this form of injury. The operation of resection of the joint, while still doubtful in most cases of disease, seems to me to be applicable to some of the cases of gunshot injury of the knee, provided there be little injury to the bones, and the camp be a standing one; in marching armies the operation is decidedly inadmissible."[3]

These extracts I have made in illustration of what I have already stated,—viz., that the great practical objection in connexion with the introduction of this operation consists not in the operative procedure itself or in the fatality as compared with amputation, but in the difficulties which have been found to attend on the after-treatment of the case. The operative procedure of itself is admittedly easy of performance, requiring no greater skill than an amputation, preferable surely to an amputation in the case of a wound of the knee-joint, when the injury inflicted on the articulation is confined to the epiphysis of either bone entering into the formation of the joint, because attended with less mutilation, and certainly entailing less life risk, requiring no great amount of skilled assistance, for no bloodvessel of importance is wounded; but rejected, as we have seen, from two considerations: 1, The difficulties which are supposed to attend upon the after-treatment; and 2, The impossibility, by means of any apparatus in common use, to afford such an amount of immobility of the limb, combined with freedom from restraint, as shall permit the sufferer, if need be, readily to be removed from place to place. Similar objections taken to the effort to save the limb after gunshot or compound fracture of the thigh, which has usually sacrificed all these cases to primary amputation, has assuredly been met, in a large number of cases during the late war in America, by means of a simple method of effecting continuous extension.[1] The cases so treated have resulted in saving limbs scarcely, if at all, shorter than their fellows, and of so seemly a form and contour that nothing better could possibly be desired. If such a result, so unexpectedly favourable, has been attained in the analogous, though, I think, more serious, injury of gunshot fracture of the shaft of the thigh-bone, it seems unreasonable to doubt that in cases of excision of the knee, were an apparatus designed for the after-treatment which, while simple and readily obtained, admitted of the easy removal and application of dressings, and permitted the movement of the patient's body and limb, without any disarrangement of the original adjustment, a like favourable result would, even in the difficulties of a campaign, or on shipboard, be found to attend upon the employment of this resection.

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[1] Notes on the Surgery of the War in the Crimea, with Remarks on the Treatment of Gunshot Wounds. By G. H. B. Macleod, M.D., F.R.C.S. 1858. P. 354.

[2] Holmes' System of Surgery, vol. ii. p. 84.

[3] Notes on the Surgery of the Indian Campaign of 1857-58. By John Brown, M.D., Assistant-Surgeon Bengal Medical Service. Edinburgh Medical Journal, Sept. and Oct. 1860.

The methods adopted for maintaining repose of the limb after excision of the knee-joint, since its earliest employment, have been various. First and foremost, there is the "*case of tin* sufficiently long to receive the whole limb, from the ankle to the insertion of the glutæus medius muscle," employed by Park on his famous patient, Hector M'Caghen. Since that time (1781), various devices have been fallen upon to keep the parts at rest, varying in the degree of complexity of their construction.

The apparatus employed by the older Moreau, he describes as follows: "This machine was composed of a board, the length of the diseased limb. It was bevelled at the upper end, that it might not hurt the thigh; and scooped out before, that it might receive the heel. It terminated in a sole; and at the side it had ledges of wood, which slipped easily into grooves; and as those ledges rose higher than the dressings, there was no need of a basket to bear up the bed-clothes. I likewise prepared cushions of baked hair, one of which I put between the limb and the underboard, and the other two I placed, one on each side, between the limb and the side boards. The foot rested against the sole, to which it was fixed by a tape."[1]

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[1] Gross, System of Surgery, vol. i., pp. 1034, 1035; Redfern Davies, Esq., Lancet, 6th June 1863.

The splint recommended by Mr Butcher of Dublin, and with the aid of which he has produced such admirable results in resection of the knee, closely resembles the splint of Moreau. "A wooden case which I had made for the purpose. The sides were attached to the back part by hinges, so as to allow of being let down at the time of dressing; they were likewise of unequal length —the internal extending nearly as high as the ramus of the pubis, while the external passed up to the axilla, similar to the long splint used by me in fractures of the thigh; the lower end of each lateral piece presented on the inner surface a number of grooves, about an inch apart, so that when the sides were elevated, the foot-board was received into any opposite pair of them, according to the distance required. This lower piece acted in two ways; not only did it maintain the foot at a right angle with the leg, but it steadied the sides and prevented their being pressed inwards from their vertical direction, by the tapes and buckles which girded the apparatus on the outside. The box was supplied with hair cushions carefully adapted to its entire extent, some being covered with oil silk. In addition to the posterior, lateral, and foot support, a broad splint well padded had to be placed over the anterior surface of the thigh, extending from a little below Poupart's ligament, as far as the junction of the upper and middle thirds of the leg, and secured firmly down by the surrounding web belts, so as to counteract the powerful tendency towards the distortion of the limb forwards. The external side of the case passed up to the axilla, the object being to insure the straight position for the limb. It was kept in contact with the trunk by a wide girth passed around both. . . . . The bed was prepared in the following way: A hair mattress was laid over a feather-bed, raised towards the feet; a blanket folded several times was placed over the part on which the shoulders and hips rested; thus the returning circulation was favoured, and the chances of excoriation of the prominent parts of the buttocks guarded against."[2]

The late Mr R. C. Price, in a paper communicated to the Lancet,[3] recommended the employment of a splint or apparatus of his invention, which he describes as follows: "The apparatus is an improvement on the ordinary M'Intyre splint. It consists of two concave portions, the upper part corresponding to the lower half of the posterior surface of the thigh, and the other to the entire length of the leg. These two portions are connected by a narrow plate of the same substance as the splint, which is of enamelled tinned iron, and when in position corresponds to the popliteal space. The lower end is provided with a wooden foot-board, which can be regulated by means of a screw and slide to suit the length of the limb. The plate connecting the upper and lower portions, together with the part corresponding to the lower third of the leg, can likewise be regulated according to convenience. When the splint is in position, sufficient support is given to the popliteal space by this narrow plate, and at the same time no obstruction is offered to the daily dressing of the sides of the wound. By the sliding arrangement of the portion of the splint corresponding to the lower third of the leg, a space is left between it and the foot-board, so that the heel, which is very liable to become affected by the slightest pressure when the patient has been confined to bed for a length of time, is left perfectly free. I have had on more than one occasion to regret the formation of a sore over the insertion of the tendo Achillis, which has acted injuriously as a troublesome source of irritation. The outer side of the splint is provided with hooks, whereby a long side splint of wood furnished with corresponding eyes, may be fastened. This splint ought to extend about two or three inches below the foot-board, and to reach a little higher than the crest of the ilium. I have had the two portions connected by a strong round iron hoop, which, if made sufficiently high, enables the wound to be dressed without the inconvenience of disturbing, in any way, the apparatus. The hoop, or arch, also answers the purpose of a cradle in keeping off the bed-clothes when the limb is not placed in a swing. All the advantages of the long splint are thus gained, without the inconvenience of daily removal, and the liability of injurious pressure being made on the outer margin of the wound, in instances where there is great swelling of the soft tissues. The use of the side splint is the same as in fractures of the femur. By it, in conjunction with the perineal band, extension of the thigh is regulated, and the entire limb kept perfectly at rest."

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[1] Jeffrey's Cases of Excision of Carious Joints. By H. Park and P. F. Moreau. P. 133.

[2] Essays and Reports on Operative and Conservative Surgery. By Richard G. Butcher. Dublin, 1865.

[3] Lancet, 24th January 1857.

Dr Frank Hastings Hamilton, in the latest edition of his work upon Military Surgery,[1] recommends "the limb to be laid in a long, well-fitted, and well-padded box or splint. The box may be made of zinc or tin, and supplied with floating sides opposite the knee, to enable the surgeon to dress the wound from time to time without disturbing the limb."

Thus, a simple posterior padded splint, the same with lateral splints, the old-fashioned fracture-box, sand or bran bags, the double-inclined plane or M'Intyre splint—simple or combined with one or two side splints, with requisite fenestræ—have been recommended by different operators, and have chiefly constituted, in more or less complicated combination, the methods of after-treatment hitherto employed by those whose efforts have been directed to obtaining firm anchylosis. By Langenbeck the immovable plaster-of-Paris apparatus has been recommended, and I am informed, constantly employed in the early period of after-treatment; in the later, passive movement of the limb is commenced, with the view of obtaining a movable articulation in imitation of the natural joint.

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[1] A Treatise on Military Surgery and Hygiene. By Frank Hastings Hamilton, M.D. New York. Pp. 516-17.

In the cases of excision practised by Dr R. Mackenzie, and in two of the cases under my own care, I resorted to the use of several of these devices, but found them inconvenient and irksome in the last degree to both patient and surgeon. In all of them I found, that while the leg and foot were certainly fixed with security, and frequently immovably, the thigh followed the movements of the body, and, consequently, a greater or less degree of displacement resulted in proportion to the restlessness of the patient. The displacements of the thigh I found, as described by every operator, to be of two kinds, rotation in a direction outwards, and abduction with a slight degree of projection forwards. These displacements, I furthermore found, could not be overcome when they had once occurred, without giving great pain, and without the complete re-application of the apparatus.[1]

It was in such circumstances that, about two years ago, I devised the following apparatus, which I have employed in every instance since that time in which I have performed the operation of excision of the knee. Since my introduction of it to the attention of my colleagues in the Infirmary, it has, I am glad to say, met with their sanction and practical approval, having been most successfully employed by Professor Spence and Dr Gillespie in several cases. I have also understood, from the communications of friends and former pupils, that it has received even already a wide reputation, not only at home, but even abroad, commending itself to the attention of practical surgeons chiefly on account of its simplicity, comfort, and ease of application. It consists essentially of two parts—1. A suspension-rod made of iron, about the size of No. V. of trade wire gauge; 2. A modelled Gooch splint, long enough to extend from the tuberosity of the ischium to beyond the heel.

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[1] *July* 5*th*.—"Had not passed so good a night, complaining much of pain in the back from posture."

10*th*.—"His bed becoming very uncomfortable, he was removed into a fresh one."

12*th*.—"Spasms in the thigh."

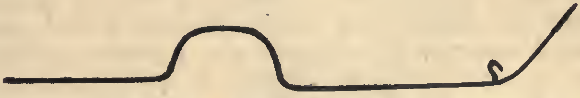
14*th*.—"Spasms still troublesome; had gotten the limb into a bad position, in consequence of turning too much on the side—the posture in which he has been accustomed to sleep for several years, but at present a very unfavourable one, as by this means the end of the femur was raised too high, and cast outwards: *this was rectified, though not without some pain and trouble. It may be sufficient to mention here, once for all, that this circumstance gave me a good deal of trouble at different times during his whole confinement.*"

March 23*d*.—"The knee very slightly bowed outwards, owing to the difficulty of keeping him in a good position, which I have already mentioned."—Mr Park's case of Hector M'Caghen, pp. 27, 28, 36.

"In order to prevent displacement of the bones, which all our efforts had been insufficient to effect completely, I cut away about two inches of the femur with the pliers, and then observed with much concern, that the bone was denuded beyond the farthest extent to which my finger could reach."—Syme on Excision of Diseased Joints, 1831, p. 139.

The suspension-rod extends from the groin to the extremities of the toes, and is bent to the outline of the limb, departing from it only in the situation of the excision, where it forms a bow or arch. To the upper surface of the rod are attached one or more hooks by which the suspension is effected.

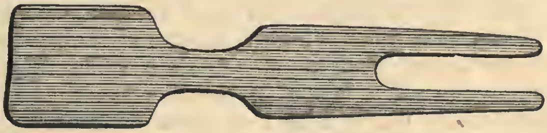
FIG. 1.



Suspension-rod for front of limb—the arch corresponds to the site of excision, the suspension-hook to the ankle-joint, upper end should terminate at the fold of the groin.

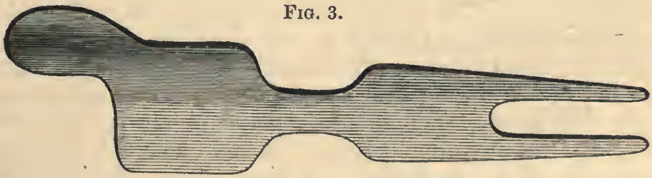
The Gooch splint should not be made too wide, and should certainly not surround the thigh and leg to more than two-thirds of their circumference. It should be scooped away laterally, at a part corresponding to the site of the excision, and should have an aperture cut corresponding to the tendo Achillis and heel. The inferior extremity of the splint is thus of a horse-shoe or stirrup shape, and admits of the ankle and foot being supported by the lateral horns of the splint, as they fold on each side of the malleoli, without the risk of inordinate pressure being made upon the tuberosity of the os calcis.

FIG. 2.



Posterior "Gooch" splint of ordinary form—hollowed out in each side on the level of the operation wound, and cut out at the lower part in a horse-shoe or stirrup form, so as to relieve the heel and tendo Achillis from any pressure.

FIG. 3.



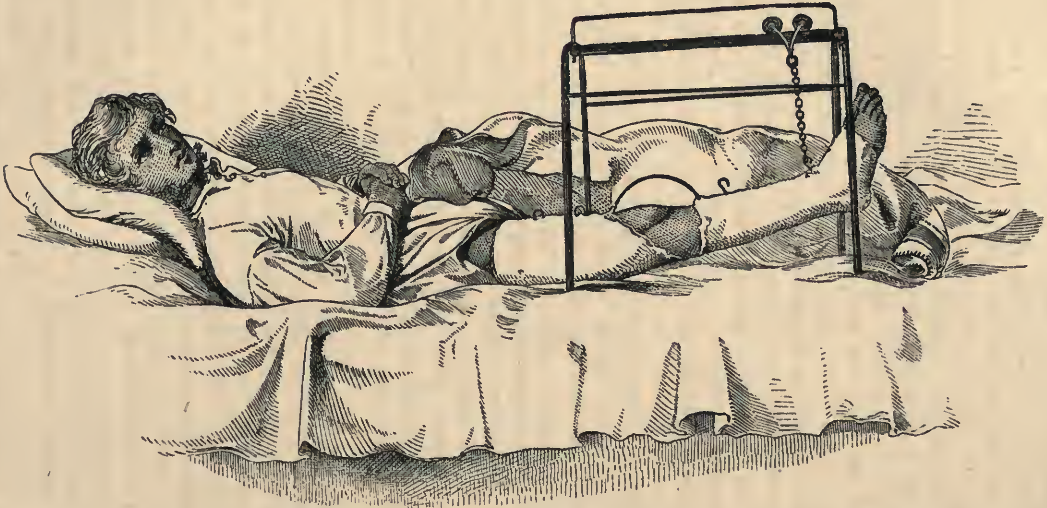
Alternative form of "Gooch" splint for excision, hollowed out as in Fig. 2.; the rounded part above, adapted to the os innominatum, and secured by means of strips of adhesive plaster.

In application, the limb is first laid and carefully adjusted upon the posterior splint, which should preliminarily be padded with lint, and covered with gutta-percha tissue, or hot paraffine, in the situation which corresponds to the site of operation. The iron rod is then placed in front, and folded lint laid between it and the limb.

at the groin (where the rod terminates above), at the upper part of the tibia, and at the bend of the ankle. These two parts of the apparatus are then retained in contact with the limb by means of an *open wove* roller bandage applied from the toes upwards, the site of the incision alone being left uncovered. The whole is then rendered immovable by means either of plaster-of-Paris applied by the hand, of a consistence like thick cream, or of paraffine, which, having been rendered temporarily liquid by being heated, is applied by a large painter's brush. When the application has solidified, the patient may then be removed to bed, and the limb suspended from the running pulley of a Salter's swinging cradle, or from the roof-bar of the common iron-wire cradle employed to support the weight of the bed-clothes. I do not regard it as a matter of indifference whether plaster-of-Paris or paraffine is used. Each has its advantages. The plaster-of-Paris is firmer and not liable to be affected by heat like paraffine, but it has the disadvantage of permitting soakage of discharge; it takes longer to consolidate, and when consolidated, is less easily clipped through by means of bandage-shears than the paraffine apparatus. I have of late, in the treatment of excisions and of compound fractures, employed the plaster-of-Paris as the substantial substratum, and thereafter applied the paraffine over it as soon as consolidation and drying has become complete, so as to secure its greater toughness, and its complete impermeability to fluids of any kind, whether blood, serum, or pus, from the line of incision, or water from the wet dressings which may be applied during the healing of the wound. The foot and limb should not be much raised above the level of the mattress upon which the patient lies, as this is apt to lead to an awkward position of the limb when the patient first begins to move about. When in bed, the sacrum and hips must be protected from all risk of inordinate pressure, by means of a large square corrugated air-cushion completely covered with a blanket and draw-sheet. This will be found to be most evenly and comfortably supported upon a firm hair-mattress, laid either on a thick hair-paillasse, or better, a spring-bed. The patient should be encouraged to sit up as much in bed as possible, even within a day or two of the operation, and as soon as it is possible should be removed out of bed during the day, either to another bed or couch, or should such not be attainable, to a mattress laid upon the floor. In sitting up, the air-pillow placed beneath upon the seat will be found a great comfort, as it admits of easy relief to the irksomeness of maintaining nearly the same sitting posture for a period of several hours.

The chief merits which I have observed in this method of after-treatment are:—1. The comparative comfort the patient experiences from the ease with which he can shift his lying posture or assume the sitting position without disturbing the adjustment; 2. The facility with which dressings are applied without detaching or removing any portion of the apparatus; 3. The permanence of the

FIG. 4.



Shows apparatus applied to limb, and slung by the ankle-hook of suspension-rod from a Salter's swinging cradle. Two or three other hooks for suspension are introduced; although sometimes useful, as affording a sense of change of position to the limb, they are not material to the apparatus.

apparatus, never requiring, when properly applied in the first instance, to be renewed during the whole period of after-treatment; 4. The ease with which the apparatus can be obtained, its simplicity and cheapness.

A few words in explanation of these advantages may not seem inopportune.

1. *The comparative comfort the patient experiences from the ease with which he can shift his decubitus, or assume the sitting posture, without disturbing the adjustment.*

From what I have premised, it is manifest that the plans of treatment previously adopted imply either a persistent recumbency, the limb being steadily fixed to an immovable apparatus, which, lying upon the bed, admits of no adjustment corresponding to any accidental or intentional movement of the patient's body, or the employment of a lighter apparatus less calculated to maintain accurate apposition, but which, being adapted to the sling of a Salter's apparatus, admits of some degree of movement on the part of the patient.

Between the two plans, certainly the latter is in many respects to be preferred. It has the clear gain of admitting of some degree of motion of the trunk, and is therefore less irksome and less likely to be accompanied with the formation of bed-sores.

But this alleviation of the uneasiness attending upon the continued maintenance of one posture is somewhat shorn of the benefits it undoubtedly confers, by the apparatus, partly from its weight, partly from its exerting a greater purchase on the leg and foot than upon the thigh, admitting of some degree of displacement in the direction of rotation outwards, abduction and advancement of the thigh-bone.

The fixed apparatus treats the case as one of fractured thigh-bone, combining in its construction the old fracture-box with the long splint of Desault. Here the limb is steadily fixed to the bed, and the leg is therefore, to all intents and purposes, immovable as the bed itself; while the thigh, subject to constant displacement, shifts the accuracy of adjustment with every effort on the part of the patient to raise himself in bed, or to shift the position of his buttocks. But as any such movement must be at best limited, the chief risk to be encountered with this apparatus is the formation of bed-sores on the sacral region. Another serious objection to this method of treatment has, however, been found to obtain in practice, viz., that in spite of every contrivance to prevent the displacement of the lower end of the femur at the site of operation, or to reduce it to a minimum, some degree of displacement will and must occur. For example, a patient cannot long lie in the recumbent posture without some degree of sinking of the buttocks into a hollow in the bed produced by the intrinsic weight of the part; this, if the thigh is free, is followed by a commensurate tilting of the femur in the direction of flexion, abduction, and rotation outwards. If the thigh is fixed, this of course cannot take place to the same degree, but must occur in some measure. Lest this objection should be considered a merely hypothetical one, I may mention, in addition to the undoubted fact, that such displacements are observed in nearly every case where the operation has been performed which I have had an opportunity of examining—explain them as we may,—this sinking of the pelvis is recognised by Malgaigne as an important "mechanism" in the production of the well-marked displacement always encountered in cases of sub-trochanteric fractures of the thigh-bone, especially when treated by the long splint,—"Plus tard encore, j'observai que le bassin du malade faisait un creux dans le matelas et attirait ainsi en dedans la tête fémorale, laissant en dehors l'autre bout du fragment supérieur;"[1]—and is overcome by him by the following device:—"Je fais coucher le malade sur un lit plat, une large planche passée sous le matelas."—(P. 719.) A plan scarcely calculated to tempt one to imitate it, risking, as it must in a great degree, the occurrence of bed-sores.

It appears to me, therefore, that the great objects to be desiderated in any apparatus for the after-treatment of excision of the knee-joint are,—1. Commensurate adjustment of the sawn ends of the bones not only at first, or at each dressing, but constantly throughout the whole period of after-treatment. 2. Such equipoise of the whole limb, that the thigh-bone shall be able to follow the inevitable movements of the trunk without losing its proper relations to the tibia.

The light nature of the new apparatus, its equal hold upon the thigh and leg, and its suspension from a single point providing for all these requirements, seems theoretically, therefore, well suited to obtain a satisfactory result. The amount of movement on the part of the patient permitted by this apparatus is infinitely greater and more varied than any other. It permits every possible degree of flexion and abduction or adductive movement of the thigh, so that the patient can incline the hips from side to side without the slighest disarrangement of the apparatus or of the adjustment. By a slight modification of the mechanism by which suspension is effected, the patient can even turn completely upon the sound side.[2]

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[1] Malgaigne, Traité des Fractures et des Luxations, tome i. p. 716.

[2] I may here mention that in one of Dr Mackenzie's cases, that of a young lad who sank from exhaustion, after excision of the knee-joint, the difficulty created by the occurrence of bed-sores in spite of the use of every preventive and palliative measure which could be devised, I was obliged to have a large bed-table constructed half the height of the greatest diameter of the ossa innominata, upon which the limb and its splints might be laid, while the patient was turned upon the sound side. This plan, although it relieved the patient's back, and afforded him for days a respite from the dorsal decubitus, was so exceedingly troublesome of adjustment to any variation in the lateral posture of the trunk, that I was obliged to lay it aside and have the limb and its apparatus buoyed upon a large thick air-cushion obtained for the purpose, which, although more comfortable, was not satisfactory in maintaining quiescence of the femur and tibia.

To effect this, a piece of tape or bandage, or a circular wire hoop, if it can be obtained, is passed round the apparatus on the level of the lower part of the calf of the leg, and to this, instead of the hook in front, the chain of the swinging cradle is attached. If the tape tends to slide over the surface of the plaster-of-Paris case, this can be prevented by the application of some fresh plaster, by which the tape may be at once fixed to the plaster case at any level upon the opposite side. I have by this device enabled patients, within a few days after the operation, to lie upon the sound side, and thus pass a more comfortable night than on their back. Should they during sleep turn upon the back, the tape sliding within the hook of the chain, or the whole case turning within the wire hoop, permits the limb to follow the movement of the body without awaking the patient, or requiring the aid of a nurse or other extraneous assistance to help him to modify his position.

The patient can also sit up in bed at an early period after the operation. In the second last case of excision of the knee which I performed, on the day following the operation I found, on entering the ward, the patient sitting up in bed combing her hair, and so turned round as to permit her long back hair to fall over the edge of the bed, while the foot of the sound limb was supported upon a seat on the other side of the bed. On inquiry, I found that she had assumed this posture of her own accord without assistance, and that, at even so early a period, neither pain or displacement followed upon so considerable an amount of freedom of motion. I find that the lightness of the apparatus admits of the patient being easily moved out of bed within a few days after the operation. This I regard as no unimportant matter in securing a favourable result, and in avoiding the risks of pyæmia. It is imposssible, so long as a patient lies in one bed, to have that bed made, aired, and properly cleansed, as is essential after an operation, if the health of the patient is to be maintained; even with draw-sheets, air-pillows, and the most sedulous use of all that painstaking care which really good nursing can secure, the patient's health must suffer so long as he is obliged to maintain the recumbent posture day after day in the same bed. I think it, therefore, as no inconsiderable element in the excellence of this apparatus that it affords a wonderful facility for removing the patient from bed to bed, and out of bed to the couch or chair.

Furthermore, I am confident from my experience of the employment of this plan of after-treatment, that it is admirably adapted to the exigencies of a campaign, and that it is likely to prove advantageous to the military surgeon, by enabling him on active service to undertake the operation of excision of the knee-joint in the midst of all the uncertainties of a regimental camp hospital, or even when on the line of march. In other words, the military surgeon may, with it, safely excise the joint where he knows that he cannot have the advantages of a fixed and permanent hospital for his after-treatment of his patient. I must confess that I have had no practical personal experience of this procedure under such circumstances, but having had ample experience of the exigencies and difficulties which do arise and which must be met in the treatment of surgical cases during a campaign; and, furthermore, having made experience of what is to be obtained from this plan of after-treatment, I can say with every confidence, that were I myself engaged in the surgical treatment of patients during another campaign, I should never hesitate to employ excision in preference to amputation in cases of injury of the knee which have hitherto been supposed to require amputation, or from which, at all events, excision has been withheld, not because the operation was deemed improper or insufficient, but because the means necessary for after-treatment were not to be obtained.

It was one of the great benefits Park anticipated as likely to accrue from his successful introduction of this excision into practice, that it should become, in army practice, a means of saving life and limb. Accordingly, writing in 1805, twenty-three years after the publication of his letter to Mr Percival Pott, in which he details with just satisfaction his well-merited success, he sorrowfully remarks,—"I have not yet learned that it has hitherto proved the means of saving a single limb in the whole of the British dominions; and since its publication, this country has passed through a long and bloody war, without this practice being at all adopted, or scarcely known either in the army or navy of Britain." "However, as we are now but just entering upon the serious part of another war, I hope it is yet in time to do infinite good by preserving a great number of limbs, if the knowledge of it can be generally diffused through the army and navy."[1] "When I speak of diffusing the knowledge of this subject through the army and navy, I feel myself justified in using this language, by having been assured by gentlemen of the profession, who had seen much service during the late war, that the only idea then entertained respecting wounds of the large articulations, was, that such limbs were to be considered as fit objects for amputation."

Were Mr Park alive at the present day, I fear he would still have much reason to adhere to the same language and still to express the, as yet, unfulfilled wish, that his new operation might prove the means of benefit to his fellow-creatures. He was himself apparently sufficiently aware wherein lay the difficulty, for he remarks further on in the same sagacious letter,[1]—"To the solicitude I have expressed to introduce this practice into the army and navy, it has been objected, that the accommodation in these situations are not always such as to admit of such attempts; and that the hurry of an action may often oblige a surgeon to have recourse to the most expeditious method of saving his patient. These objections I admit to a certain degree. I allow that the excision of the knee may be wholly inadmissible in ships, as the necessary state of quietude can hardly be obtained there for a sufficient length of time to accomplish a cure; and, perhaps, similar difficulties may occur in military hospitals belonging to an army in action." [1]

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[1] Subsequent observations by Mr Park, appended to the former work, in Jeffrey on Excision of Joints, pp. 60, 61, 63, 64.

[1] "The following example we believe to be the only successful case of excision of the knee-joint made for gunshot injuries ever yet placed upon record. The operation was made by R. B. Bontecue, surgeon, U.S.N.; and for this brief history we are indebted to Dr J. W. Lyon, House-Surgeon to the Belleview Hospital, who is engaged in preparing an elaborate paper on Exsections.—A man, aged 20, was wounded in the right knee, 22d October 1862. The ball passed through a portion of the external condyle of the femur and lodged in the popliteal space. On the second day after the injury, Dr Bontecue removed one and a-half inch of the lower end of the femur with a saw, and with a pair of bone-forceps cut away the exposed surface of the tibia until the bone was exposed. The patella was dissected out. A portion of the wound healed by the first intention; on the twenty-eighth day the wound had entirely closed; and at the end of little more than two months from the date of the operation the patient was discharged. There was then 'no pain or tenderness about the joint.' "—Hamilton's Military Surgery, p. 514.

Table of Cases of Excision of Knee practised on Account of Gunshot Injury of the Articulation.

		Deaths.	Recoveries.	
[1] Textor, senior,	1847	1	...	
[2] Fahle — Schleswig-Holstein,	1851	1	...	
[3] Lakin—Crimea,	1855	1	...	
[4] Brown—Alumbaugh,	1857	1	...	
[5] London Hospital—Hutchinson, small shot,	1861	1	...	
[6] Birmingham case,	1861	...	1	
[7] Verneuil's,	1863	...	1	
[8] Bontecue,	1862	...	1	"Trough stuffed with hay."
[8] Heller,	1863	1	...	
[8] Hinkle,	1863	1	...	
[8] Bently,	1863	1	...	
[8] Bently,	1863	1	...	
[8] Homans,	1864	1	...	Fracture box.
[8] Unknown,	1864	1	...	Fracture box.
[8] Bontecue,	1864	1	...	Bandage of Scultetus and bran bags.
[8] Rush,	1865	1	...	
[8] Fairfax Seminary Hospital case,	1862	1	...	
[8] Thorne,	1863	...	1	
	18 cases	14	4	

[1] Fuchs' Dissertation, 1854; and O. Heyfelder's Traité des Résections, par E. Boeckel. Paris, 1863, p. 106.

[2] Friedrich Esmarch, Die Resectionen nach Schusswunden. Kiel, 1851. Stathams Esmarch, p. 113.

[3] Medical and Surgical History of the British Army during the War against Russia, vol. ii. pp. 352 and 379.—Macleod, Notes on the Surgery of the War in the Crimea, p. 349.

[4] Brown, Edinburgh Medical and Surgical Journal, October 1860.

[5] The Lancet, April 20, 1861.

[6] Medical Times and Gazette, May 1861.

[7] Gazette Hebdomadaire, November 1862; Soc. de Chirurg. de Paris, Séance du 10 Juin 1863.—Ligoues, Traité de Chirurg. d'Armée, p. 750.

[8] Reports on the Extent and Nature of the Materials available for the Preparation of a Medical and Surgical History of the Rebellion. Circular No. 6, War Department, Surgeon-General's Office, Washington, November 1, 1865, pp. 58, 59, 60.

2. *I have next to consider the special advantage which this method of treatment affords for the application of dressings to the site of operation.*

This is no matter of secondary importance either to patient or surgeon. Pain, which can scarcely fail to be excited by the removal of any portion of the apparatus essential to the quiescence of the limbs, is thus prevented, while the complete exposure of the line of incision throughout its entire extent admits of a thorough drainage being effected so far as discharge of any kind is concerned, and while the free circulation of air around the area of the operation keeps the parts cooler and more comfortable than they can be when shut in by splints or a box and cushions. In operating, I have hitherto always adopted the semilunar incision, by which the soft parts over the knee, including the patella, are raised in one flap.[1] I have deviated from this plan only in one case of compound fracture of the patella, where the double elliptical incision, originally recommended by Mr Syme,[2] was adopted as a means of including the bruised and lacerated integument together with the patella, and corresponding to the site of injury. In the semilunar incision the perpendicular length of the flap raised from the front of the knee is about five inches, and the open space left between the bandages upon the leg and thigh amounts to fully six inches. In this space, the whole extent of the line of incision is exposed, and the extreme angles, which correspond to the level of the upper end of the distal epiphysis of the femur, are both the most dependent part of the whole wound, and are at the same time free from all confining dressings. I have accordingly found little trouble with bagging of matter within the excision wound, little occasion for making counter-openings to allow of the escape of pus, and since I employed the paraffine as a varnish to the padding of the splint and to the gypsum, no annoyance from either soiling with discharge, or in warm summer weather the formation of maggots beneath the margins of the gypsum bandage. The discharge as a rule has not proved copious; and in some cases I have scarcely ever applied the smallest portion of any dressing to the line of incision throughout the whole progress of the case. In one example, in fact, I employed no dressing whatever, permitting the serosity which escaped immediately after the operation to form an incrustation which remained untouched until it fell away spontaneously.[3] This case, already published in a paper on the employment of acupressure in the arrest of arterial hemorrhage in recent wounds, has, I am aware, been somewhat discourteously sneered at as a falsehood by a reviewer of Sir James Simpson's work on Acupressure, who seems to possess a peculiar system of reasoning which convinces him that what *he* has not seen can not exist. I conceive that the best dressing which can possibly be applied is well-carded tow which has the power of absorbing whatever discharge flows from the wound without plugging up the orifice of escape, which constitutes a serious objection to the employment of either cotton, lint, or charpie.[1] It is very rarely necessary to remove the metallic sutures which have been introduced immediately after the operation till the union of the soft parts is complete. These, if not too numerous, and if no great tension from oozing, inflammatory action, or the advent of phagedænism occurs, will usually be found to retain their position, maintaining satisfactory apposition of the edges of the wound, without exciting any irritation. It is usually well to leave the dependent angles of the wound for fully an inch without any sutures, that a free drain for blood and fibrinous serosity may be afforded in the early period of the case, when the confinement of either in a decomposing state would determine more extensive after-suppuration than would otherwise in all probability have occurred. Immediately after the operation, however, after securing the actually bleeding vessels, and applying the immovable gypsum apparatus, without introducing any sutures, I am in the habit of applying a common roller bandage firmly around the knee, so as to afford a temporary support to the flap, and thus prevent as far as possible the distention of the cavity with blood clot. To do this satisfactorily, openings in the bandage must be left or made at two points corresponding to the angles of the incision. Were this not attended to, the bandage, while by its pressure it restrained the oozing in some measure, would only the more certainly confine the blood which oozed out after the closure of the wound. This bandage is, however, merely a temporary one, to be removed in about six or eight hours after the operation, when the sutures should be introduced. Should acupressure have been employed in the operation, the needles may safely be withdrawn at the same time.

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[1] This long anterior flap incision, originally recommended by the late Dr Richard Mackenzie, and employed by him in three out of the four cases on which he operated, is a modification of the incision proposed by Mr Park in his brochure on this subject, where, in the postscript of his letter to Mr Pott, he says, "*Query*, May not the end be in some cases obtained by means of a single incision, made transversely half round the joint, so as to divide the lateral ligaments?"—P. 45.

[2] Syme on Excision of Diseased Joints, p. 133.

[3] Edinburgh Medical Journal, July 1865.

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[1] I am glad to find this view of the usefulness of tow as a material for dressing, long since recognised in the Edinburgh Infirmary, confirmed by the experience of Mr Redfern Davies in the late American campaign. "One of the objects in applying lint to a suppurating wound is that it may absorb the discharges. Lint is composed of cotton. If a bale of cotton is immersed in a river for a month, the cotton in the centre of the bale will be found to be upon examination perfectly dry. If the same quantity of picked oakum be similarly treated, it will be found in a few hours to be completely saturated. So, when a pad of lint over a suppurating wound is removed, after having been upon it for many hours, it will be found to be—save at its points of immediate contact—quite dry, and the pus beneath will gush out. Thus the pad of lint acts as a tampon, preventing the free escape of pus, which necessarily burrows in different directions, giving rise to extensive and dangerous abscesses. Now, when *picked oakum* is placed over such a wound, upon its removal the wound will be found dry and clean, the oakum saturated with pus. The picked oakum acts like a syphon, discharging the pus as it is secreted by capillary attraction, and, partly from its fibres being tarred, is a very excellent antiseptic, and removes all unpleasant odour."—*Lancet*, June 6, 1863.

3. *The Permanence of the Apparatus.*—As a rule, I never have required to shift the apparatus, from the moment of its first application, till it is finally removed,—consolidation of both soft and hard parts having advanced so far as to render this safe. In my earlier cases, I have done so if the discharge undergoing decomposition has rendered the splint or margins of the gypsum bandage in any degree offensive. Latterly, the paraffine, as a waterproof varnish for both splint and gypsum bandage, has rendered even this unnecessary.

In this respect, the apparatus contrasts strongly with the other measures recommended for effecting retention during the after-treatment, and tends, therefore, to render the progress of the patient a much more agreeable one, both for himself and his surgeon.

I can quite conceive that the permanence of the apparatus may, in certain circumstances, form a theoretical objection to its employment. One is so ready to think that a permanent apparatus which does not yield, which covers up the greater part of the limb concealing it from observation, must be liable to impede the circulation of the extremity, to cause constriction or undue pressure at some part, to interfere with swelling, in fact, to preclude that constant and close attention to every part of the limb during the after-treatment which, by some, is considered an essential of any good apparatus. To those who have familiarized themselves with the employment of the starch, dextrin, gypsum, or paraffine apparatus, in the treatment of fractures, and especially of compound fractures, this objection is not likely to have much weight; and, when we recollect how safely the patient's sensations of ease, or the contrary, may be taken as a true exponent of the progress of matters within the hard case, the objection to the use of such an apparatus in the treatment of excision of the knee will at once be removed. Should we wish to remove the apparatus, either partially or completely, at any period of the after-treatment, this is easily effected by shears for the purpose. The powerful curved instruments, with short blades, of unequal length, made by Lutter, of Berlin, seem to me preferable to those (*Suetin's scissors*) commonly supplied by instrument makers in this country, which, by the length of blade, in proportion to the handles, lose power and accuracy in their application.

4. *The Ease with which the Apparatus can be obtained, its Simplicity, and Cheapness.*—An iron rod, easily procured from any smith, a long Gooch splint, fashioned for the purpose by the carpenter, or made of two common Gooch splints, of a size suited for the thigh or leg, stitched together, and then cut to suit the limb in length and contour, two *open wove* bandages, a pound of gypsum, and a couple of ounces of paraffine, are all the requirements. A few points, even in regard to such simple matters, may as well be mentioned. The measurements for the iron rod should be made by the surgeon himself, and in

making them, he should commence from below upwards, measuring in detail with a piece of common tape. The measurement from the tip of the toes, to the flexure of the ankle, should first be made for the foot piece; then, from there, to an inch below the tuberosity of the tibia, for the leg piece; then, five or six inches should be allowed for the diameter of the portion of a circle which arches over the knee; and, last of all, the measurement for the length of the thigh portion should be made from two inches above the patella, to the fold of the groin. This allows for the ordinary amount of bone removed in the excision, which usually varies from one and a-half inch to two inches. If these measurements are not carefully taken, the surgeon may find to his annoyance a rod quite unsuited to the limb presented to him for application after the completion of the operation, which will necessarily delay the final adjustment till a properly constructed rod has been procured.

The Gooch splint should not be made too broad, nor of too *thin* wood. The amount and extent of the cutting out on each side of the splint is of less moment. If not sufficient, it can easily be rectified by means of a pocket-knife at the time of the operation. The opening below for the heel and tendo Achillis should, however, be seen to, that it may be at least three-quarters of an inch wider than the transverse width of the tuberosity of the os calcis. If it is not so, it is apt to press unpleasantly, even when well padded, against the malleoli. Should this have been overlooked, and occasion annoyance after the apparatus has been applied, the pressure can easily be relieved at any time by inserting a portion of wood beneath the heel, between the two lateral extremities of the splint, where they project beyond the sole of the foot. This piece of wood, to be of use, must be longer than the greatest breadth of the malleoli.

It has been oftener than once suggested to me that, with the gypsum bandage, the Gooch splint, or, in fact, any posterior splint, is an uncalled-for addition and complication of the apparatus. I think this is a mistake, for, after the operation, when the parts are very movable and easy of displacement, even when the patient is fully chloroformed, it affords a great additional security that the apposition of the cut osseous surfaces shall be maintained till the gypsum has consolidated, that we can adjust the limb upon a splint. Thus it is rendered more firm than it could possibly be with the rod alone, not only during the application of the bandages, and the application and setting of the cream of gypsum, but throughout the whole period the apparatus is retained. Furthermore, the splint behind serves to diffuse the pressure of the bandage, and to prevent its girding the limb, and interfering with its circulation. The open wove bandages are preferable to the common cotton or linen bandages, because their open structure fits them better for readily absorbing the gypsum, and forming along with it a tough and hard case, softly padded on its inner surface.

The gypsum may sometimes constitute a difficulty in employing this apparatus. When the plaster-of-Paris is of fresh quality, recently burnt, or, at least, if old, secured from air and moisture since it was first burned and ground in an air-tight case, no difficulty should be experienced. Mistakes may be made by one unused to its application, in mingling it with water; and the usual mistake is rather on the side of making the cream too thick than too thin. The plaster powder can usually be best mingled by being sprinkled into the water from the one hand, while the plaster and water are stirred together with the other. This should be effected in a common wash-hand basin. If experienced in the use of the gypsum, or if possessed of an abundant supply, the whole quantity likely to be required may be prepared at once, just before it is to be applied. If, however, the operator is inexperienced or inexpert in its employment, he may have it made in two or more smaller quantities, so as to secure himself against the plaster hardening too quickly for his rate of application.

I have been asked if this method of treatment is original? So far as I know, the only anterior suspension splint which has been employed or recommended is that of Dr Nathan Smith, of Baltimore.[1] The nature of that splint, and its essential difference from the splint and apparatus I have just described, will be at once perceived from the following extract from Professor Gross's work on surgery:[2]—

"Professor N. R. Smith has for a long time treated fractures of the thigh with great ease and success with what he calls the *anterior splint.* It consists of a single piece, made of wire, of the size of a No. 10 bougie, and bent at each extremity—the whole representing the form of a long parallelogram, 3 inches wide above, and 2¾ inches below. It must be long enough to reach from a point a little above the anterior spinous process of the ilium, to an inch beyond the toes, when the thigh, leg, and foot are extended, 3 feet 8 inches being a good average length for adults. The side pieces are firmly connected by cross pieces, at a distance of about 8 inches. Thus constructed, the wire frame is easily bent to suit the case in hand. The angle at the tibio-tarsal joint, 6 inches from the extremity, is about 120°, to secure an easy posture for the foot; that at the knee, and the one at the hip are each about 160°,—the latter being 7 inches from the upper extremity. The splint, properly padded, or lightly wrapped with a muslin bandage, and secured to the limb by a roller extended from the toes upwards, is suspended, by means of a pulley, cord, and loops, to the ceiling,—a compress being placed upon the instep, and another upon the groin, to ward off pressure. The proper position of the hooks is a matter of great consequence. In general, the upper one should be attached nearly over the seat of the fracture, and the lower a little above the middle of the leg,—the object being thoroughly to equalize the pressure of the splint. The roller confining the apparatus should be well stitched, to prevent it from slipping, and great care taken that it do not make undue constriction. This apparatus, the efficacy of which has been thoroughly tested in numerous cases, is exceedingly light and comfortable, and is equally well adapted to fractures of the thigh and leg, in every portion of their extent."

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[1] See Treatment of Compound Fractures of the Thigh from Gunshot Wounds. By Redfern Davies. Lancet, 6th June 1863.

[2] A System of Surgery. By Samuel D. Gross, M.D., Philapelphia. 1862. Pp. 1033-34.

This apparatus of Dr Smith would, I have no doubt, be perfectly capable of adaptation to cases of excision of the knee, just as it is for cases of compound or gunshot fractures of the thigh or leg, or for cases of excision of the head of the femur; but the advantage over the single rod apparatus would, I think, be more than doubtful. Its double line of wire on the outer and inner side of the anterior aspect of the limb would, I should fear, give rise to a considerable interference with the line of incision, while the want of the posterior splint and the adherent gypsum bandage, which forms an essential part of the apparatus I now recommend, would detract considerably from the firmness and immobility of the leg and thigh during the process of gradual healing and consolidation.

(*To be continued.*)

ARTICLE IV.—*Hæmorrhage from Fibrous Tumour of the Uterus: its Nature, Source, and Surgical Treatment.* By J. MATTHEWS DUNCAN, M.D., Honorary Member of the Hunterian Medical Society.

IN a very large number of cases of uterine tumour, hæmorrhage is the prominent symptom, and forms in itself the main feature of the disease, so far as importance to the patient goes. In such cases, the most urgent duty of the physician in attendance, is to aim at the moderation or complete arrestment of losses of blood: and all the more, because the evil is one which may be said to feed upon itself. The more anæmic a woman becomes, the more prone is she to hæmorrhagic flux, and the less able is she to endure its evils.

Now there can be no doubt that, at present, the profession possesses only a set of very inefficient and unsatisfactory remedies against this and every other form of metrorrhagia or menorrhagia. The mineral acids, saline laxatives, gallic acid, tannic acid, acetate of lead, ergot of rye, cold, horizontal position, rest, are a few of the resources most valued, while the bleeding lasts, in cases of this kind. But every practitioner of experience knows that, although often apparently in some degree efficacious, they as often appear to have no decided favourable influence. In the run of cases of the graver sort, the practitioner has generally reason to doubt whether medicines do any direct good. Plugging the vagina is a mode of treatment useful only in cases of what may be called flooding, or severe hæmorrhage, as distinguished from long-continued dribbling. Its use demands much attendance from the physician; and it is extremely irksome and painful to the patient. Of the plan of painting the bleeding surfaces, by the careful injection of a solution of the perchloride of iron by a small intra-uterine syringe, I have some reason to think well; but my experience is, as yet, not matured so as to justify me in speaking unreservedly in its favour. But, supposing the best, intra-uterine injection can only be regarded as a palliative, and is available only if the services of a skilled practitioner are constantly at hand; and even then it requires repetition.[1]

Such being the state of therapeutics in connexion with this important bleeding, it becomes the physician of the diseases of women to study the subject closely. It is, when enunciated, evident to all that, if we have failed empirically to cure a disease, we must, as science advances, apply its acquisitions, and find out a cure based on philosophical grounds. I know that any contribution I have to offer to the science of this subject is very imperfect in itself, and far from being fit to be made the basis of general rules of treatment; yet I am sure that the method I recommend is the right one.

This method consists essentially in investigating anatomically and physiologically the whole history of this bleeding, and applying the knowledge thus obtained to the improvement of its medical and surgical treatment; and, in so doing, taking care to keep ingenious theories and conjectures in their proper subordinate place. I know no author who deliberately sets the subject before the profession in this light, while many do not devote a line to the anatomy and physiology of the parts implicated in the hæmorrhage.

Dr M'Clintock, one of the most recent writers on this disease, may be cited as giving a very correct view of the manner in which this hæmorrhage is treated. "The reason," he says, "for this frequency of hæmorrhage in connexion with fibrous tumours of the womb, has never been clearly and satisfactorily accounted for. A great many ingenious theories and conjectures have been put forward by different authors, and I shall not add to their number by proposing another. That the hæmorrhage must depend, however, upon some peculiarity belonging to the uterus itself, is at once proved by the fact that hæmorrhage is not a symptom of fibrous tumour in any other part of the body. That it does not always, or altogether, proceed from the surface of the tumour, abundant observation has shown; whilst experience teaches us, that the nearer the tumour is to protruding into the uterine cavity, the greater is the liability to the occurrence of hæmorrhage."[1]

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[1] I do not wish here to enter upon the history or merits of this operation. But I may mention that it has been, I believe, extensively used by Dr Braun of Vienna and Dr Routh of London. Dr Marion Sims uses a diluted liquor ferri persulphatis. Dr Savage uses the tincture of iodine, and claims for it, as has also been done for the other injections, more advantages than the mere arrestment of bleeding.

The bleedings from fibrous uterine tumour may be divided into two kinds, passive and active. Of these the more frequent and ordinary is the passive. For patients suffering from this disease are soon rendered anæmic. Their blood is watery. It oozes away, often copiously enough; it is not actively discharged. It is a loss to the patient, not a relief. All this is evident; but a more important distinction among such hæmorrhages remains to be made. They may be capillary, that is, from numerous vessels so small that their ruptured coats escape detection; or they may have their source in considerable venous sinuses temporarily or permanently open.

I have no doubt that, in a majority even of the graver cases, the hæmorrhage is from capillary vessels, resembling the hæmorrhage of ordinary menstruation, and having its source in the same vessels;[2] that, occurring at monthly periods in women still ovulating, it is a menorrhagia differing from more usual forms of menorrhagia in its peculiar exciting cause. It is well known that, in some cases of fibrous tumour of the uterus, there is no loss of blood whatever. Of such cases I have here no occasion to speak. When extraordinary losses of blood do take place, there are, in varying cases, different explanations easily applicable.

First, The mere presence of the tumour in the organ, acting as an irritant, may increase and prolong the common congestion and hæmorrhage of menstruation, or may produce these states apart from the periods of ovulation. This irritation is well known to increase according as the tumour is near the mucous membrane of the uterus, and it is natural to expect this result from its proximity; for, when such tumours become polypoid, or are true polypi, then there is a still farther increase of irritation and of congestion, and a more frequent attendance of hæmorrhage upon the disease.

Second, It is readily conceivable that, entirely apart from irritation of the uterus, and specially of its mucous membrane, a fibrous tumour may cause and keep up capillary hæmorrhage by mechanically impeding the returning current or currents of venous blood, as they pass through the uterine wall, or broad ligaments, or great veins of the pelvis. This is not only a conceivable, but is rendered in some cases a highly probable cause, by the study of the analagous condition of pregnancy. In the latter condition, tendency to hæmorrhoidal congestion and other venous diseases, in parts below the uterus, as well as to uterine bleeding, is not, I believe, always the result of mechanical disorders. But there can be no doubt that it often is caused or aggravated by such bad mechanism, and removed or relieved by the simplest means for mechanically averting the evil, such as the use of posture.[1]

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[1] Clinical Memoirs on Diseases of Women, p. 127.

[2] See Guy's Hospital Reports, vol. ii. of 2d series, p. 118, or Dr Oldham's Account of the Anatomical Source of Hæmorrhage in Menstruation.

Third, There is another explanation of the great capillary hæmorrhages accompanying fibrous uterine tumour, namely, the existence in some cases of greatly increased extent of bleeding surface. Every pathologist is familiar with the very varying conditions of the uterine cavity in this disease. It is often distorted, often elongated, sometimes in addition greatly increased in real (not potential) capacity. Of all these conditions, examples are constantly occurring. Works on morbid anatomy supply numerous drawings illustrative of them. The source of menstrual hæmorrhage is this mucous surface, and it is natural to expect that with the increase of its superficial extent the amount discharged should also be augmented. I see no reason for doubting that all the mucous surface, of the body of the uterus, or of parts connected directly with it, such as the mucous membrane investing a fibrous tumour, yields the bloody flux.

Lastly, Among causes of capillary hæmorrhage may be mentioned presence or misapplication of uterine contraction. Whether this be really influential in cases of this kind I am not prepared to assert.[2] I merely suggest the investigation of the question for physiological inquirers. That, in the second great class of hæmorrhages in connexion with fibrous tumour, it has a potent influence, I have no doubt.

Cases in which capillary hæmorrhage occurs and in which the above-described causes operate in producing it, increasing it or continuing it, may be classified as cases in which astringents or other medicines administered by the mouth may be expected, at least, to have some power of restraint or arrest. By so classifying them, I do not imply that other means, even surgical interference, may not also be useful as hæmostatic remedies. It is probably to bleeding of this kind that Mr Baker Brown alludes when he gives his opinion as to the rationale of some kinds of surgical interference,[3] hereafter described.

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[1] The following extract, from Cruveilhier's Traité d'Anatomie pathologique générale (tome troisième, p. 677), gives a clear statement of the condition of some of the vessels liable to be implicated in injurious pressure.—"J'ai eu occasion d'étudier les vaisseaux utérins dans le cas de corps fibreux volumineux: les veines utéro-ovariennes et les veines utérines qui viennent de l'hypogastrique, ont un volume énorme: dans un cas de ce genre où les veines superficielles avaient sillonné la surface de l'utérus, plusieurs avaient le volume d'une très grosse plume à écrire et même davantage. Les arteres n'avaient augmenté de volume que dans une bien moindre proportion."

[2] In connexion with this question, the dissections and remarks of Rouget demand careful attention. See the Journal de Physiologie. Paris, 1858. Cruveilhier speaks of expulsive efforts causing bleeding, without mentioning the open condition of vessels as co-existent. Perhaps Atlee means to refer to the same thing when he writes what is hereafter quoted (see foot-note, p. 637).

[3] Obstetrical Society's Transactions, vol. vi. p. 34.

The second class of hæmorrhages in this disease is that from open venous canals. If the former class finds an analogue in menorrhagia, this class finds its analogue in the floodings of pregnancy, an analogy which future improvements in surgery may extend to the treatment of it.

The great anatomical fact, in this class of hæmorrhages, is the close resemblance of the development or hypertrophy of the muscular tissue of the uterine walls, to that occuring in normal pregnancy. The muscular tissue and the uterine sinuses ramifying in it, and especially in the looser layers immediately surrounding the tumour, grow in a closely similar manner. Cruveilhier, whom no author has excelled in graphic description of the anatomy of this disease, decidedly overstates this analogy when he says in his great work on pathological anatomy (text accompanying Pl. vi. livr. xiii.), and elsewhere repeats, that every fibrous tumour which is developed in the thickness of the uterus, has, as a consequence, a hypertrophy and softening which represent exactly the hypertrophy and softening of this organ during pregnancy. The words, "represent exactly" are to be objected to as an error from want of care in statement, for Cruveilhier himself points out differences between the hypertrophy and softening in the two conditions. Much has yet to be done before exactness can be attained in the statement of these differences, but even now some may be noted. The uterine development in fibrous tumour is confined to the proper or muscular layer of the organ. At no stage of the disease (uncomplicated with pregnancy) is there any production of the wonderful decidual hypertrophy of its mucous membrane. The hypertrophy of the muscular coat is often, indeed generally, in interstitial tumours, greater than in a pregnancy of corresponding bulk. At no time before parturition is the proper tissue of the uterine wall seen of the thickness of an inch, as it may be found around such tumours.[1] The same exaggerated size is observed, in many cases, in the uterine sinuses ramifying around a fibrous tumour. Both these differences may be due to the comparative long continuance of the morbid cause of hypertrophy, and perhaps to its essential unnaturalness or morbidity. Lastly, there are great differences in the shape of the uterus and in the thickness of the different muscular layers, in cases of fibrous tumour, otherwise apparently alike.

"The venous vascular network," says Cruveilhier,[2] "between a fibrous tumour and the tissue of the uterus, always considerable, even in fibrous tumours of small size, becomes truly prodigious when fibrous tumours have acquired large volume, and especially when they are œdematous. Thus, around an œdematous fibrous tumour, of the bulk of the head of an adult, I have found veins of the size of the little finger, filled with coagulated blood." It is bleeding from openings in these vessels that we have now to consider.

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[1] For one recorded example, among several, see Dr Sanders' description of the tumour excised by Dr J. D. Gillespie. Edinburgh Medical Journal, July 1866, p. 35.

[2] Traité d'Anatomie pathologique genérale, tome iii., p. 685.

The *first* kind of opening that has been found is a simple round aperture, establishing a connexion between the uterine cavity and one of the large sinuses lying in the muscular tissue, enveloping a tumour. A case of this kind has been described by myself.[1] I am not aware of any other quite similar, but the almost accidental discovery of the opening in the autopsy of the case recorded by me suggests to me the probability that, even in dissections of uteri containing fibrous tumours, such an aperture, unless carefully looked for, may escape notice.[2]

The case is interesting, because the hæmorrhages proved fatal, and because of the discovery of the source of the bleeding; and for the observation being made, I am indebted to the presence of a small clot, whose decolorized end, not more than half a line in diameter, projected on the healthy, and to the naked eye, at first sight, apparently smooth and entire surface of the mucous membrane of the body of the uterus.

CASE I. Miss G., a young lady, had consulted me in the spring of 1863. She was very fat and anæmic, and it was easy to diagnose the presence of a fibrous tumour in the uterus. Its upper border reached to near the umbilicus, and it felt, as it was afterwards proved to be, of little above three inches in diameter. She complained of great menorrhagia, but was unaware of its cause. She left town to spend the summer by the seaside, and returned to Edinburgh in autumn.

I received a hurried message, requesting me to pay her a visit. On seeing her I felt sure she was dying, and this event occurred in a few hours afterwards. The only striking symptom was slightly bluish tint of the face, with deep laboured noisy (not stertorous) respiration. She had no complaint, and felt no special pain. Her skin, pulse, respiratory sounds, heart's action, were nearly natural. For several days she had had no hæmorrhage; only, during the last two days of her life she had had a little show of brownish slimy mucus.

I am indebted to Dr Grainger Stewart for performing the autopsy, and for aiding me in making out the facts now to be recorded. Nothing very interesting was observed in the chest, etc. I shall confine my narrative to the condition of the uterus. The right ovary contained a cyst about the size of a walnut.

The uterus occupied the central region of the hypogastrium. Its upper margin was nearly on a level with the umbilicus. Its os was high in the pelvis. The cervix was hypertrophied, and, measured by the extent of the arbor vitæ, was elongated to a dimension of an inch and a half. A globular fibrous tumour, fully three inches in diameter, occupied the fundus uteri. It was moderately hard. Venous sinuses that would admit a small crowquill were seen permeating it in various directions. It projected downwards moderately into the deformed and expanded uterine cavity. It was everywhere surrounded by a covering of the proper tissue of the uterus, the layer between the tumour and peritoneum being more than twice as thick as that intervening between it and the mucous lining of the uterine cavity. The thickness of the latter layer was an eighth of

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[1] Edinburgh Medical Journal, January 1864.

[2] The not very rare rupture of a varicose vein in the leg is worthy of study, from its points of similarity and dissimilarity to the circumstances of the case above given. For an account of it, see Dr A. R. Simpson's paper in the Edinburgh Medical Journal for February 1865.

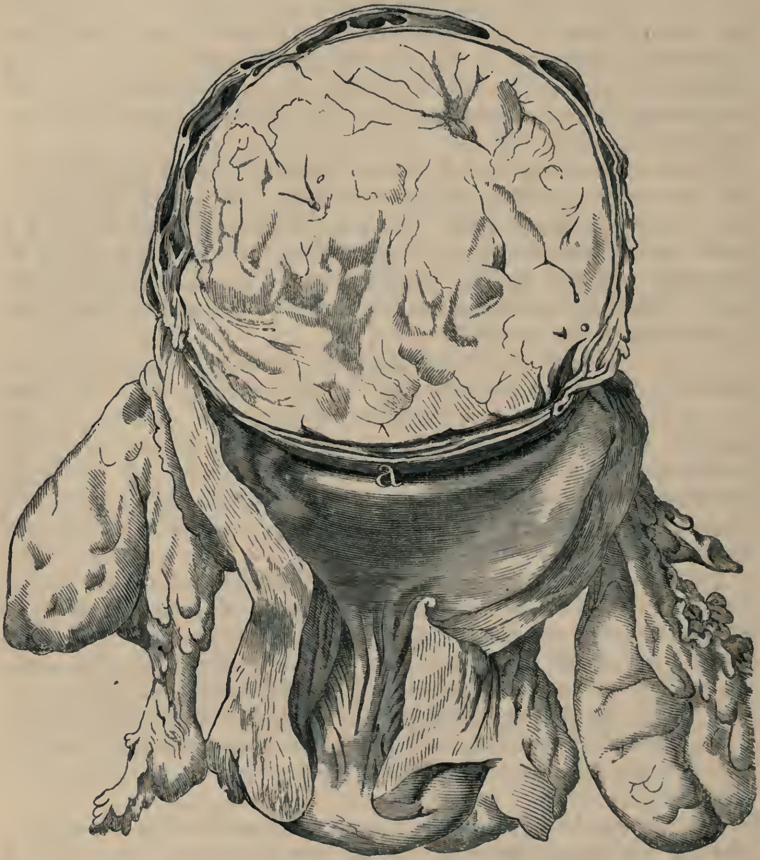

Fig. 1.—Showing a Section of the Uterus in Case I.[1]

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[1] The opening of the uterine sinus was at *a*, the most prominent part of the tumour, as it pushes the uterine mucous membrane downwards.

an inch. In this layer of uterine tissue, especially in the parts adjacent to the tumour, there was developed a reticulation of enormous uterine sinuses, such as are observed in the same part in pregnancy. These were largest above and below the tumour, and contracted in dimensions as they approached the side of the uterus, to emerge from it between the serous layers of the broad ligament. The uterine cavity was deformed and extended by the projection into it from above of the fibrous tumour. It contained a brownish mucus. Its mucous surface was pale, smooth, and healthy, and to a casual observer presented nothing but a little clot about the centre of the part projected by the underlying tumour. On inspecting this clot with a glass it was found to be protruded through a small round opening which it did not nearly fill. The opening was one-twelfth of an inch in diameter, and resembled somewhat the opening of a venous sinus exposed by separation of the placenta. A probe could be easily passed through it into the uterine sinuses already mentioned. The clot was of about two lines in length, and projected from an empty flat uterine sinus.

The *second* kind of opening which I have to describe may not differ greatly from that last given. Cruveilhier's description of the only case, which I know to refer to, is not precise enough to enable me to decide its exact nature. Yet I place it as a second kind, distinct from the first and third. It consists of "openings of the uterine sinuses, large, and occupying the lower part of the tumour." Cruveilhier's case furnishes fig. 2, pl. 6, livr. xiii. of his great work on pathology, and is described as follows in the third volume (p. 683) of his Treatise of General Pathological Anatomy :—

"This fibrous tumour presents a specimen which has undergone œdematous ramollissement. The ovoid uterine globe exhibits very nearly the bulk which it acquires in a woman arrived at the ninth month of pregnancy. Moreover, the uterus, in the thickness of its walls, in the softness of its tissue, in the calibre of its arterial and venous vessels, represents, trait for trait, the uterus of a woman arrived at the term of pregnancy."

"The tumour was of a very remarkable, fluctuating softness. Its section showed an aspect quite peculiar. Here and there were seen cavities,—a kind of *geodes*, full of serosity. This tumour was traversed by very considerable venous vessels, filled with concrete and adherent blood. The small masses, whose agglomeration constitutes fibrous tumours, were separated from one another, and serosity filled the interspaces. The softness of the tumour rendered its enucleation *en masse* difficult; nevertheless, the line of demarcation was easily found, separating the uterine tissue from the tissue of the tumour."

"The woman who carried this tumour had succumbed to a hæmorrhage, provoked by expulsive pains; and I discovered the source of the hæmorrhage in large openings of the uterine sinuses, which occupied the inferior part of the tumour. This patient had been subject for a sufficiently long time to hæmorrhages, always provoked by expulsive pains."

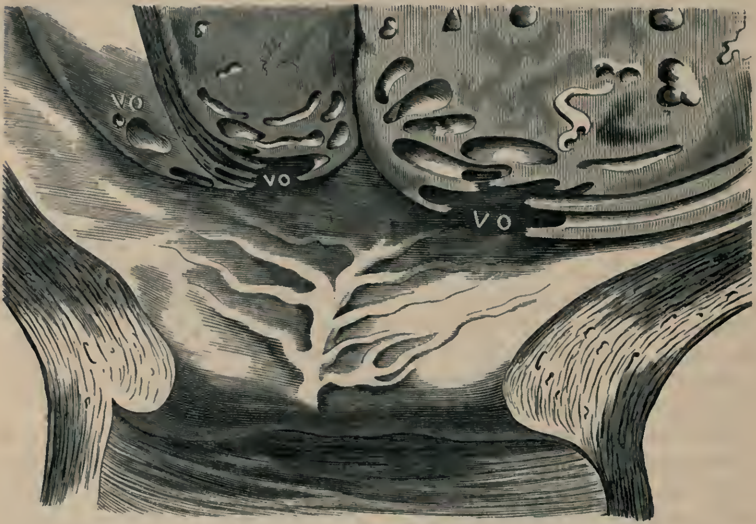

FIG. 2.—Showing the lower part of the Tumour in Cruveilhier's case.[1]

The *third* kind of open uterine sinuses is the result of partial expulsion of the spontaneously enucleated tumour into the uterine or vaginal cavity.

It may be just to regard these three kinds of bleedings as stages in a gradual process which may be called sanatory, inasmuch as the enucleation and expulsion of the fibrous tumour, if fortunately arrived at, may be regarded as its completion.

First, then, occurs a simple ulceration of the mucous membrane, investing the most prominent part of the tumour as it bulges into the uterine cavity. This ulcer is small, rounded, and establishes an opening into a uterine sinus. Secondly, the ulceration increases, and several openings are formed. Lastly, through the increase of the ulceration, the tumour is partially denuded: uterine contractions follow, and expelling the tumour through the opening in its investing wall formed by this ulceration, produce new lacerations of the sinuses, as they lie at the line of demarcation between the last separated portion of tumour and the adjoining uterine wall.

The case of Mrs E. (Case XIII.) hereafter given, is a very remarkable example of the third or last kind of bleeding from open uterine sinuses. In the tumour there described, large venous canals, open at various points, were observed ramifying between the lobules of the part of the tumour which had been for some weeks lying bare of uterine investment in the uterine cavity. These vessels contained recent clots, probably produced by the plug of lint soaked in the solution of perchloride of iron, which had arrested the last terrible attack of flooding.

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[1] The letters V O indicate the openings of uterine sinuses.

In all these three forms of open uterine sinuses, bleeding may be the escape of the whole or a part of the blood permeating the open sinuses. This bleeding may be intermittent in consequence of changes in the current of blood through the affected sinuses. The bleeding may cease in consequence of there being no blood for a time passing through the canals. The bleeding may return in consequence of the erect position, or the excitement of a menstrual period, or efforts in going to stool destroying the retentive power of the abdomen, and producing repletion of the affected vessels, which may have been previously quite empty. This kind of hæmorrhage may be arrested by expulsive pains or other uterine contractions, and consequently, ergot of rye may be a useful medicine in the circumstances. Uterine contraction, if affecting the open canals, may be expected to close them, compressing them between the tumour and the uterine wall. A similar action is supposed by Baker Brown and M'Clintock to be the cause of the hæmostasis following incision of the os and cervix uteri. This operation is said to "permit the fibres of the body of the uterus to contract upon the contained tumour, and thereby to compress the vessels and prevent hæmorrhage."[1]

But Cruveilhier points out that expulsive efforts may have, as a result, the laceration of the layer of looser tissue which immediately covers such tumours, and that if great vessels are present in the thickness of this layer, fatal hæmorrhage may be the result of the laceration. Such hæmorrhage may be repeated with each repetition of the expulsive efforts. This statement of Cruveilhier's does not appear to me fully to describe the matter, and he does not, so far as I know, advance any case, carefully detailed, in proof or illustration of it. I have already said that I regard uterine action, as in some cases, hæmostatic or sanatory. I would ascribe hæmorrhage to such uterine action, or efforts as result in some degree of expulsion, a change which cannot go on without laceration, unless it be the consequence of a slow process of enucleation, with advancing ulceration at the line of demarcation between tumour and uterine wall.

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[1] M'Clintock on Diseases of Women, p. 149. M'Clintock, in another place (p. 152), quotes Atlee's account of the arrest of hæmorrhage after incision into the exposed portion of the tumour. With Dr M'Clintock's last remarks I quite coincide. Atlee, says Dr M'Clintock, "is of opinion that the vessels of the membrane enveloping the tumour are the source of the hæmorrhage (as Dr Davis supposed), and not the uterus itself. And, arguing from this (rather questionable) foundation, he offers a theory as to the way in which the above practice affects the suppression of the hæmorrhage: 'The veins of the investing membrane become at times greatly engorged, in consequence of their circulation being impeded by the muscular action of the uterus; while the arteries, by reason of their more resisting coats, continue to supply them with blood. The point of least resistance must consequently be at the os uteri, as all the other parts are compressed by the contracting uterus. The veins on the surface are thus distended. The mucous membrane is delicate, and offers but little resistance to the rupture of these vessels'; and the practice above-mentioned is based upon this fact, he says. Few practitioners will be disposed, we think, to accept, without sufficient proof, this anatomical description, or to acquiesce in the reasoning based upon it; . . . ."

The case of Mrs E. (Case XIII.) is a fine illustration of the bleeding produced, not by expulsive efforts alone, but by these efforts accompanied by partial extrusion of the tumour. In this case, the very great hæmorrhages which threatened to cause sudden death, were observed only during uterine attempts at expulsion of the tumour; and the condition of parts was very carefully examined, and well ascertained. I believe the mechanism of this kind of bleeding is as follows: The tumour embraced by the hole in the uterine wall through which it is passing, analogous to the passing of the fœtal head through the os uteri, is necessarily pressed on, or partially strangled by, the active uterine fibres forming the aperture. This strangulation checks the returning current of venous blood in all the uterine sinuses beneath it. But the deeper and still adherent parts of the tumour receive[1] supplies of blood which pass into the separated portion, and from these supplies is supported the outward flow or hæmorrhage. This theory appears to me to have no fault of extreme ingenuity; it demands no laceration of vessels, and no new laceration of vessels with each return of expulsive efforts. It merely demands open vessels and partial expulsion.

When the expulsion is nearly perfected, when the tumour is a sort of false polypus, then the great hæmorrhages are often arrested; as numerous examples, which I might adduce, testify. There are still expulsive efforts and lacerated vessels, but violent hæmorrhage has ceased.[2]

To many, I have no doubt, it will occur as a reflection, that these descriptions of bleedings from open uterine sinuses, however accurate and exact they may be, can be of limited practical importance, because of the supposed rarity of the conditions, a rarity which the silence of many authors, and the tenor of the remarks of others, certainly imply. But I am of opinion that the process of spontaneous, partial, or nearly complete, enucleation of interstitial fibrous tumours, and the consequent opening of vessels, is far from rare. I have seen several such cases. I believe it to be a more frequent occurrence than the change of an embedded tumour into the condition of a polypus. This latter change is far more frequently spoken of and described in books than the former, and evidently believed to be of more common occurrence. I offer opposition, on this point, only from my own experience; and I have never observed the change of an embedded fibrous tumour into a fibrous polypus, that is, the gradual protrusion from the uterine wall of a fibrous tumour, carrying with it a mucous, and a partial or complete muscular covering. But I have observed, in all its stages, except absolute spontaneous separation, the gradual change of a fibrous tumour into a false polypus, that is, the enucleation of a fibrous tumour or its expulsion bare, into the uterine cavity, or into the vagina, the only connexion with the uterus being, at last, not a stalk or pedicle, but a portion, often a small portion, of the investing capsule of the tumour, which, in botanical language, is sessile. In such cases I have repeatedly removed the tumour by dragging, without the use of any cutting instrument. Tumours in this state of partial separation by enucleation have been, I doubt not, often described as polypi; but this has been done in ignorance, and it evidently is either error or confounding of very different things.

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[1] For an account of the circulation in a fibrous tumour, see Dr R. Lee's work on "Ovarian and Uterine Diseases," p. 195.

[2] I venture to suggest that Drs West and M'Clintock should reconsider their opinion (stated in page 176 of the work of the latter) as to the diminution of bleeding following advance of a polypus from the uterus into the vagina. I believe this is true of false polypi, that is, fibrous tumours enucleated, but not separated. I am doubtful of its correctness if true polypi alone are regarded.

The following cases are examples of the various states in which fibrous tumours are found, as true or false polypi, in the genital passages.

Case II. In the following case is illustrated the occurrence of a true fibrous polypus of the uterus. It is remarkable on account of the small amount of menorrhagia it induced.

Mrs —— has never suffered from violent menorrhagia. She has recently had three successive pregnancies which have terminated favourably. Her last child was born ten days ago without any artificial assistance. During labour a globular tumour was felt in the vagina, attached to the right anterior quarter of the cervix uteri and with very little constriction at the neck forming its pedicle. It was almost sessile and of the size of an ordinary hen's egg. I pulled the tumour gently downwards and excised it with scissors. A little gush of blood followed the excision, but was easily checked by a moderate amount of plugging. The tumour has an entire mucous and muscular coating, the latter thinning towards the end of the tumour remote from the uterus. She made a perfectly satisfactory recovery.

Case III. The next case is one of the somewhat rare kind of fibrous uterine polypus, which is a continuous outgrowth of the proper tissue of the uterus. It is the only case in which, among a large number of operations, I have tied the pedicle of a polypus. The ulcer on the polypus and the appearance of small openings in the venous ramifications on its surface are noteworthy.

Mrs —— was placed under my care by Dr Littlejohn. She had enjoyed good health until about three months before I saw her, at which time she had a miscarriage at the third month of pregnancy. Ever since then she had had a copious discharge, which she describes as consisting of blood and matter. A large elongated polypus protrudes partially from the vagina. It might be compared in size and shape to a flattened hen's egg, and its stalk of about an inch in length was nearly about half as thick as the body of the polypus. A considerable artery could be easily felt pulsating in the thick pedicle. I therefore tied it with silk, and then excised by scissors below the ligature. The case required no further care.

The polypus was entirely composed of the proper or muscular tissues of the uterus. It was therefore not an ordinary fibrous polypus, for its stalk was continuous with its substance and with the proper tissue of the cervix.

Near the pedicle of the polypus was a small ulcer on its surface. Under the ulcer, venous ramifications could be seen, having apparently small openings on its surface.

CASE IV.

The three next cases are examples of the common spontaneously enucleated, but not disconnected, fibrous tumour, or false fibrous polypus uteri.

Mrs R., æt. 49, had her last child nine years ago. For seven months past she has had great losses of blood at the monthly periods, and profuse leucorrhœa when bloody discharge is absent. Under these circumstances, she was sent to me by Dr Paterson of Carnwath. A tumour, about the size of the clenched fist, occupied the roof of the vagina. It was easily pulled down and its attachment cut through. No hæmorrhage followed. The woman made a good recovery.

The tumour was a lobulated fibrous mass, destitute of covering by mucous membrane or muscular tissue.

CASE V.

I was called by Dr Myrtle to Mrs ——. She was æt. 47, and childless. Four months ago, she suffered considerably from pain in the belly, and ever since then has had bloody discharge. A tumour, as large as a hen's first egg, lies at the os uteri. I fixed it with a volsella, and removed it with curved scissors. It was a naked fibrous tumour.

The sixth case I shall give as a quotation from the reports of the Medico-Chirurgical Society.[1]

CASE VI.

"Dr Matthews Duncan exhibited a uterine polypus of very large dimensions, which he had recently removed from an unmarried woman, aged 47 years. It measured five and a-half inches in length by four and a-half in breadth, and was of the ordinary fibrous structure. It was, as he had pointed out in former specimens, quite destitute of muscular or mucous coverings, being, in fact, an enucleated fibrous tumour which had been expelled from the uterine wall, its original seat, into the vagina, from which it was removed by operative interference. In this case, it was remarkable that, during all this growth, enucleation, and change of situation, the woman's health had been scarcely disturbed in the least degree. She had irregular vaginal discharges, but, as she had no suffering, she paid little attention to them. Indeed, it was not until the supervention of a cough, which forced the tumour down upon the outlet of the pelvis, and compressed the urethra, giving rise to retention of urine, that she had any complaint to make of suffering in the hypogastric regions."

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[1] Edinburgh Medical Journal, 1861, vol. vi. p. 954.

"It is a mistake to suppose that the difficulty of dealing with these large polypi lies in dividing the (so-called) pedicle. In the present case, the pedicle could not be reached by the finger, until the tumour was removed from the vagina by traction. This was a very laborious process, effected only by repeated and persevering efforts, assisted by the bearing down of the patient. It was, indeed, a difficult labour in a primpara; only, instead of a child's head, a relaxed vagina, and labour pains, there was a fibrous tumour, an undilatable vagina, and traction by hooks fixed in the tumour. There was no difficulty in dividing the pedicle; for it snapped across as the tumour descended, and scarcely a drop of blood issued from it. If the pedicle had not broken, it would have made little difference in the operation; for the extent to which the easily movable uterus is dragged down in such a case is not considerable."

It would be merely tedious to adduce more cases of this kind.

(*To be continued.*)

Part Second.

REVIEWS.

A Treatise on the Principles and Practice of Medicine; designed for the Use of Practitioners and Students of Medicine. By AUSTIN FLINT, M.D., Professor of the Principles and Practice of Medicine in the Bellevue Medical College. Philadelphia: Henry Lea. 1866.

DR FLINT is well known, both in America and in this country, as the author of various works on medical subjects, and as a pretty frequent contributor to periodical literature. In the present volume he appears in a more ambitious character, as the author of a complete treatise on the principles and practice of medicine. To be successful in such an undertaking requires qualifications which few single physicians can be expected to possess. Both the science and practice of medicine have made such advances, that to be equally acquainted with all branches has become almost impossible. Of course, it is easy enough to frame a compilation of a certain character; and in such an undertaking every one must avail himself largely of the labours of others; but to prepare a satisfactory work, the materials must have been carefully digested by the author, and be communicated to his readers in the form in which they have impressed themselves upon his own mind.

We are, on the whole, disappointed with Dr Flint's work; we do not think that the hope of the author, expressed in the preface, has been fulfilled,—"that it may be found to represent fairly the existing state of the science of medicine, with respect to the subjects of which it treats; and to reflect the views of those who exemplify, in their practice, the present stage of the progress of medical art." Dr Flint has been decidedly more successful in the practical than in the scientific part of his work, which is certainly not on a level with the pathology of the day. The first eighty pages are devoted to the principles of medicine or general pathology, and contain a brief description of some of the principal changes occasioned by disease in the tissues of the body, with three short sections on etiology, symptomatology, and general therapeutics. In speaking of fibrinous exudations, Dr Flint adheres to the old view that the products of inflammation are a direct exudation from the bloodvessels, and does not once allude to the doctrines of the Cellular Pathology which have made so much progress both in Germany and in this country. Dr Flint, in speaking of the diversities met with in the constitution, course, and consequences of fibrinous exudations, thus alludes to the doctrine of blood-crases—"With reference to these diversities, as well as to the different exudations, the view taken by Rokitansky is, that they represent corresponding diversities pertaining to the fibrin contained in the blood. The blood-changes which are supposed thus to account for differences as regards the local products of disease, this author calls the *crases* of the blood. Each of the different exudates, or the different varieties of the same exudate, in other words, has its own particular blood-crasis." The fact however is, that the doctrine of blood-crases, shown by Virchow to be untenable, has been long since abandoned by its author.

Of course, similar views are entertained by Dr Flint as to the mode of origin of tubercle; he views it as an exudation from the blood occurring in connexion with "a special constitutional state expressed by the terms tuberculous cachexia or dyscrasia." Naturally enough Dr Flint has great difficulty in deciding as to the relations of grey and yellow tubercle to one another. As Lebert's *corpuscules tuberculeux* are not found in them, Robin has denied the tubercular nature of the granules found in the pia mater in cases of tubercular meningitis; and Dr Flint, following the latter histologist, appears to have great doubt as to the nature of the so-called grey tubercle. "As regards gross characters and behaviour," he says, "this deposit certainly differs widely from true tubercle." The word *gross*, in the above extract, is employed by Dr Flint to express the appearances as recognised by the naked eye, and is used in contra-distinction to *microscopic;* it has certainly the merit of brevity, but has a harsh and unnatural sound, and the use of the word in this sense will not, we trust, be adopted in this country.

The section on the degenerations is not satisfactory. Dr Flint fails to make the distinction between fatty degeneration as a passive, and as the result of an active process: the distinction, however, is most important; in the former case, there is usually a persistence of the normal structures (cells, muscular or other fibre, as the case may be); while in the latter, the tissues affected generally perish, and loss of substance is the result. Waxy degeneration is treated of very shortly, little more than half a page being devoted to it, and the description is exceedingly imperfect; nothing is said regarding one of its most striking peculiarities, that the change usually commences in the minute arteries of the affected part.

The second, and by much the larger portion of Dr Flint's volume, is devoted to special pathology. The local diseases are first treated of, and are considered according as they affect the respiratory, the circulatory, the digestive, the nervous, and the genito-urinary systems. The general diseases, embracing the fevers, gout, rheumatism, diphtheria, and scurvy, are brought together in a separate section. We do not agree with Dr Flint that "diseases of the skin undoubtedly belong to surgery," and we consider that a text-book on the practice of medicine is incomplete in which no reference is made to them. Dr Flint's other reason for omitting them, that "there are numerous treatises devoted specially to their consideration," would be an equally good one for excluding any or every disease in the whole range of medicine.

The first of the special diseases treated of is acute pleurisy, or, as Dr Flint invariably styles it, acute pleuritis. We may mention the arrangement he adopts in considering it, as it is that which he follows in the case of nearly every disease. After describing its anatomical character, Dr Flint gives its clinical history, which comprehends what we should call the symptoms, as distinguished from the physical signs; next follow sections on its pathological character and duration; and it is not till we come to the diagnosis that we have any information regarding the physical signs to which it gives rise. This arrangement is one which we do not approve of; the physical signs constitute an important part of the clinical history of every disease; while under diagnosis we expect to find only a reference to the diseases with which it may be confounded, and of the means by which they are to be discriminated. If Dr Flint's arrangement be in this respect faulty, we have nothing to say regarding his description of the disease, which is clearly and practically given. In reference to the treatment recommended, we have only to remark that Dr Flint prescribes opium more freely than most practitioners are in the habit of doing in this country; he says that "it will be generally judicious to prescribe opium, in some form, in doses sufficient to relieve pain, and place the system fairly under an anodyne influence." In the treatment of inflammations of serous membranes, we are decidedly of opinion that this treatment is judicious. Opium not merely relieves pain, but diminishes constitutional irritation; and if there be no contra-indication, such as arises from its tendency to arrest secretion or to mask symptoms, it is often very valuable. Dr Flint, we are glad to see, is opposed to the use of mercury, and expresses himself far more decidedly than most systematic writers are in the habit of doing. "Mercury," he says, "I have long ceased to employ: of doubtful efficacy, the annoyance and other evils incident to mercurialization render it objectionable."

The next chapter is on "Pneumonitis." Why the convenient and time-honoured name pneumonia should be replaced by the harsher and not more expressive term pneumonitis, we must leave to American physicians to say. Were there a consistent nomenclature of diseases, and were it decided that all inflammatory affections should be indicated by the termination *itis*, we might consent to the new title; but in the meantime we prefer the old one, which means simply disease of the lung, both on account of its softer sound, and from the associations connected with it. It is no doubt prejudice, but our pleasure in reading Dr Flint's work is seriously interfered with by his use of terms with which we are not familiar in this country. We have already alluded to his peculiar mode of employment of the word *gross;* we shall only mention one or two others, though there is scarcely a page in which some peculiar term or idiom, or some variety in orthography, will not offend an English eye. *Replete*, in English, is only used as an adjective; and there is no such compound as over-replete. But Dr Flint boldly coins a new verb, which is used pretty frequently, as in the following sentence: "As a consequence of pulmonary obstruction, the right ventricle is over-repleted." In describing the phenomena of fever, Dr Flint says, "The urea and uric acid, as a rule, are increased—a fact showing augmented *disassimilation*." What advantage disassimilation, a word entirely new to us, possesses over destruction or waste of tissue, we are at a loss to conjecture. Again, a chapter is headed "On Diseases affecting the *Collatitious* or Solid Viscera of the Abdomen." The word collatitious, when employed at all, signifies *contributed by many;* but we do not see how this can be considered synonymous with solid.

To return to pneumonia, we may advert to Dr Flint's observations on the employment of blood-letting in its treatment. His remarks regarding it are the following:—"It is admissible in certain cases as a palliative, and perhaps, to some extent, a curative measure, in view of the promptness of its effects. The circumstances which warrant its employment are, high febrile movement, the pulse denoting increasing power of the heart's action, and the condition of the plethora, or at least a robust constitution. It is contra-indicated wherever the febrile movement is not marked, the pulse denoting activity without power, the patient anæmic or with a feeble constitution. In most of the cases in which it would be admissible, provided the same ends could be secured by other means, the latter are to be preferred. These consist of depletion by saline purgatives and sedative remedies." With these opinions we cannot agree; we do not consider blood-letting indicated by a strong pulse, or contra-indicated by a weak one; what we have to look to is the state of the respiration; if the respiration is seriously embarrassed, if there is a larger amount of blood in the lungs, than the diminished extent of breathing-space is capable of aerating, blood may with advantage be withdrawn, not that its withdrawal tends to cut short the inflammation, but that it fulfils the important therapeutical indication of endeavouring to put the diseased organ at rest.

We shall only allude to one statement of Dr Flint's in reference to the prognosis in pulmonary tuberculosis. He says, "The occurrence of perineal fistula is favourable, and so I have been led to regard laryngitis, excepting the cases in which the epiglottis and tip of the larynx are involved, so as to interfere with deglutition." This is quite contrary to our own experience, and is opposed to the opinion usually entertained by physicians in this country. A tubercular affection of the larynx is not only a cause of much additional irritation, but we believe it usually occurs in cases which have a tendency to run a rather rapid course.

The only other subject to which we can refer, is to Dr Flint's views with regard to Bright's disease. First, as to the acute form. Under the heading "Clinical History" the following statement occurs:—"The affection may end in complete recovery, or it may eventuate in a chronic affection. The latter is rare. As a rule, if the patient be not cut off by uræmic poisoning or the complications which are liable to occur, the recovery is complete and permanent." Our own experience has been by no means so favourable. No doubt the majority of cases recover; but every physician who has had much experience in the treatment of this disease must call to mind cases where, in spite of every care, fatty degeneration has occurred, and chronic disease has become established; where, for example, the kidneys have continued highly irritable, hæmaturia being brought on by the slightest attempt to rouse them to action, and where the disease has become chronic almost before the acute symptoms have disappeared. The occurrence of dropsy in connexion with the acute form of the affection is thus explained. "The diminished density of the blood-serum from the loss of albumen, together with the embarrassment of the capillary circulation from the retention of urinary principles, occasions the dropsy." The second of these conditions, the impure state of the blood, whereby its natural relation to the tissues is disturbed, is, we have no doubt, an important cause of capillary obstruction and dropsy. But we do not think that impoverishment of the serum has much or any influence in this stage. Dropsy is often one of the very earliest symptoms, and occurs before any appreciable impoverishment of the blood can have taken place. A much more important condition is the increased mass of blood, which is a temporary condition in this disease. The urine is scanty, the skin usually does not act, and as the quantity of fluid taken is generally not diminished, —nay, owing to the thirst, is often increased,—a temporary increase in the fluid part of the blood must result.

Dr Flint recognises the four following forms of kidney as associated with chronic Bright's disease. "1. The form called, after Bright, the large white kidney; 2. The hard contracted kidney; 3. The fatty kidney; and, 4. The lardaceous, amyloid, or waxy kidney." This is a division of which we do not approve; we would abandon the term "large white kidney" altogether. In many cases, the kidney so designated is in the state of waxy degeneration; in fact, no one familiar with the condition can look at Bright's original plates without recognising characteristic illustrations of this form. "Fatty kidney" alone, we would not admit as a form of Bright's disease. There may be a very large accumulation of fat in a kidney without its function being seriously or perhaps at all interfered with, provided that accumulation has been passive, that is, has not resulted from a preceding active process. It is well known that the kidneys of almost all domestic cats, especially of those inhabiting cities, present exquisite examples of fatty degeneration, and yet these kidneys have performed their functions perfectly. The danger is where there has been an acute affection, and where the increased material taken up by the cells has undergone a fatty degeneration. The process is exactly analogous to what occurs in the secretion of milk; the gland-cells undergo a fatty degeneration, the cells burst, and the free particles of oil constitute the globules of milk. But in this physiological process the cells are renewed, and no injury to the gland results. In the pathological process it is different. Cells once destroyed are not restored, and so permanent injury of the organ results. The cells and their fatty contents may be discharged, or they may remain and accumulate in the kidney, but in either case the secreting power of the organ is seriously impaired.

Dr Flint speaks of the waxy kidney as being rare; this is certainly not the case in this country, and we believe that, when it is supposed to be so, it is because the observer is not familiar with its anatomical characters.

We must here conclude our notice of Dr Flint's work. Our remarks have been principally directed to points on which we do not agree with the author. We would not, however, wish it to be supposed that we consider the volume devoid of merit. On the contrary, we fully recognise Dr Flint's diligence and practical familiarity with disease; although we do not consider his treatise to come up to our ideal of what a text-book ought to be, or to be on a level with the present state of pathological science.

Fecundity, Fertility, Sterility, and Allied Topics. By J. MATTHEWS DUNCAN, A.M., M.D., F.R.S.E., Lecturer on Midwifery in Surgeons' Hall Medical School, Physician for Diseases of Women to the Royal Infirmary, Corresponding Member of the Imperial Royal Society of Physicians of Vienna, etc. Edinburgh: Adam & Charles Black. 1866.

NUMERICAL statistics are a science of modern date. Notwithstanding the importance and even mystery which the ancients attached to number, they appear to have had no idea of the results which could be obtained by the accumulation and enumeration of facts properly classified. We must come down to our own century, and almost to our own times, before we find any scientific or general development of the numerical method. In some departments of knowledge the value of the results thus obtained are notorious; modern society could not exist without them. Notwithstanding various errors and fallacies occasionally arising from their abuse, it will not be disputed that the most valuable and certain principles of political economy are founded on numerical statistics, and, as our blue books testify, a large part of whatever is scientific in the conduct of government rests on the same basis. In medicine the use of numerical method has been, on the whole, much less satisfactory. Numbers can be correctly applied only to data identical or similar; and in medicine, unfortunately, the facts are often ill observed or impossible to ascertain, and the accumulation of them is consequently a mere multiplication of error. In therapeutics this has been especially the case. In pathology, as now more accurately known, statistics have yielded, in many cases, valuable results, but in other instances where the data are not sufficiently ascertained, the attempts have been unsuccessful, or the conclusions fallacious. As medical science, still in its infancy, attains greater perfection, there can be no doubt that statistics will take a wider, more prominent, and successful part in its progress. But meanwhile there are certain questions, medical, or allied to medicine, which are known to be susceptible of accurate elucidation by means of numbers; and of one great department of such questions, the present volume principally treats.

Dr Duncan's work is not directly practical; it is throughout scientific in its object, and the early part is exclusively so; although the application of his results to practice is, of course, sufficiently obvious and important. The materials which form the basis of his principal inquiries, the author has derived from the Register of Births for Scotland in 1855. The systematic registration having been first established in that year, the schedule in use required from the public a variety of interesting details in connexion with each return; a system which, owing to the trouble attending it, has not been continued, so that the returns of that year possess for statistical purposes an exceptional and extraordinary value. Dr Duncan has limited his selection from these registers to the legitimate births in Edinburgh and Glasgow, amounting to 16,593 children; numbers sufficiently large without being excessive. Out of these data he has constructed an elaborate series of tables, so devised as to elucidate with precision and detail the subjects of fecundity, fertility, and sterility in most of their varied aspects and relations; and from the results obtained, he has endeavoured to deduce more fully and clearly than had previously been done, the principal laws and conditions which regulate human productiveness as it is manifested either in the individual or the mass, under the present artificial circumstances of our civilized population. Several of these inquiries and their conclusions have been already laid before the profession, partly in the Transactions of the Royal Society of Edinburgh, and partly in the pages of this Journal; they are incorporated with others in the present volume. Both from the great labour, care, and skill expended in the working out of details, and from the importance of the results, as either new, or confirmatory of what was previously known or merely conjectured, the work is one of sterling value. It forms an original and important contribution, not only to obstetric science, but also to the department of political economy which treats of population, and to the principles of life insurance. It will add materially to Dr Duncan's reputation as a scientific investigator.

Within our limits, it is not possible to enter fully into all the topics discussed. But a general idea of their nature, extent, and mutual connexion may be formed by considering them as consisting of two sets, which are, however, not separated, but occur intermixed with one another in the volume before us. One of these sets is of general interest, while the other is more strictly of a professional character. To the former division belong the parts treating of fertility (including the production of twins), sterility, nubility (or the proper age for marriage), etc.; to the latter, the papers on length and weight of new-born children as affected by primogeniture and the age of the mother; mortality of child-bed as influenced by the number of the labour, the age of the mother, and the duration of labour; the doctrine of the duration of pregnancy, etc. As the title of the book indicates, it is the former set of questions, regarding the productiveness of the population, which has been chiefly subjected to detailed and elaborate investigation. The results, however, being presented in a condensed form, are so numerous that we must content ourselves with a few observations on some of them. No justice can be done to the inquiries without a study of the book itself.

Among the more recent researches of the author, which we find towards the latter part of the volume, are those on nubility, in reference to the proper age for marriage, with a view to productiveness under favourable conditions. Sadler maintained, from the results of a limited range of observations, that late marriages are more prolific than early ones, and are attended by less mortality of their offspring, so as to be more conducive to the permanent increase of the population. Dr Duncan has shown the fallacy of these opinions, and pointed out the source of their error. From a review of the whole subject, as cleared up by his previous inquiries into fecundity, fertility, survival of child-bearing, and the health and longevity of the progeny, he concludes that women married in the second quinquenniad of child-bearing life, viz., 20 to 24, by avoiding, on the one hand, the diminished issue of late marriages, and on the other, escaping the evils which accrue to mother and child from premature maternity, are the most prolific in desirable offspring, and contribute most largely to the adult population. These conclusions of the author, based on ample data, agree in the main with the opinions expressed by Major Graham, and with the statements made from observations on a comparatively limited scale by the committee of the Statistical Society of London.

The fecundity and fertility of women, as shown by the number of legitimate children, are analyzed and exhibited under a great variety of aspects. It is found, that in the present condition of our population, the age of the majority of mothers is 30 years; and about three-fifths of the births are due to women of 30 years and under, and only two-fifths to women of 30 and upwards. This depends, however, upon the mothers being more numerous at that age, and does not imply that the age of 30 is the most prolific. On the contrary, when the productiveness of married women is considered, it is found that their fecundity (*i. e.*, the proportion of wives who are mothers to those who are not) is more than twice as great under 30 years of age as it is above it. And the fecundity of wives we find stated to be greatest in the first years of the child-bearing period (15 to 19 years of age); after which it gradually diminishes, declining with great rapidity after the age of 40, the natural limit of the child-bearing period being placed at 49.

But here a difficulty occurs. The fecundity of wives, as already mentioned, is said to be greater at the commencement than at any subsequent period of child-bearing life. Nevertheless, from the results of other tables, Dr Duncan tells us, that while this statement is true in regard to a mass of wives, it is not true if applied to the individuals forming that mass: "A different law governs individuals. Their fecundity is greatest from 20 to 25" (not from 15 to 19), "that is, a woman marrying at that age is more likely to demonstrate her fecundity" (*i. e.*, to bear a child), "than if she married at any other age."—(P. 23.) Or, to put it in another way, more wives are sterile from 15 to 19 than from 20 to 24, and consequently fewer can be fecund. This affords a very good example of the apparently conflicting conclusions which are not unfrequently drawn from statistical data, for nothing can seem more contradictory than to say that fecundity in the mass of wives is highest at the age of

15 to 19, and yet that the fecundity of individuals of that mass attains its maximum five years later (viz., 20 to 25). The greatness of the discrepancy is more fully brought out in the 68th table (p. 193), from which it appears that the amount of sterility in wives from 15 to 19 is as high as 7·3 per cent, or one in 13·72; while the sterility of wives from 20 to 24 is *nil:* every wife apparently bearing at least one child, and some more than one, since the number of first children is 1905, and the number of wives only 1835. Dr Duncan explains this difficulty by what he terms the law of intensity of fertility, that is, by the more rapid succession of births. "The comparatively greater intensity of fertility of fertile wives married from 15 to 20 over that of those married at from 20 to 25, does more than make up for the sterility of some of the younger. Though less fecund, they are more fertile as a mass." This explanation, although it is not worked out in detail, is very probably correct, especially when we take into account the larger proportion of wives beginning their married life, compared with those continuing it, in the first than in the second quinquenniad. The early years of wedded life are more apt to be prolific, and if the popular belief is not unfounded, not a few marriages among certain classes are contracted to conceal and legitimize the forthcoming effects of ante-nuptial intimacy. But the explanation points farther to the real source of the difficulty, which is, it appears to us, that in regard to the mass of wives, data proving fertility have been taken to prove fecundity. If we rightly understand the distinction drawn by the author, fecundity is simply the liability of married women to have issue, as proved by the proportion of those who become mothers to those who do not. A single birth, is all that is requisite to prove fecundity; and no more than one birth for each mother ought to be taken into account. On the other hand, fertility means the production of a number of children; it begins where fecundity ends. A woman is known to be fecund who bears one child; she is said to be fertile only when she has two or more. If these definitions are admitted, then it is apparent that in taking quinquennial periods, and comparing the number of legitimate births with the number of wives, since we do not exclude successive births, and therefore are not restricted to one birth for each individual, the results will show, not the fecundity, or liability of a certain proportion of wives to become mothers, but only the fertility, or number of times that the maternal condition has been presented by the subjects of the investigation, taken collectively within the period observed, during which many individuals have, no doubt, been several times mothers. The tables, which take notice of first births only, accordingly exhibit the fecundity more truly, and they show that the age of greatest fecundity (*i.e.*, of greatest liability to bear a living child), is 20 to 25, not 15 to 19. We submit, therefore, that if, instead of "comparative fecundity," the term, "comparative fertility of wives,"

had been substituted in the tables and conclusions of Chapter III., the seeming contradiction and difficulties just noticed would have been avoided. It is, further, not improbable that, although the quinquennial periods, 15 to 20 and 20 to 25, are numerically convenient, yet physiologically it might be more true to nature to place the years 15 (or say 13) to 17 in a separate category, as probably belonging to immaturity, while the period of maternal maturity should commence at 18. The results might thus be expected to present greater uniformity.

Although statements admitting of discussion sometimes occur, and indeed in regard to some questions the materials for a solution are confessedly wanting, yet, as a rule, the author's conclusions are fully borne out by the tables, which display much ability in their construction. Many of the results are curious. Thus we observe that the great prolificness of women from 20 to 25 years of age, compared with the period antecedent and those subsequent to it, finds a parallel in the case of the domestic fowl. According to Mr Geyelin, a hen during the whole course of her life cannot lay more eggs than 600, which in a natural course are distributed over nine years, in the following proportion; the rapid increase in the second year, the continued fertility of the third and fourth, and the rapid fall in the fifth year, being very remarkable:—

First year after birth,	. 15 to 20	Sixth year after birth,	. 50 to 60
Second ,,	. 100 ,, 120	Seventh ,,	. 35 ,, 40
Third ,,	. 120 ,, 135	Eighth ,,	. 15 ,, 20
Fourth ,,	. 100 ,, 115	Ninth ,,	. 1 ,, 10
Fifth ,,	. 60 ,, 80		

Dr Duncan finds that women married early and having issue continue longer productive than those married later. In regard to the rate at which mothers bear children, the conclusions arrived at are, that mothers bear their first children up to the third or fourth in more rapid succession than the subsequent ones onwards to the tenth; and that mothers having large families (*i.e.*, more than ten children) have their children more rapidly than other mothers during the whole fruitful period of their lives. "Wives, therefore, who bear numerous progeny do so in virtue of two differences from other women. They bear their children more rapidly, and they continue longer fertile than their neighbours."

A most interesting contribution to Dr Duncan's volume, though probably too strictly mathematical for the majority of readers, is found in Part VI., from the pen of Mr Tait, the distinguished Professor of Natural Philosophy in the University of Edinburgh, who, with the purpose of supplying some simple method of comparing the fertility of different races, has endeavoured to represent by formulæ some of the principal results of Dr Duncan's inquiries. Many of these results could not, with any advantage, be represented by formulæ, but some of the conditions of fecundity and fertility are shown, with surprising success, to admit of that mode of expression.

Professor Tait employs the terms fecundity and fertility in the following senses:—"By fecundity at a given age we mean the probability that during the lapse of one year of married life at that age, pregnancy producing a living child will ensue." "By fertility at any age, we mean the number of children which a married woman of that age is likely to have during the rest of her life, or some numerical multiple of it." The propositions which he deduces from his formulæ, are, that in the mass of women *fecundity is proportional to the number of years a woman's age is under fifty, and fertility is proportional to the square of the same number*. This, of course, assumes that the fecundity is greater from 15 to 19. The results of the calculations founded on these principles coincide with the results of Dr Duncan's table up to the age of 40, beyond which their agreement is less accurate. Another important result which Professor Tait has arrived at, is that early marriages hasten the advent of sterility, *i.e.*, of the time when mothers cease to bear children; while late marriages which, when fertile at all, are rapidly productive, tend to prolong the fruitful period, and so, to a certain extent, increase their prolificness.

For certain discrepancies, to some of which we have already alluded, in the tables of fecundity, fertility, and sterility when compared with each other, Professor Tait accounts by the known tendency of women to understate their age. "That this is no fancied explanation," he says, "may be gathered from the following scandalous facts recorded in the Census Report of 1851, p. 24:—

In 1841 the number of girls of ages 10–15, was . .	1,003,119
But in 1851 these had become young women aged 20–25, and numbered	1,030,456

This number, when corrected from the tables of mortality, obviously includes about 140,000 women whose ages had increased by less than 10 in ten years.

Again, in 1841 the number of women aged 20–25, was .	973,696
But in 1851 those who had reached 30–35, were only .	768,711

indicating, at first sight a fearful death-rate, but really showing how strong is the desire to be considered as remaining under the magic limit of thirty years of age." The same feminine source of error is seen in Dr Duncan's 5th table, in which the total women alive from 15 to 19 years of age are given at 31,538, and those at from 20 to 24, at 34,631. The propensity to misstate their age is ably turned to account by Dr Duncan in refuting Dr Granville's fanciful hypothesis of a decreased productive energy in mothers at the ages of 29 and 31 as a compensation for the exuberant prolificness at 30. It appears impossible at present to exclude this very considerable element of uncertainty. Perhaps, in the future, some statistician may arise to calculate exactly at what rate females grow younger after five-and-twenty.

The laws which Dr Duncan enunciates in regard to sterility, the occurrences of twins, the length and weight of the fœtus in relation to the age of the mother (in regard to which his observations have been for the most part confirmed by Professor Hecker of Munich), the mortality of childbed, the duration of pregnancy (a topic suggesting physiological difficulties which still await their solution), might all furnish ample material for comment; for the author contents himself with drawing only those conclusions which immediately result from his elaborated details, without pursuing them to their more remote relations and consequences, and without indulging in speculation. He has furnished data, on which others may build as well as himself. Within the limits assigned us, we have been able to give only a very inadequate idea of the rich mine of fact and inference which this volume contains, yet enough has been said to justify and explain our recommendation of it as a work replete with original and valuable information, the study of which is indispensable to all who are interested in the subjects of which it treats, or who are engaged in similar inquiries. We need hardly add that so far as accessories are concerned, the clearness and correctness of the printing and the general elegance of the volume leave nothing to be desired.

Clinical Notes on Uterine Surgery, with Special Reference to the Management of the Sterile Condition. By J. MARION SIMS, A.B., M.D., etc. London: 1866.

IN 1858, on receipt of Dr Sims' pamphlet on silver sutures in surgery, and especially on their use in the cure of vesico-vaginal fistula, we felt assured that, whatever faults there might be in the literary production, it announced a great discovery; and we immediately commended it to the notice of our surgical practitioners. We were not deceived. Dr Sims is the author of one of the most useful surgical discoveries of the century. This is true, even though he himself first announced it in almost the words we have used. We know that surgeons here and elsewhere may have criticism to make upon every point and peculiarity in Sims' proceeding; even his silver wire is by some declared no better than prepared silk. But the grand fact remains, a proud one for American surgery, that, whereas vesico-vaginal fistula was a disease which surgeons either did not attempt to heal or failed to heal if they did attempt it, now the very great majority of cases are healed by an irksome, but really a simple operation. Whereas, in Edinburgh, London, Dublin, and Paris, single cases of cure of vesico-vaginal fistula were paraded with ostentation as great works of great surgeons; now, all over the country, in villages and places where operations are rare, cases of cure are frequently observed. This painful condition, instead of being an opprobrium, is now a source of credit to the profession. This is the work of Marion Sims and of him alone. It is enough to hand his name down with honour to future generations.

Originally a practitioner in Alabama, Dr Sims there perfected his vesico-vaginal operation, finding a copious supply of submissive patients in the negro population, and perhaps it may be to the great power of endurance of pain possessed by the negro, that we owe a discovery whose fruits are valuable to white and negro equally. It is said that Sims made this profitable work, much in the same way as Astley Cooper is said to have made money by curing unsound horses. Sims is reported to have bought the sick and suffering negresses, cured them and resold them, so turning an honest penny. After perfecting his discovery, however, he made it profitable in another way, going to New York and enthusiastically working at the whole range of the diseases of females. Here he soon acquired an extensive and lucrative practice. He then set on foot a scheme for the establishment of a great woman's hospital in the city, and received a large amount of pecuniary support. But his designs were interrupted by the outbreak of the great American War. Coming from the south, he had warm sympathies with the southern cause. He longed for the establishment of the independence of the Confederate States. Of course, New York was, in slang phrase, too hot for him. He left America, and shortly began to practise in London and Paris. It is generally understood that in these capitals he enjoyed a considerable professional income. His exile is, we believe, now terminated, and we wish him returning prosperity in New York where we suppose he is again settled.

The present work is the fruit of the leisure afforded him by voluntary exile.

The first remark we would make on the book is, that it shows, as it might have been expected to do, that Sims is not a man of one idea or of ingenuity in only one limited direction. To be sure, he has made no discovery to be named in the same day with his operation for vesico-vaginal fistula, but he shows a fertility of resource in various surgical difficulties, and a skill in execution that have been never surpassed. It is a pity that he has not coped with ovariotomy. So far as he goes, he surpasses all his predecessors. Formerly, any one ingenious uterine contrivance attracted much attention, for a time at least. Here we have ingenuities by the bushel, very clever things, but they are each and all lost by being confounded with the numerous others. If any one wishes to study uterine manipulations, we recommend him to this volume. He will there find how best to do what he wishes to do. The experienced practitioner in diseases of women will there get many valuable directions and suggestions. But here we are stopped short by the previous question,—What should we do? much, little, or as little as possible. All these amounts have their able advocates. Sims is, we hesitate not to say, almost absurdly favourable to doing much; we might say, to doing everything by operative interference.

Practitioners in the diseases of women may be divided into two great classes; those who rely chiefly on constitutional treatment, and those who rely chiefly on surgical treatment. Among the former is found much weakness, vapidity, talk; among the latter, little philosophy, little wisdom, much boasting, and a little charlatanry. Some practitioners cure all dysmenorrhœa by an over-refined treatment, of which a little is for liver, a little for colon, a little for general health, all with a great deal of talk, which is safely guarded from incisive criticism by its indefiniteness and obscurity. Some practitioners, of whom Sims is one, declare all dysmenorrhœa to be mechanical, to arise from obstruction to the discharge of the menstrual fluid (forgetting that some of the most violent pains are before any is excreted, some in cases where none is excreted). Such treat with mechanical contrivances all their patients, young and elderly, virgin or not. It would be difficult to say whether of the two sets of therapeutists is the more unphilosophical, which the more unsuccessful. Of course, from the nature of the human mind, the dashing practitioner who, heedless of the risk of killing his patient, or nearly so, in the attempt to cure perhaps a trivial malady, splits open the cervix of each sufferer whether there be mechanical obstacle or not,—such practitioner is sure to be the popular or fashionable man. In spite of this we urge the proverb,—*in medio tutissimus ibis.*

We do not mean to enter into a detailed criticism of Sims' book, and have referred to the subject of dysmenorrhœa merely to illustrate the great fault of the work. The author has still to learn the constitutional aspect of women's diseases and their constitutional treatment. If he is to make the best kind of progress, which we are sure he desires, he must henceforth study his failures. He must force himself to look intently at the success of his mechanical treatment forming the demonstration of his own error, the mechanical success not proving a therapeutical success. He must candidly study the treatment without surgery, and recognise its proper amount of success. He must retract a great deal of his physiological and pathological dogmatism on topics which are far from being arrived at such a stage of advancement as to admit of security of conclusions.

Dr Sims' work is greatly devoted to the cure of sterility, a subject which is being somewhat prematurely snatched from the possession of quacks. Enough has been done to admit of the matter being treated by an enlightened practitioner such as Sims. But light is on this topic so far surpassed by the power of darkness, that we think it better, even now, to leave the name at least of "curing sterility" to quackery. Such slip-shod writers as Dr Gardner of America may consistently write a book on it, but not such as Dr Sims. The latter indeed throws himself open to much criticism on his treatment of this subject; and fearing the power of darkness we shall not follow him a single step into the field. We must have much more diffused light over the ground, to make it possible for men of wisdom to announce cures. Let all interested in the diseases of women struggle to add to this light, by the ascertainment and publication of facts bearing upon it. This we say, without wishing to imply that such safe means of supposed curing as are now in vogue should be hastily, if at all, laid aside.

The Nervous System. By LUDOVIC HIRSCHFELD, Professor of Anatomy to the Faculty of Medicine of Warsaw. Edited in English by ALEXANDER MASSON MACDOUGAL, F.R.C.S., with artistically coloured Lithographic Illustrations, designed by J. B. Léveillé. Authorized Translation. To be completed in twelve parts. Part I. London: Churchill and Sons.

WE observe with pleasure the appearance of an English edition of these justly celebrated anatomical plates of the nervous system. The publication is particularly opportune at the present time, when the attention of physicians is specially directed by recent discoveries to the lesions and disorders both of the cerebro-spinal and sympathetic systems. We trust the work will receive the encouragement it deserves. The first part is excellent; we shall be glad to find the remaining ones appearing at intervals sufficiently near to preserve the continuity of the work.

Lake Habitations and Pre-Historic Remains in Italy. By BARTOLOMEO GASTALDI. Translated for the Anthropological Society of London, by C. H. CHAMBERS. London: 1865.

IN this memoir Signor Gastaldi of Turin has collected and arranged a number of facts concerning the early inhabitants of Italy, which have recently been brought to light by investigations into the turbaries and marl-beds of Northern and Central Italy. The work is illustrated by numerous woodcuts. Those of our readers who are interested in the progress of archæology will find in this book a readable account of the habitations, weapons, and utensils of the primitive inhabitants of Southern Europe.

Part Third.

MEETINGS OF SOCIETIES.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XLVI.—MEETING I.

7th November 1866.—Dr Moir, *President*, in the Chair.

I. EXCISION OF THE ANKLE-JOINT.

Dr Watson exhibited the astragalus, along with the lower epiphyses of the tibia and fibula, which he had two days before removed from a child. He effected their removal from the *inner* side, having extended by incisions two apertures which existed upon the inner and outer side of the ankle-joint.

II. URINARY CALCULUS FROM A FEMALE.

Dr Watson exhibited a calculus which he had recently removed from a lady. The operation was effected by instantaneous dilatation of the urethra and neck of the bladder by a three-bladed urethra dilator, commonly called Weiss's dilator. The calculus, composed of phosphates upon the surface, and oxalate of lime internally, was of the size of an ordinary walnut. The degree of dilatation practised merely admitted the point of the forefinger into the bladder. The stone was seized by means of bullet forceps and instantly extracted, requiring for this purpose but slight effort. The urine-retaining function of the bladder was restored in a few days.

III. EXCISION OF THE KNEE-JOINT.

Dr Watson showed the articulating ends of the femur and tibia entering into the formation of the knee-joint, which he had this day removed by excision. The patient, an adult, had long suffered from disease of the knee, manifesting the characters of white-swelling or synovial degeneration. The invasion of the cartilaginous textures had slowly advanced, but left so much of the condyles still coated with their cartilage of incrustation that the patient could move the knee up to the time of operation. He was, however, becoming more and more exhausted by copious suppurative discharge coming from an opening a little above the level of the external condyle. The suppuration was also extending upon the anterior aspect of the thigh under cover of the muscles. The case was, immediately after the operation, put up in the manner he usually adopted by a suspension rod and posterior splint.

IV. CASE OF TRAUMATIC TETANUS SUCCESSFULLY TREATED BY THE LOCAL APPLICATION OF INFUSION OF TOBACCO.

The Secretary read a paper on the above subject by Dr Junor of Peebles. It will be published in an early number of this Journal.

V. ON THE AFTER-TREATMENT OF EXCISION OF THE KNEE-JOINT.

Dr Watson read the first part of a paper on this subject. The discussion was adjourned until the concluding portion should have been communicated.

SESSION XLVI.—MEETING II.

5th December 1866.—Dr Moir, *President*, in the Chair.

I. FIBROUS POLYPUS UTERI.

Dr Matthews Duncan showed a true fibrous polypus uteri which he had that day excised from a patient of Dr Ferguson of Leslie. It had caused persistent dribbling hæmorrhage for a long time. Dr D. removed it by curved scissors.

Bleeding after the removal was inconsiderable. It measured three inches in length, and two and three-quarter inches in breadth. It was a true fibrous polypus, the fibrous tumour composing it having a complete muscular and mucous tunic. When removing it, Dr D. thought he had to do with a cyst. Fluid flowed freely from the punctures made by the volsella. This appearance was produced by extreme œdema of the polypus, and especially of the cellular investment of the fibrous tumour. At the lower part of the polypus, the cellular envelope was so distended by œdema as to be an inch in thickness; the fluid lying in the cellular interspaces and separating the tumour from its muscular envelope to the extent above described. Dr D. called this a true fibrous polypus to distinguish it from a false fibrous polypus or enucleated fibrous tumour, having no muscular or mucous investment. The variations of the œdema probably formed the explanation of Dr Ferguson's observation of the varying size of the polypus.

II. URINARY CALCULI REMOVED BY LITHOTRITY AND LITHOTOMY.

Dr Watson exhibited two urinary calculi which he had removed from two patients since the last meeting of the Society. The one he removed by lithotrity, in an adult patient from Caithness, who had suffered from symptoms of stone for several years. The debris collected weighed three drachms and a-half, and consisted of lithates and lithic acid. The other case was that of a lad of ten years of age, from Perthshire, who from birth had suffered constantly and severely from symptoms of stone. The calculus, incrusted with phosphates, weighing four drachms and twenty grains, was removed by the ordinary lateral operation. Both patients had gone home, having recovered without a single bad symptom. Dr Watson stated that in adult cases, where the stone was of moderate size, he had of late uniformly practised lithotrity.

III. CASE OF CLEFT PALATE, PECULIARITY OF POSTERIOR PILLAR OF THE FAUCES ON THE RIGHT SIDE.

Dr Watson showed a patient upon whom he had lately operated for cleft palate. The operation was performed according to Langenbeck's method. The result was perfectly satisfactory. The sutures were removed upon the second day after the operation. Dr Watson showed this case, not so much on account of the operation or its result, as on account of a peculiar malformation of one of the palatal muscles on the right side. This consisted in the complete separation of the palato-pharyngeus from the pharynx throughout its whole extent, except where the lower end became continuous with the pharyngeal walls. The posterior pillar of the palate was thus truly a *pillar* and not a *pilaster* as in the normal condition of the parts. Dr Watson showed Langenbeck's needle for suture of the cleft palate, and stated that he had found it infinitely to be preferred to every other designed for the introduction of sutures in this operation, or in that of vesico-vaginal fistula. Dr Watson stated, that he had operated in several cases of cleft palate in young children with satisfactory success, the children being, of course, under the influence of chloroform during the whole operation.

IV. ON THE NATURE, SOURCE, AND SURGICAL TREATMENT OF THE BLEEDING FROM FIBROUS UTERINE TUMOURS.

Dr Matthews Duncan read a paper on this subject, the first part of which appears in the present number of this Journal.

V. *Dr Watson*, in order to allow the reading of the other papers on the list, offered to postpone the second part of his communication on excision of the knee-joint till next meeting.

VI. CASE OF UNITED FRACTURE OF THE ULNA.

This case, by Dr John Duncan, will appear in our next number.

VII. ON THE FŒTAL CIRCULATION.

Professor Macdonald, of St Andrews, communicated to the Society his views on the above subject.

PROCEEDINGS OF THE EDINBURGH OBSTETRICAL SOCIETY.

SESSION XXVI.—MEETING I.

14*th November* 1866.—Dr BURN, *Vice-President*, in the Chair.

I. CASE OF TEDIOUS LABOUR. BY DR PATTISON.

5*th October* 1866.—Mrs T., æt. 21—first child.—I was sent for early this morning. On my arrival, Mrs T. was complaining of severe pain in the back, returning regularly every ten minutes. She had no abdominal pain, nor did the uterus contract on applying my hand to the abdomen. On examination, the os was felt far back, barely open to the size of a threepenny-piece; the lips thick; the head of the fœtus high up; presenting in first position; not pressing on the os; no bag of membranes. On further inquiry, I was told she had passed more water than usual early in the morning. On my return, four hours afterwards, the os felt thinner, the parts cool and soft; she complained of agonizing pains in the back, recurring every ten minutes, but there was no advance of the head. In the evening, she was in much the same state.

6*th*.—This morning I found her still in much the same state; no sleep; the same agonizing pain in the back; no advance of the head; no abdominal pain; frequent vomiting; no headache; pulse natural; the liquor amnii gradually escaping, which I encouraged by pushing my finger into the os to allow it to come off. Finding her in the evening in much the same state, I determined to stop the pains, and gave her forty drops of Batley, to be repeated in four hours, and a suppository, containing half a grain of morphia.

7*th*.—I was called about five this morning. The pains of the same character; no sleep; frequent vomiting; considerable thirst; she feels exhausted from the want of sleep and other symptoms; the os about the size of a sixpence, thinner. I dilated with my finger; passed Keiller's dilator, and got the os to the size of a crown; the head slightly advancing. All this morning she had been under chloroform. On coming out of it, the vomiting returned, with thirst and general uneasiness—feeling anxious about herself. During the day, hot flannels, and cloths wrung out of hot water, were applied to the back, with little or no relief. As the os was in the same state at nine P.M. as in the morning, I asked Dr Keiller to see her, which he kindly did about eleven P.M. She had been sitting over steam; injections used; the bowels freely opened; she could make water with some difficulty. She was vomiting a dark coloured liquid, not exactly black vomit; relieved by ice. Dr Keiller examined and found the os about the size of a large crown-piece; the head advancing slightly. He did not consider it necessary to interfere, but advised delay, as there was no unfavourable symptom. Dr Keiller advised another suppository, and said he thought, in the course of the night, the pains might come round to the uterus; if we interfered, the uterus might not contract well, as it was now acting very irregularly.

8*th*.—The pains were more abdominal this morning, though still very severe in the back. She had no sleep, and no relief to the pains; os thinner, and more dilated, with the head pressing on it; liquor amnii coming away; the parts cool and soft; vomiting ceased; great thirst, with considerable exhaustion; the head acting on the os. About half-past three P.M., as the head was slowly advancing—nearly coming out of the uterus—when abdominal pressure was of great use, I gave a large dose of ergot which increased the pains. At four o'clock, with abdominal pressure, I increased the action of the uterus—the patient using all her power. I forced forward the head, and ultimately the child was expelled dead, about a quarter to five P.M. I removed the placenta in a quarter of an hour afterwards, which was pale, flabby, and discoloured, with an intolerable stench, and contained a clot of blood in the centre.

This tedious case lasted fully 108 hours, from the escape of the waters, and the commencement of pain in the back. I never saw in my practice such long-continued agonizing pains in the back, with no relief from the opiates, suppositories, steam, injections, chloroform. The latter, indeed, was so far useful, as it was only when under its influence that the patient had relief, and sleep, if it may be called so, during the long continuance of the case, but it produced no other benefit. The child (a female) weighed eight pounds twelve ounces; length, twenty-three inches. Could the weight of the child (the placenta was rather smaller than usual) have any effect in causing reflex action on the spinal nerves, and thus cause the severity of the pains in the back? The vomiting had no effect on the os, though it lasted for forty-eight hours, neither had the opium. Perhaps some will think they assisted, though at the time I did not.

I have observed in three cases, where the patients were past thirty-seven, thirty-eight, and forty-one, that if the waters broke, three days elapsed before the child was born without instruments, and I would ask if any one can corroborate my statement; and on referring to some of my cases where premature labour was induced if the waters escaped, three days commonly elapsed before the child was born. Had the placenta separated before the birth of the child, or was it the clot of blood in the centre that had undergone decomposition, causing the stench? The cord was dark, flabby, and had a very unhealthy appearance. How long does it take for the placenta to undergo decomposition within the uterus?

Dr Keiller remarked that the case reported by Dr Pattison was one of considerable interest, for although it was by no means uncommon for practitioners to fall in with cases unusually prolonged, and annoyingly tedious in the first stage, still the cause of the delay was generally to be accounted for, either in the rigidity of the os, or other condition interfering with the dilatation of the passages, or some morbid and inefficient action in the expulsive powers. The peculiarity of this case, as had been stated by Dr Pattison, was the extreme severity of the pain in the back during so long a period. When he (Dr Keiller) saw the patient, along with Dr Pattison, the comparative absence of abdominal pains, and the slight degree of dilatation in the os uteri, seemed to indicate non-interference, and the administration of opiates to allay the restlessness, and apparently useless pains which the patient suffered from. Dr Keiller did not afterwards see the case.

The early rupture of the membranes often proves unfortunate, as, besides being a cause of tedious first stage, it often leads the patient to suppose that labour has commenced, and leads her to prematurely use bearing down expulsive efforts. He (Dr Keiller) had often seen the membranes give way long before any symptoms of labour commenced, and yet no bad results take place; and it occasionally happened that a double bag existed, and rupture of one layer, with discharge of its fluid contents, might take place long before the rupture and expulsion of the amniotic fluid usually preceding labour. Had the case now reported occurred in less experienced hands, it might have led to the supposition that the patient was allowed to, as it were, go too soon into labour (which, of course, was not the case in the present instance), and that the anxiety and fruitless bearing-down efforts of the patient in the tedious first stage, had greatly tended to the exhaustive delay.

Dr Cochrane suggested the separation of the placenta as the cause of the death of the child.

Dr Burn thought such a cause was not very common, but when it did happen, it was the occasion of great anxiety. He mentioned the case of a patient who had been ill from Friday till the following Tuesday, when the forceps were applied to the head at the brim, but without avail. Craniotomy was performed, and the patient recovered.

Dr Archibald, of St Andrews, mentioned two cases. The first was that of a second labour, where the head made no progress, and he turned to save time and suffering; but the child was dead; in this patient, craniotomy had on a former occasion been performed. The other was a first child. The woman was young, and, after a delay of twelve hours, as there was little progress, the forceps were applied, but the child was dead, and the mother died in twelve hours thereafter from exhaustion. Dr Archibald advocated earlier interference in such cases, and had frequently regretted delay.

Dr Bryce mentioned a case of a first child which he was called to see at two A.M., when the os was about the size of a sixpence, and firm. The pains were severe. He ordered a dose of tart. of antimony, and left. The child was born in twenty minutes.

Dr Andrew Inglis referred to a similar case, in which, after the patient had been ill seventy hours, the *finger* only could be admitted into the os. During a severe pain the os split up, and the child was born immediately. The mother had a second child in thirteen months.

II. CASE OF CHOLERA IN A PREGNANT FEMALE. BY DR CHARLES BELL.

This case occurred many years ago, and long after cholera had ceased to be epidemic, and when there were no cases of the disease in the country. I have been induced to bring it before the Society, not only because it may be interesting at this time, as showing that cholera may occur sporadically, without extending itself by infecting those around, and thus diminish the dread of its contagious character, but because it presents some important facts in an obstetric point of view.

On the 16th May, I was requested, between nine and ten P.M., to visit Mrs Ramsay, aged thirty-six, residing in a miserably dirty apartment, No. 11 Leith Wynd. I found her in a very exhausted state, and quite pulseless at the wrist. Her hands were deadly cold, but the temperature of her body was nearly natural. Her eyes were sunk, and surrounded by a deep purple areola. Her whole countenance had a peculiar yellowish hue, which was more marked round the mouth. She stated that she had suffered from diarrhœa for some days, but that it ceased four days previously, when vomiting came on, which had continued ever since, and had been particularly severe all day; she was sure she had "vomited a hundred times since morning;" she had excruciating cramps in her limbs for several days; and she was then suffering from severe pain in the right side of her stomach. She had great thirst, but whenever she drank anything she immediately vomited. She had not passed water since early in the morning.

It was too late to get any medicine; she was, therefore, merely ordered to have castor-oil, and to have bottles of hot water applied to her feet.

On making an early visit the following forenoon, I was told that the poor woman had died at seven A.M.

Examination of the body three days after death.—There was a small quantity of dark-coloured fluid in the cavity of the chest, but the lungs were healthy, and natural in appearance. The heart was of the normal size, and its valves and muscular structure healthy; but on its surface, and that of the large arteries, there was several ecchymosed spots, varying from the size of pin points, to that of fish scales, which were similar to what were found on almost every body examined in the cholera hospital in 1831. The stomach was large, and, when cut open, it was found to contain a considerable quantity of dark fluid like hare-soup, on the surface of which there floated an oily substance, in large spots. Towards the cardiac orifice, there was a large ecchymosis, which extended several inches up the œsophagus. There were vascular patches in other parts of the lining membrane, which was very soft, and could be easily pealed off by the nail. There was no thickening of the muscular coat. The liver was paler than natural, but there was no other peculiarity. The gall-bladder contained a considerable quantity of bile. The intestines looked healthy, but their contents were pale, and watery. The kidneys were of the natural size, but on one of them there was a dark spot, like an eschar; and on this part, and around it, there were numerous small white tubercles, like drops of pus, which were as hard as calculi. There was nothing remarkable observed on cutting into the body of the organ. The uterus was large, and reached to within an inch and a-half of the umbilicus. It presented nothing abnormal on its surface. On laying it open, a fœtus was found, apparently in the sixth month of gestation, which corresponded with the woman's supposed period of pregnancy. The quantity of liquor amnii was large, but in other respects it was natural. The membranes were distinct, and could be easily separated. The placenta was readily detached, and its uterine surface presented numerous small orifices, with defined edges, filled with clots. The orifices were most numerous opposite the insertion of the cord, and they corresponded with the small tortuous vessels on the surface of the uterus, which also contained coagula. Besides these small orifices, there were others of a larger size, with equally well defined edges, which contained no coagula. The walls of the uterus towards the fundus were much thicker and stronger than near the cervix. The os was dilated to the size of half-a-crown, and its lips were obtuse. On the inner surface of the cervix there were numerous yellow vesicles, containing a viscid fluid. Most of these vesicles were not larger than pin heads, but some of them were as large as peas. The bladder was much contracted, and contained no urine. Its inner surface was covered with a secretion similar to that secreted in leucorrhœa.

In an obstetric point of view, this case is remarkable as showing that the os uteri may be dilated to a considerable extent, and there may be violent vomiting and purging without abortion taking place. It may be said, however, that the dilatation of the os uteri was nothing more than what is usually met with after death; but I have observed it where cholera did not prove fatal, as well as in other instances.

Dr Murray alluded to one case of cholera which he had attended where the patient was eight months pregnant, and no miscarriage occurred.

Dr S. Smith mentioned two cases of cholera in pregnant women,—one at seven, and one at eight months—where both recovered without any uterine action coming on.

Dr Keiller said he believed that females were more liable to cholera during the menstrual period than at other times, and stated that the late Dr John Mackintosh had been struck with the comparative frequency of cholera seizures in menstruating females during the time he had charge of the Cholera Hospital in Drummond Street. He (Dr Keiller) thought this great liability of women while menstruating, might be accounted for by the fact of their comparatively lax condition of bowels at that time.

In reference to this point, *Dr Smith* said that, among other cases of cholera which he had seen, some had been preceded by miscarriage or menstruation, and one case occurred three days after confinement.

Dr Stephenson thought it might be questioned whether this was really a case of cholera. The kidneys were evidently diseased, and all the symptoms, however much they similated cholera, could be produced by other blood poisons than cholera.

Dr Bell said that the symptoms during life, as well as the appearances on dissection, must clearly prove to every one who has had experience in the disease, that it was a decided case of cholera.

Dr Bryce said that real Asiatic cholera might occur sporadically.

Part Fourth.

PERISCOPE.

UTERINE SURGERY.

SUCCESSFUL REMOVAL OF A FIBROID TUMOUR OF THE UTERUS WEIGHING 4500 GRAMMES (9 LBS. 1 OZ. AV.), BY M. KOEBERLÉ OF STRASBOURG.

MADEMOISELLE P., from Cuba, thirty-seven years of age, had suffered for five years from an abdominal tumour, which had gradually increased in size, and which, for the last two years, had become complicated by ascites. Eighteen months before, she had an attack of peritonitis. Six months after this, she had inflammation of the cellular tissue in the left iliac fossa, which went on to suppuration. An opening was made, and for several days pus and fæcal matter escaped from the wound. At this stage of her illness, the abdominal tumour was looked upon by MM. Richard, Demarquay, Nélaton, and others, as an ovarian cyst. M. Velpeau, on the other hand, regarded it as a fibrous tumour of the uterus. My first impression also was, that the tumour was a fibroid, on account of its consistence, its irregular form, and its history; but I thought it better to give no decided opinion till after a preliminary tapping. On account of the great distention of the abdomen, it was difficult to make a proper examination of the tumour, though it presented many of the characteristics of ovarian disease. The cervix uteri was normal, small, central, slightly inclined to the left; the pelvic cavity was entirely free. Menstruation was regular; the menses for some time after the severe peritonitic attack had disappeared, but lately they had again become normal. After each menstrual period the ascitic fluid rapidly increased, and then diminished during eight to ten days. This circumstance I have often noticed as symptomatic of fibrous tumours complicated by ascites. During this period of increase of size of the abdomen, the urine became scanty, and thirst was distressing; as the size diminished, the urine became copious, and the thirst disappeared. Of late, however, the decrease of size had become less and less marked after these menstrual periods. The circumference at the umbilicus was forty-six inches. Nearly twenty pints of ascitic fluid were removed by tapping, and the size of the abdomen was reduced to thirty-nine inches. It was now possible to make a more correct examination of the tumour. It was irregular, of varying consistence, soft at some places, hard at others; nowhere was any bruit to be heard; there was an indistinct fluctuation; it was very movable, and one could move it backwards and forwards above the pubes for two and a-half to three and a-half inches without sensibly affecting the cervix uteri, or without the patient complaining of any drag in the genital canal. On examination above the pubes, one could likewise feel a sort of resistance on the left side, deep in the pelvis, as if the tumour arose from the left ovary.

Notwithstanding some peculiarities which appeared to indicate that the tumour might be a fibroid arising from the fundus of the uterus, the impressions left on my mind were, fortunately for the patient, rather in favour of its being a multilocular cyst of the ovary. To have tapped the tumour would certainly have cleared up the diagnosis; but as the probability was in favour of its being an ovarian tumour, I did not wish to put my patient to this additional risk. And it is to this error of diagnosis on my part, that Mademoiselle P. owes her recovery; for I would not have undertaken the removal of a uterine fibroid, complicated by ascites, peritonitis, and suppuration in the iliac fossa, with intestinal perforation.

There was no abdominal adhesion, and on the supposition that the tumour was ovarian, the intestinal adhesions which one would expect to meet with, did not appear to me to contra-indicate operation, especially as the pedicle seemed to be very long. The ascitic fluid was dependent on the presence of the tumour, and would disappear with it. The general health of the patient was very good, and though thin and of a delicate constitution, she could easily bear a considerable loss of blood, should adhesions be present.

The operation was performed on the 7th of July 1866. An incision was made from the umbilicus to the pubes. After some ascitic fluid had escaped, I was able to recognise the true nature of the tumour and to explore, by the hand, its connexions. It was composed of interstitial fibroids developed in the fundus of the uterus; it was adherent on the left side, over a considerable extent to the brim and cavity of the pelvis. The cervix uteri was very much elongated. It was very difficult to reach this adhesion, which was about the size of the surface of the hand. I managed, however, by dividing with the bistoury, the scissors, or fingers, the portions of it which contained no vessel of any size, to reduce it to near the thickness of three fingers, so as to enable me to surround it with a metallic ligature, and secure the important vessels which it contained. This ligature was then tightened by a serre-nœud, four inches in length. The cervix uteri was also secured by another metallic ligature, and as the ovaries and Fallopian tubes were healthy, they were not included in this ligature. The tumour was then cut away, leaving sufficient tissue to prevent the ligatures from slipping. The serre-nœuds were tightened till all bleeding ceased. The ligature round the uterus answered very well. The serre-nœud embraced tissue not more than the thickness of the finger, and it dipped into the peritoneal cavity only about an inch and a-half. It was otherwise, however, with the ligature round the pelvic adhesion; there, the instrument dipped more than three inches into the pelvic cavity, and it was difficult to arrange the serre-nœud without seriously interfering with the cicatrization of the wound, and I had to take my choice between removing it and cutting off the ligatures short or using the cautery. It was thought safer to divide this pelvic adhesion by the actual cautery. This was accomplished by the careful use of small cautery irons, and the serre-nœud was thus removed without any hæmorrhage taking place. The intestines were carefully protected by the hand and sponges from receiving any injury during the cauterization of the adhesion, which presented many difficulties on account of the depth of the parts concerned, and the little space in which there was to work. The pelvic cavity was then sponged, care being taken not to interfere with the cauterized portions. Little blood had been lost, and the wound was closed by five deep and nine superficial sutures. A glass tube, four inches in length, was placed in the pelvic cavity behind the neck of the uterus, so as to give free exit to any serous accumulation The portion of the uterus beyond the ligature was transfixed in three points by transverse needles, so as to keep the parts in apposition to the abdominal wall. No dressing was applied. The operation lasted an hour and a-half.

It is sufficient to add that the patient recovered in three weeks without the slighest unfavourable symptom. This is the sixth case in which M. Koeberlé has removed the uterus,—of these three recovered. In the three fatal cases death was caused by hæmorrhage.—*Abridged from Gaz. Hebdom.*, 23*d November* 1866.

PATHOLOGY.

A CASE OF GENERAL OCHRONOSIS OF THE CARTILAGINOUS AND CARTILAGINOID STRUCTURES. BY RUDOLPH VIRCHOW.

A MAN, æt. 67, died in the Charité Hospital, Berlin, a few days after admission on account of an injury of the head. He had been suffering from general dropsy, with ascites and hydrothorax, and was found on post-mortem examination to have an aortic aneurism. On preparing to open the body, the costal cartilages were observed to be black, and on section they appeared as if they had been steeped in ink. The same appearance was presented by the sterno-clavicular cartilages, the intervertebral fibro-cartilages, the pelvic synchondroses, the articular cartilages of the extremities, and the cartilages of the larynx and trachea. In all, excepting the articular cartilages of the extremities, the colour was quite black, in them it was brown or smoky. In all the cartilages connected with bone, the colour was most intense next to the bone, and in the layer immediately beneath the perichondrium. In the laryngeal and tracheal cartilages, it was most intense in the centre, but the superficial layers had ossified. On microscopic examination, no black granules were to be seen, only brown and yellowish, such as are commonly met with in melanotic tumours. The pigment was generally distributed throughout the structure, but was by far most abundant in the intercellular substance, little existing in the capsules of the cartilage cells, and still less in the cells themselves.

This peculiar appearance certainly was not a result of decomposition; had it been so, the colour would have been most distinct on the surface, which was not the case. At first sight, Virchow thought it might be a result of impregnation with salts of silver. He was in so far confirmed in this view, by the fact, that in many of the fibrous structures, ligaments, and tendons, in the deeper layers of the periosteum, and in the inner coat of the arteries, particularly the aorta, the same discoloration existed; and that in many of the joints, small black processes, in groups or solitary, resembling crystals, projected from the synovial membrane. But these little crystal-like bodies proved to be minute cartilaginous outgrowths from the synovial membrane, coloured like the natural cartilages; and there was no history of the patient having taken a course of silver; and most important of all, Dr Kühne found, on chemical examination, that no silver was present, and that the only metal to be detected was iron; that the colour depended upon the presence of an organic-colouring matter, which somewhat resembled hæmatin. From these facts and considerations, Professor Virchow concluded that he had to do with a vital pathological phenomenon, in so far related to the pigmentary deposition in the bronzed skin of morbus Addisonii. In this case there was no disease of the supra-renal capsules, and the only important point in addition to what has been mentioned was that in all parts, in which pigmentary deposit existed, the tissues were in a state of formative irritation.

Virchow concludes his paper as follows:—"I shall, for brevity, call the the pigmentary condition of the parts Ochronosis. If it be referable to an imbibition from the blood, and occurs chiefly in non-vascular and nerveless parts, which are, however, in a state of formative irritation, the question arises whether it has no physiological analogue. It seems to me that it has. The ordinary pigment formation in the skin, the hair, and the choroid appears to depend upon a similar process (see Archiv, vol. i. pp. 468, 469). The formation of ephelides, etc., has no slight resemblance to the peculiar condition under consideration. The colour is distributed in large patches, and so may be considered related to bronzed skin. But there are also in cartilages and ligaments conditions which may be regarded as minor degrees of ochronosis. I long ago pointed out, that in old people the semilunar cartilages of the knee-joint become dark yellow or brown, and there is nothing easier than to find deep yellowish-brown discolorations in the bronchial and costal cartilages. The intercellular substance always contains the colouring matter, which is quite homogeneous and diffused, and which must be distinguished from the much rarer granular and reddish-brown colour sometimes met with in the cartilage cells. I am of opinion, therefore, that the present case was only an example of this more common ochronosis much more pronounced than usual."—*Virchow's Archiv, October* 1866.

THERAPEUTICS.

THE ADMINISTRATION OF COD-LIVER OIL.

Dr Ludovic Rouland proposes the following formula for disguising the taste of cod-liver oil:—Of the oil, 100 parts; alcohol, of the density ·9396 (*i.e.* rather weaker than proof spirit), 60 parts; essence of peppermint, 3 parts. These form an emulsion which may be given in tablespoonful doses thrice a-day. Dr Rouland has succeeded in giving the oil in this form in cases in which it was previously intolerable. But in certain cases which have resisted the oil even in this form, he has employed the following substitute with advantage:—almond oil, 60 parts; cacao butter, 3 parts; alcohol, of the density ·9396, 30 parts; syrup of fir-tops, 40 parts; tincture of orange-peel and phosphate of lime, of each 5 parts. Hypophosphite of lime may be used instead of the phosphate.—*Abeille Medicale.*

DECOCTION OF SAGE IN PROFUSE SWEATING.

M. Vignard, of Nantes, after the manner of Van Swieten, has successfully employed decoction of sage for the relief of profuse sweating. In the case of a man twenty-five years of age, who had suffered, from time to time, during many years from attacks of this kind, the remedy proved effectual. The sweating began suddenly between two and three o'clock in the morning all over the body, and was so profuse as to completely saturate the bed-clothes and to a considerable extent the mattress also. In consequence of the regularity of the attacks, sulphate of quinine was tried as an antiperiodic, but unavailingly; the perspiration regularly reappeared, and without any apparent pathological cause. At length, M. Vignard prescribed the following preparation:—Take of chopped sage leaves, a large teaspoonful (*une forte pincée*); of water, six fluid ounces. Boil the sage for a minute or two in the water; let it stand to cool, then filter and sweeten to taste. From that time the perspiration ceased whenever the decoction was taken, but reappeared when it was omitted. M. Vignard suggests the use of this remedy in the colliquative sweating of phthisis.—*Journal de Médicine de Nantes.*

ON THE ANTISEPTIC PROPERTIES OF THE HYPOSULPHITES.

Dr Constantin Paul, who has previously written upon the physiological and therapeutical properties of the sulphites and hyposulphites, contributes to the *Bulletin de Thérapeutique* two facts relative to the disinfectant properties of the hyposulphites.

First fact:—A patient suffering from severe dysentery arrived in Paris in October 1865, during the prevalence of cholera. He inhabited a healthy locality, in which there had been no cholera. Some days after his arrival, the chambermaid took cholera and died in twelve days; after her several other servants were seized with cholera, but none died. Believing that this outbreak was due to the extremely offensive evacuations of his dysenteric patient, Dr Paul employed a solution of hyposulphite of soda as a disinfectant. To the great comfort of those in attendance, the fetid odour from that moment disappeared. Not content with this amelioration, however, Dr Paul sought to disinfect the offending matter before it left the intestine, for which purpose he employed the solution in the form of enemata. The effect of this was not only to destroy the odour of the evacuations, but also to give considerable relief to the patient, the intestine becoming less painful and defœcation less irksome.

Second fact:—Dr Paul considering that the lochiæ may be a fruitful cause of of puerperal diseases, resolved to try the disinfectant action of hyposulphite of soda as a preventive of these distressing obstetric sequelæ. For this purpose, he sprinkled the napkins to be applied to the parts with solution of the hyposulphite, and found that by that means the disagreeable odour which attends the accouchement-chamber was completely dispelled; and he believes that not only a disagreeable but a dangerous condition of the atmosphere of the apartment may thus be obviated. Dr Paul states, that far from causing irritation of the parts, the solution has rather anæsthetic properties, and, moreover, that wounds previously dressed with it become much less sensitive and less painful when touched.

SUB-NITRATE OF BISMUTH IN THE TREATMENT OF EPIDEMIC DYSENTERY OF HOT COUNTRIES.

M. Brassac, of the French navy, having had an opportunity of treating dysentery upon M. Monneret's plan of large doses of bismuth, speaks of the results with great satisfaction. Beginning with 15 to 20 grammes (231 to 308 grains) a-day the quantity was rapidly increased to from 60 to 70 grammes (926 to 1080 grains), by the rate of ten grammes a-day, proportioning the quantity to the presumed gravity of the case, and being guided by the effects of the first doses. When convalescence was established, the bismuth was gradually diminished, moderate quantities being continued for fifteen or twenty days even after the patient had been brought to one stool *per diem*. This precaution of continuing the treatment after the establishment of convalescence is strongly enforced. M. Brassac never prescribed the bismuth in small doses, which he believes to be injurious, and, moreover, he attributes the small success of his colleagues, as compared with his own, to their timidity in the use of the drug. The quantity prescribed for the day was divided into not more than five doses, given in unleavened bread, or in broth or rice-water, or mixed with other suitable articles of diet. He found that after a few doses the patients were able to take such articles of diet as before the administration of the bismuth they dared not touch. Bismuth was also in some cases applied by enemata.—*Bulletin Général de Thérapeutique.*

THE PHYSIOLOGICAL AND THERAPEUTICAL ACTIONS OF CURARA.

Dr Auguste Voisin of the Bicêtre, and M. Henry Liouville, *interne* of the hospitals of Paris, have recently communicated to the Institute (*Académie des Sciences*) an interesting paper descriptive of their experimental and clinical studies of the curara poison. Following in the footsteps of Fontana, Claude Bernard, Pelikan, Kölliker, Vulpian, Kühne, Thiercelin, Benedickt, and others, they have carefully investigated the subject up to a certain point, and their labours are calculated to encourage a still further research into the medicinal action and therapeutical value of energetic poisonous substances. [We shall return to this subject, and speak of it more fully in a future number; at present, we purpose only to give a summary of the important results obtained by MM. Voisin and Liouville.]

Four samples of curara were placed at their disposal, two in calabashes, the others in small earthen pots; the two former supplied by M. Claude Bernard, himself a distinguished pioneer in the investigation; the third, by M. Carrey, a traveller and author of distinction; and the fourth, purchased from M. Menier, a druggist.

Having proved to their own satisfaction that they had the real curara, they made two kinds of preparation, aqueous solutions and powders. The solutions intended for application by means of the hypodermic syringe, were made of the strength of one-fifth, one-tenth, and one-hundredth, and were varied according to circumstances. In order to avoid the uncertainty which would arise if the quantity employed were estimated by drops (which would vary according to the density of the fluid, the size of the orifice of the syringe, etc.), the actual weight of the dose was ascertained at each experiment. The channels by which the substance was introduced into the system, were the subcutaneous cellular tissue, the cutis vera, the mouth, and the rectum; but preferably, and more frequently, by injection into the subcutaneous cellular tissue, as being the most prompt and certain manner of inducing the action of the poison. When given by the mouth it produces comparatively little effect even in considerable doses, especially if the stomach contains food. They experimented physiologically upon several of the lower animals—rabbit, guinea-pig, cat, and frog—and administered it therapeutically to patients in the Bicêtre, cautiously advancing from the former to the latter; never venturing upon the exhibition of a dose in the human subject, the effects of which had not been previously ascertained in one of the lower animals.

With reference to the action of curara upon the lower animals, MM. Voisin and Liouville state that its effects were always characteristic, and that they have in no instance, whatever the dose or animal operated upon, observed symptoms which could be confounded with those produced by other poisons, such as strychnia, for example, to the action of which that of curara has by some observers been compared. Another point of interest has reference to the changes of temperature which are observed during the action of curara. In cases in which poisonous doses were given, a remarkable elevation of the temperature of the rectum was recorded; an increase to the extent of from 3° to 4° C., whilst the superficial temperature of the body presented no remarkable alteration. The circulation was at first excited and irregular, but gradually became depressed, ending with asphyxia. The pupil is usually dilated by moderate doses; but after a large dose, although at first widely dilated, contraction follows the dilatation somewhat suddenly, and subsequently there is an alternation between dilatation and contraction. Double exophthalmia was also observed after poisonous doses. The nerves of voluntary motion are always implicated, though to a variable extent, and not with precise uniformity; the muscles, however, contract under the influence of electricity, and cutaneous sensibility and muscular irritability are unimpaired. It would seem that, in the case of frogs and animals of that order, death is preceded by complete abolition of motive power; whilst in mammiferous animals there may be considerable motive power at the moment of death.

One of the principal objects which the observers had in view, was to determine the posology of curara, believing that its employment as a medicine was greatly retarded by the uncertainty of the dose. The results of their experiments upon the lower animals in that respect, were as follows: 1. The curara of Claude Bernard, No. 1, produced marked, but not fatal, effects in vigorous rabbits, in doses of six milligrammes (·0924 of a grain), applied hypodermically. 2. The curara of Claude Bernard, No. 2, produced marked, but not fatal, effects in rabbits, in doses of seven milligrammes (·1078 of a grain), applied hypodermically. 3. The curara of Carrey produced marked, but not fatal, effects in rabbits, in doses of three milligrammes (·0462 of a grain), applied hypodermically. 4. The curara of Ménier produced marked, but not mortal, effects upon rabbits in doses of seven milligrammes (·1078 of a grain), applied hypodermically.

Acting upon the results of these experiments, MM. Voisin and Liouville cautiously approached the therapeutical application of curara to the diseases of the human family. The only patients treated by them were epileptics in the Bicêtre, twelve in number, either adults or youths above fifteen years of age. In each case the patient was carefully weighed, this being of considerable importance in relation to the dose of the medicine. The treatment in most of the cases began in August 1865, and still continues. They set out with exceedingly small doses, even so small as two-tenths of a milligramme, their object in doing so being chiefly to disarm the infinitesimalists. These doses produced no appreciable effect, whether physiological or therapeutical, and they cautiously increased them until they came to a knowledge of the proper quantity. Having arrived at doses of from sixty to seventy milligrammes, they continued cautiously to increase them, until they reached, for subcutaneous injection, to the dose of eighteen centigrammes; endermically (by vesication), to the dose of thirty-eight centigrammes; and by the mouth or anus, equally, to a dose of forty centigrammes. The effects of the medicine they describe as of two kinds, local and general. The local effects vary with the manner of its application, and are chiefly manifested in the subcutaneous and endermic methods. When injected subcutaneously, the skin over the injected part immediately presents a pale rose-coloured swelling, resembling the stinging of a nettle, surrounded to a variable extent by a red blush, which disappears for the moment on pressure. The nettle-sting appearance remains only a few hours, but the reddened surface continues with special characters—namely, a remarkable and sudden elevation of temperature of the surface, a consecutive phlegmonous appearance, and subcutaneous puffiness. The local phenomena which follow the subcutaneous injection of curara, differ according to the condition of the solution employed. If the solution be previously filtered, the redness of the skin, the subcutaneous puffiness, and the increase of temperature, seldom remain longer than a few hours, or at most, a few days; but, on the other hand, if the solution be not filtered, the effects are persistent, and the swelling either terminates at the end of a few weeks, by the formation of a still more persistent fibrous knot, or by suppuration; and, lastly, several months after daily subcutaneous injections, the skin of the part may be the seat of a circular furfuraceous desquamation. Swelling of the neighbouring glands does not always accompany this phlegmonic condition. When applied endermically to a blistered surface, the local consequences are a sharp, stinging pain the moment the part is sprinkled with the powder, elevation of temperature, and sometimes peripheric erythema.

The general effects produced by curara vary in degree according to the dose, with occasional peculiarities depending upon the manner of the application. MM. Voisin and Liouville divide these general phenomena into two classes, according to their severity, terming them relatively ordinary and violent. The ordinary, or less severe phenomena, are chiefly manifested in the circulatory system; the pulsations are somewhat increased both in force and frequency, and are dicrotous during a certain number of hours; sometimes profuse perspirations are witnessed; the temperature of the axilla is increased, at most, from 1° to 2° C., and the number of respirations from four to eight, at the highest: the urine is increased in quantity; it is remarkably light-coloured, and contains sugar. The more intense or violent general phenomena produced by relatively large doses of curara are, in one word, fever with all its characteristics. They consist of disturbance of the circulation, respiration, calorification, and motion; of hypersecretions, and of symptoms connected with the cerebral and visual functions. In the first place, there is shivering, a feeling of intense cold, cutis anserina, chattering of the teeth, starting, trembling of the entire body, accompanied almost immediately by a small and rapid pulse, anxiousness, sighing respiration, elevation of axillary temperature, and, in one instance, by diplopia. The motor power of the lower extremities very rapidly diminishes or disappears altogether; equilibrium and the co-ordination of their movements are disturbed, and sometimes the patients are utterly unable to move their legs and to assume a sitting posture. Considerable thirst, severe headache, sleeplessness, and sometimes diuresis, are added to the previous symptoms. The feeling of cold is succeeded, for several hours, by hot skin; frequent, full, and dicrotous pulse; redness of the body, especially of the face and ears; injected conjunctivæ; and, lastly, a profuse sweat. The paralysis of the extremities continued but for a short time (a quarter of an hour to an hour at most) in the cases of MM. Voisin and Liouville, because they were cautious to apply in good time the ligatures recommended by Claude Bernard —namely, a few turns of a bandage at some part between the heart and the situation at which the curara was applied. The secretion of urine, during these phenomena, is notably increased, chemical analysis proves the presence of sugar, and physiological experiment establishes the presence of curara. The elimination of curara in the urine appears to cease in about twenty hours, judging from experiments made upon the lower animals by injecting the urine of the patients into their cellular tissue. The fever continues for a longer or shorter period, according to the dose; they have known it continue for five or six days, gradually diminishing in severity.

With respect to the therapeutical value of curara, MM. Voisin and Liouville do not profess to have made much progress. What they say simply amounts to this, with reference to epilepsy, that if there is hitherto not much to be adduced in favour of curara, at least there is nothing to be said against it: time will show the rest. Two other points of interest are mentioned in their memoir—namely, the treatment and detection of poisoning by curara. Both by experiment upon the lower animals, and by clinical observation, they have learned that the treatment of poisoning by curara consists in the application of a ligature round the limb, between the part injected and the heart; the free exhibition of diluents, and of all kinds of evacuants, artificial respiration when required, and as a last resource, in case of asphyxia, tracheotomy. For the detection of curara employed as a poison, they depend upon the production of its characteristic effects upon the lower animals; the urine of the victim if in life, or a solution obtained from the organs and tissues after death, being injected into their subcutaneous cellular tissue.

Part Fifth.

MEDICAL NEWS.

NAVAL MEDICAL DEPARTMENT.

"SURELY in vain the net is spread in the sight of any bird," was our natural reflection on laying down that would-be-so-candid and persuasive production, yclept "Everything about them" (that is, about Naval Doctors), recently published in this good town of ours.[1]

The ingenious author declares his conviction "that the comparative falling-off in the entry of Scotch assistant-surgeons is to be attributed to the readier means of information the Irish medical students possess as to the changes and improvements which more especially have recently been made in the position, pay, etc., of the whole body of medical officers in the navy."

We, on the other hand, unhesitatingly assert, from intimate knowledge, that our Scotch students *know too much*—only too well, all about the position, pay, etc.—in a word, all about the grievances, still unredressed, of which our naval brethren justly complain; and we fear that several (if it be not an Irishism to use the word, when so few have been *caught*) young Irishmen have been deluded into the service by a kind of "touting," by the advice and pressure of interested "grinders," and through what we cannot but consider a want of high professional spirit.

The disinterested author scarcely does justice to his older countrymen, "who have suffered (in the service), and still are strong." Let us assure him, they have not failed with patriotic spirit duly to enlighten their and his young countrymen on the questions at issue, and with good effect too. Moreover, we ourselves (with humility we say it) have borne our own small share of the burthen and heat of the day. In the September number of this Journal, we took occasion to make a few brief comments on the precious legacy left by the late Liberal board to their successors in office; and we ventured to express the hope that "My Lords" now at the helm of that uneasy barque, "The Royal Navy," would be wise enough to improve the present occasion, and give at once redress and satisfaction. But, alas! as yet they have made no sign: and we have too good reason for saying, by way of caution to all, but more especially to our own canny countrymen, that we have no hope that the greatest desideratum of all—the optional retirement at 20 years—will be granted, *unless at a great sacrifice*. We formerly pointed out that "My [Liberal] Lords" made the very tempting offer of optional retirement at 20 years, with 12s. *per diem,—i.e., at a loss of* 4s. 6d. *only a-day!* If "their Lordships" of to-day offer 1s. more, it is, we shrewdly suspect, the highest bid that they will make, at any rate, for the present: and it will be made in vain, we fear, and, we must add, we hope, in the best interests of the Queen's service.

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[1] "Everything about them," for the Information of Medical Students, especially of the Scottish Universities, etc. Edinburgh: Edmonston and Douglas: 1866.

Most likely the gallant author who enjoyed the opportunity of "reading the letter in one of the naval and military papers a few weeks ago," may have seen the leading article on his little work in the Army and Navy Gazette of the 8th ult.; but, lest his young countrymen, over whom his patriotic heart yearns so tenderly, should not have had the same advantage, we insert it here for their edification:—

"Some northern patriot, who saw much of sea life in the days before assistant-surgeons were admitted into the ward-room mess, which change it took a forty-three years' war to effect, has stepped forward to reclaim the naval service from the neglect—bordering on contempt—into which it has fallen in his native land beyond the Tweed. He has published a pamphlet under the title of 'Everything about them,' *i.e.*, about the Doctors in the Navy. The intention is a good one, that of enticing young medical men of 'Edinboro' toun' not to think twice about it, but to go on board ship and trust again to the Admiralty circulars as things that can never be departed from by the Admiralty, or overruled by the captain of the ship in which they may be sent to the coast of Africa or to the West Indies. The writer attributes the scarcity of young Edinburgh M.D.'s in the Navy to the rarity with which they behold the elegant forms of our iron-clads in northern waters, and the rare opportunities the students of the northern universities have of coming in contact with the officers belonging to ships of the Navy, whereas we know that the history of life on board related at the schools by those returned from sea is the most deterring cause of any extant. And we infer that, until the profession in the service be rendered content with its treatment and the fair dealing extended to it, there can be little hope of gaining many more candidates than at present from the Scotch or English schools of medicine, where the teachers also gain inspiration from the elders of the naval profession. Certainly the author has done everything *ad captandum*. Thus the facility of entering appears great, but when the curriculum is regularly exacted, many candidates stick fast on the threshold. And again, the examination returns have shown that many who apply are not the shining lights of the schools they come from, often bringing discredit on their *alma mater*, and the last paragraph in page 13 might mislead into a belief that the possession of a diploma is a sufficient guarantee of education, whereas it is not so. The notification of the outfitters in the Strand, at which the recruit may obtain his ribbons cheap, exclusive of his linen; that of the advance of pay on joining, the startling grades of pay to which so few attain; the extra pay and allowances which cause dissatisfaction to the fortunate recipients, from being inferior to those of their relative rank in the executive class; the very absurdly small cost of living in naval messes, and the pleasures of the social board, free and unrestrained; the half-service of an individual in the joint capacity of valet and housemaid; the way to promotion even to that of inspector-general on the completion of thirteen years' service; the glittering heaps of prize-money and of honorary distinction, are dilated on with a charming *naïveté* that would well become a recruiting sergeant, confident of the dazzle of his trappings and of the penetrability of the minds of his hearers. But, alas! for what we know of medical students on both sides of the Tweed, there is an inaptitude to be deceived, and our humble opinion is that such small efforts as these are thrown away on the minds of intelligent professional men; but of this we are sure, that much would be secured at once in dealing with the schools that are specially addressed by the author, if the Admiralty would state, without further waiting on the War Office, what are its finalities on the points of full pay to the older medical officers, hospital allowances (whether to be or not by relative rank with the executive of the Navy), and what of the honorary distinctions that have not been awarded, although the power exists. As in the instance of the Earl of Chatham and Sir Richard Strahan, so here, we think, much valuable time is being lost."

ROYAL COLLEGE OF PHYSICIANS OF EDINBURGH.

At the Annual Election Meeting of the Royal College of Physicians of Edinburgh, held on 6th December, the following office-bearers were elected for the ensuing year:—*President*—Dr John Smith. *Vice-President*—Dr John Graham Macdonald Burt. *Council*—Dr William Seller, Dr John Moir, Dr Bobert Bowes Malcolm, Dr Alexander Wood, Dr John Graham Macdonald Burt, Dr Robert Paterson. *Examiners*—The President, Drs Wood, Douglas, Burt, R. Paterson, Wright, Keiller, Pattison, Cumming, Matthews Duncan, J. W. Begbie, Haldane, Sanders. *Treasurer*—Dr Samuel Somerville. *Secretary*—Dr Rutherford Haldane. *Curator of Museum*—Dr William Henry Lowe. *Librarian*—Dr Matthews Duncan. *Registrar of Applicants for License*—Dr Rutherford Haldane. *Clerk*—Christopher Douglas, W.S. *Auditor*—Kenneth Mackenzie, C.A. *Under-Librarian*—John Small, M.A. *Officer*—Thomas Marshall.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

The following office-bearers for the ensuing year were elected on Wednesday 19th December 1866:—*President*—John Moir, M.D., F.R.C.P. *Vice-Presidents*—D. R. Haldane, M.D.; J. D. Gillespie, M.D.; Robert Omond, M.D. *Councillors*—Arthur Mitchell, M.D.; R. P. Ritchie, M.D.; D. A. Robertson, M.D.; William Stephenson, M.D.; A. Halliday Douglas, M.D.; J. A. Smith, M.D.; Joseph Bell, M.D.; Arthur Gamgee, M.D. *Treasurer*—George W. Balfour, M.D. *Secretaries*—Patrick Heron Watson, M.D.; Charles Dycer, M.D.

PUBLICATIONS RECEIVED.

Cooley,—The Pill-Book. By Arnold J. Cooley. London, 1866.

Copland,—The Forms, Complications, Causes, Prevention, and Treatment of Consumption and Bronchitis. By J. Copland, M.D., etc. New Edition. London, 1866.

"Everything about them." Edinburgh, 1866.

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Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*On the Neglect of the Study of Skin Diseases in England.* By TILBURY FOX, M.D. Lond., M.R.C.P., Physician to St John's Hospital for Diseases of the Skin.

(*Read before the Medical Society of London, Nov.* 1866.)

MR PRESIDENT,—In the admirable address delivered on the first night of the present session, you took occasion to remark incidentally that it would be of utility if the Medical Society occasionally took cognizance of some of those topics of the hour which were attracting special attention in the profession, and you justified your remarks by a reference to the good that had and might still ensue from debates on the subject of cholera.

I shall venture to give expression to two or three convictions, with the view of increasing, if possible, attention to a matter which has caused no little stir during the last two or three years, and about which much has been said, and much more has been written. If the policy of my choice be questioned, I shall seek for protection in the suggestion which originally fell from the chair, and which prompted me to acquiesce in the request of one or two Fellows, that I would open a discussion on the present state of the study of Diseases of the Skin in England.

One is naturally disappointed at the small results which have followed the consumption of the very large amount of print, paper, and colouring which publishers have placed in our hands during the last three years, whilst there seems to be fair prospect of additional production; and one can only hope that the concentration of attention towards a much-neglected subject will lead to the production of able investigators.

The press has already unanimously pronounced the verdict that more ignorance prevails in skin matters than in any other branch of medicine, and that by far too little is known by the majority of us of the most ordinary eruptions of the surface; and, indeed, this is not to be wondered at, for there is *no opportunity* in England by which a man who aims to prosecute the study of cutaneous medicine can do so with much prospect of satisfactory success. Since Willan's time, what *real* advances have been made by the labours of Englishmen? Most of us have, no doubt, read the articles which appeared in the *Medical Times and Gazette* a little time since, entitled, "Modern Dermatology;" they were penned by an accomplished hospital physician, a non-specialist, and sought in an impartial manner to convey the impressions and conclusions at which a man free from bias on the subject, and without any pet theory to uphold (an outsider, in fact, as regards skin matters), would arrive. To what conclusion did the writer come? That all advances of late have been made by continental observers; and, in truth, men are guided in skin matters by one or two empirical formulæ, and possess no confidence in any means of cure they handle, trusting to empiricism of the rankest kind. Whilst men avow openly that they know but little of the subject, a vast amount of valuable material for investigation and a large field for research are rendered useless from want of organization, and in consequence of the difficulty of access thereto.

Our machinery for the treatment and teaching of skin disease must surely be of the most inefficient kind. It is, indeed, a startling fact that at no single hospital in London is there a ward set apart for cases of skin disease, and at only one school of medicine (University College) is there a special course of lectures on cutaneous pathology, and that consists of six lectures. I cannot, however, avoid mentioning that Dr Jenner, to whose teaching in all that relates to the subject I owe so much, has always been a zealous and able teacher and demonstrator wherever the opportunity presented itself; and it was during the time that I was his assistant that I first appreciated the importance of the study of skin disease, and got an insight into the great pathological significance of the phenomena presented by them. But since the time that Dr Jenner held the post of clinical professor at University College, I do not know that it has been usual to hear a clinical lecture on a disease of the skin in any one of the London hospitals except on the rarest occasions.

At Guy's Hospital and at St Thomas's, a beginning has been made, and cases of skin disease are seen once a-week by one of the physicians. At University College, the same arrangement holds good. But at no hospital is there a special course of lectures save at the one named. How few original observations are being made. How few clinical demonstrations are given. How regularly does the word *eczema* do duty for a whole batch of unknown cases. How often is diagnosis shirked! Shall I catalogue the diagnoses I have seen within the last few months? Scabies mistaken by "good men" for syphilis; varicella for scabies; the erythema of leprosy for secondary syphilis, by a number of Indian medical officers; chloasma for syphilitic stains, and made a reason for invaliding home to England; scrofulo derma for lupus exedens: so that the patient, who merely required a course of tonics and cod-liver oil, was made to give up a good appointment, which was his all; and a host of others. Why, the ancients really were, in many cases, more perfect in their diagnosis than we are. The ancient literature of cutaneous medicine is replete with examples of the careful way in which distinctions were made, and the just grounds upon which conclusions were based,—conclusions which are our *guides* at the present day in matters of doubt in many a disease,—for example, in lepra, leprosy, alphos, leuce, vitiligo, pityriasis, kerion.

Suppose we take the work in our out-patient departments. Is the patient stripped and examined carefully? Rather, is not some one part, where the disease is the most easily seen, hastily glanced at? Are a few typical cases selected and the whole history of the patient carefully scrutinized? Is the use of remedies prompted by any degree of philosophy? Is much more than the routine of arsenic, zinc, and citrine ointments gone through? Is any attempt made to determine the natural course of any one disease uninterfered with by external agencies? The amount of clinical teaching in the case of eye, ear, lung, heart, kidney, or brain disease is large, but small is the amount of attention paid to skin pathology. Is a glance once a-week enough for the cure of scabies or an eczema, the two commonest diseases that we meet with? Suppose we use our compound sulphur ointments to a case of itch *for a week*, what frequently happens? the setting up of secondary inflammation, which is then looked upon as part and parcel of the scabies, and an evidence that still more vigorous treatment is needed; whereas it should be wholly otherwise. I have seen these cases of mal-treatment, for it is nothing else, over and over again. If skin cases are to be treated *successfully*, they must be seen *frequently*.

Are the medical officers in the out-patient room particularly anxious to encourage skin cases? Are the latter not sent from one room to another to shift for themselves, wholly uncared for? What one general hospital has that most important item in the machinery of treatment,—a complete set of medicinal baths?

These are matters of common observation, of frequent comment, and of much regret. No wonder that men go abroad to learn what they fail to get in England. It is impossible to avoid the censure which we cast upon ourselves by this one fact, that so sadly deficient is the study, and so little the opportunity afforded even the more zealous student, of acquiring really a moderate amount of knowledge of skin diseases, that he finds it not only worth his while, but necessary almost to his credit, to undergo the special expense and trouble of a residence abroad to make good the deficiency.

And what good does he not get? Hebra has 250 beds, he gives a lecture once a-day, and demonstrations besides. Paris and Vienna both have magnificent skin hospitals, and many other cities likewise. Here, in England, the fancy is, to speak of those who devote their attention to skin matters as something little better than quacks, and the theme of their study as of a lower grade than that of other specialties. In some degree, dermatologists have brought this upon themselves; they have started special hospitals as media for self-advertisement; but above and beyond is the fact, that our ignorance of the subject of skin pathology is so decided, that most men find it convenient to follow the advice,—"Give a dog a bad name and hang him." If we look at the matter from a *diagnostic* point of view, we can see enough to awaken in us a sense of our shortcomings; if from a *therapeutical* point of view, sufficient to shake our blind faith in the routine practice of treating the vast majority of cases by a process which is in too many cases nothing less than slow poisoning by arsenic; if from a *pathological* point of view, a multitude of facts that prove how intimately connected are diseased changes in the skin with alterations in the system at large or the functions of some internal organ, and how necessary it is that the dermatologist should possess a full acquaintance with pathology in general. This deficiency is, unfortunately, always apparent in the writings of those who devote themselves to cutaneous medicine. A recent instance will confirm the statement. The senior surgeon of one of the leading hospitals asks a well-known dermatologist, one whose experience has been very large indeed, how to treat boils and carbuncles. The answer, which we read in the *British Medical Journal* for November 10, 1866, contains the writer's pathological views, which regards "boils and carbuncles as having frequently or constantly a parasitic origin." The proofs are, not the finding of a vegetable fungus on microscopic examination, for it is mostly absent, but the occasional occurrence of contagion, and the beneficial effect of a so-called parasiticide (properly a caustic) acid nitrate of mercury. Such a statement I conceive to be most unjust towards cutaneous medicine, and one likely to bring it into undeserved contempt. For, analyze the case. Carbuncles and boils have nothing in common with vegetable parasitic diseases, which have now been studied with much care and most certain and useful results; and nitrate of mercury cures a host of most dissimilar diseases. Are they, then, parasitic? Acid nitrate of mercury, moreover, is not a parasiticide in the true sense of the word. The fact of boils being occasionally contagious is no proof that they are of parasitic origin. Contagion, according to all recent research, is brought about by the transplantation of living particles of matter, and animal as well as vegetable cells seem to possess this property of transportability. My own observations on molluscum teach me that the peculiar cells formed in the little sebaceous tumours are the media by which occasional contagion is brought about. If the cells from a leprous sore, from cancer, and as would seem from tuberculous ulceration, from syphilis, and a host of other diseases, can be transplanted, why may not the pus-cell of a boil? What law would it contradict? Indeed, it would only confirm those new views of contagion which the most recent observations seem to have established,—the researches, for instance, of Mr Crookes and Dr Angus Smith, negatively, and Dr Beale, positively, in Rinderpest.

In discussing the question of carbuncle, we ought to know that their formation is associated with three special conditions:—

1. The presence of bacteridia,—an accidental feature.

2. Excess of uric acid in the blood, and urea in the urine,—an evidence of excessive waste or deficient elimination or oxidation.

3. Diabetes,—a connexion noticed by Cheselden in 1768, Latham, Prout, Landouzy, Kuchenmeister, Jordão, Menestrel, Wagner, and recently Dr Fonseca of Pernambuco, in which place the occurrence of a particular species of carbuncle is regarded as diagnostic of diabetes. We cannot have forgotten the fact that a striking exemplification was afforded in the death of Cardinal Wiseman, and that carbuncle and diabetes were conjoined in his case.

If we would unravel such a mystery as that of boils and carbuncles, we must be *au fait* with the most advanced doctrines of the day. Clearly no more profound investigation exists in the whole range of medicine than the study of the connexion between errors of sugar-producing function and carbuncular diseases; and not to be "up" in the recent literature of general medicine is clearly reprehensible on the part of the dermatologist, and brings discredit upon the branch of medicine to which he devotes his attention. Take one more illustration, Herpes zoster, or shingles, the mention of which alone brings before the mind many very complex and difficult questions in reference to the pathology of diseases of the nervous system. Without more examples, it may be fairly put, that the more deeply the subject of skin diseases is studied, the more intimately connected do they appear with alterations in the functions of the system at large; that the accusations of semi-quackism and superficiality do not belong to the subject, but only the manner in which it may have been handled. The subject truly has never been fairly placed in its proper light in its relation to general pathology, whilst the minute study of the changes in the skin *must* help to clear up many a doubtful point in cell pathology.

I pass on to remark that the special institutions that have been established from time to time have not advanced science one step. I venture to hope that, if I speak boldly, at any rate I am not far short of the truth. I protest against the waste of material, the slovenly, and therefore obstructive, modes of observation, the want of fairness to students, to the public, and to the poor. If the general hospitals fail to help in this matter, I apprehend that Mr Bowman's words, "Specialties are a necessary part of the progress of the age, and, as such, should be allowed to develop themselves," will find a practical exemplification, and that we must accept with regret the establishment of special beyond the walls of the general hospitals.

Something is wanted to help us out of an ugly dilemma. We have no settled opinion about matters most elementary—on classification the most varied difference—of the natural course of diseases the least possible knowledge, and about elementary lesions and nomenclature the amplest contradictions, and we are opening our arms wide abroad, and imbibing with no little eagerness the views and opinions of foreign dermatologists. This, I conceive to be, to us English, a source of no little danger; indeed, it is the point of all others that I would try to raise a discussion upon. English skin diseases and those of foreign climes resemble each other up to a certain point, but thence they diverge, and have peculiarities in either case which must be always regarded,—differences that will not permit the same treatment to be alike applicable to the two cases. The English constitution and habit require a special plan of treatment of their own.

Now, were there any authority at our medical schools, a *corrective* influence would be exerted upon all novel introductions; for the mass of practitioners look to such an oracle for their guidance, and the young observer takes good heed that he do not lose the more matured experience of his elders. Unluckily for us, there is no such safeguard. To whom, at our medical schools, can we look for guidance in this matter?

But to return. The distinction between the skin diseases of this and other countries may be seen in books, in plates, and, better still, in practice; but few enjoy the opportunity of putting this latter test into operation. Last year (1865), I was fortunate to spend some time in Paris, Vienna, and in the East, and, as may be supposed, my attention was on the keen hunt for all things new in matters of skin diseases. I took the trouble to go carefully into the special features of diseases of the skin in Egypt and Syria as compared with those seen in our own country. These notes will be found in the *Medical Times and Gazette* for the current year (1866).

For some years past foreign, especially German, views have been steadily introduced into England, and the labours of poor Willan and Bateman have been almost lost sight of. Some years ago we were essentially French in our ideas,—Biett, Schedel, and Cazenove reigned. Now, it must have been within the constant experience of most men, that the application of foreign descriptions to practical diagnosis has often failed; and it is not to be wondered at in the least, when we remember the difference of constitution, of climate, of hereditary tendency, of diet, and other circumstances.

Many of Hebra's descriptions of ordinary instances of disease, always good, really represent exaggerated forms of English skin eruptions. This does not for one moment detract from the greatness of Hebra's position. I give him every possible honour as an able, most acute, and independent worker and thinker; but as I have marked the contrast myself, it may not be wholly useless to point out the fact that such a difference does exist; and may it not be well to remark the essential distinction of cutaneous mischief of the

English at a time when Hebra's capital book is to be presented to us in an English garb? Well, it so comes, that writers have tried to find the counterpart of Hebra's diseases, and been unable to do so. Take Lichen ruber, and Lichen scrofulosus, and Eczema marginatum, for examples. A simple case will do: scabies (itch). Hebra will show you some dozens of cases; but all those which I saw were ecthymatous to a marked degree, affected by no little ulceration and an unhealthy condition of skin, which seemed to betoken a special want of vigour and cleanliness on the part of the afflicted.

Take Lichen ruber, already referred to. It is a much more severe disease in Austria; in England it is less scaly, less developed, less inflammatory, and unaccompanied by marasmus and death.

Eczema, in the East, is much more like a lichenous patch than an eczematous patch.

Eczema marginatum is not a variety that we usually meet with here.

Favus is very common, according to my own experience, throughout Egypt, and the constitutional aspect is much marked and perfect. Syphilis is not severe in the East; it is becoming more so, but at present the eruptive manifestations are not so common.

Rubeola notha (bastard measles) is an exceedingly common affection in Cairo, and scarlatina seems almost always to have a favourable termination, according to the authority of Egyptian physicians. Dr Paterson, of Cairo, corroborates this opinion.

Pityriasis rubra (Devergie's description), and general psoriasis, too, are not very often seen in this country; they seem to be pretty abundant abroad.

I have never seen or heard of such terrible development of molluscum in our own country as those which I saw in Hebra's wards at Vienna. Plica polonica is another example in point. Pellagra is another; it is occasionally seen in England, but only in slight forms. The Morphea alba and nigra are very usual in some of the Eastern localities; rare in England. The Barbadoes leg, again, may be cited. Now all these diseases I have mentioned occur both in our own and other countries. They are not diseases absolutely peculiar to any clime, and we see differences which they present under the circumstances mentioned.

But the best examples of modification produced by climate and race are noticed in the ulcerative class of surface diseases: lupus and rupia may be taken as examples. We are all apt to be rather startled at the hideous ravages portrayed in some of the plates of foreign dermatologists, and a great truth underlies the contrast. I suppose that the meat-eating and porter-loving Englishman, with his hardy pursuits, acquires a greater amount of *stamina*, and that degenerations dependent upon constitutional debility are not so marked in his case as in that of the vegetable-feeding black or the fish-eating northerner.

Time only allows me to rough out the fact of the variation in aspect of eruptions dependent upon difference of habit and of clime; but it is one which I feel sure this Society will not think of small importance. I would merely add, that I was particularly struck, in going through Hebra's wards, with the peculiarly "ulcerative" aspect assumed by most of the diseases. One thing more I cannot fail to note, that "the nervous element" seems to me to enter more fully into the composition of English skin eruptions; that is to say, a smaller amount of eruption is "accompanied by a greater degree of irritation" in the Englishman than in the foreigner. No doubt, the "struggle for existence" and the "race for wealth" in our little island tell specially upon our nervous systems, which seems, as it were, ever at "full-cock," and ripe for mischief. On the other hand, there appears to be more reparative power about the eruptive diseases of our own land. Such is my strong conviction. If true, it is of no little moment in regard to treatment: if there is less degeneration of tissue, there is much activity about the early stages. This, however, necessarily involves greater reaction; hence it seems proper to hold such a disease in check with a high hand at the outset, and support more quickly and by a more abrupt transition farther on in the treatment.

There is much reason, then, in the established difference of routine in the treatment of English and foreign diseases. The ptisans and liquid slops enjoyed by foreigners do not, as a rule, suit the English habit, which wants more purgatives and decided febrifuges. Nature, with good nursing, manages to get on pretty well in hot climates; but the sthenic type of our maladies often requires a special check to excessive action, and so we find that the foreign treatments do good in those cases which assume the features possessed by the disease of foreign parts: the various "waters" are beneficial in cachectic and ulcerative stages.

Enough has been said to show that, in our own city, the subject of skin diseases is but too sadly neglected; that, at the present time, it is most desirable that we should be placed in possession of the opportunity of carefully studying typical cases of disease. The whole field of cell-pathology and tissue-action is one to which special attention is being directed; and in the matter of diseases of the surface, investigation of the kind is the only method by which we shall clear up the discrepancies and terrible confusion which attach to the subject of dermal pathology; moreover, at the present time, the reception of foreign views, and their adoption to English practice, ought not to be allowed with so little reservation. Indeed, we sadly want to encourage the foundation of a British school of cutaneous medicine; for to apply the treatment recommended by foreign authorities closely to the English forms of disease, is a grave error. A dozen beds; the selection of typical cases for observation; and the ascertainment of the natural course of typical instances of disease, is the first work we have to do. And, as one way of encouraging a greater degree of attention to the subject, I venture to appeal with no little earnestness to the various Examiners at our Medical Boards; they have in their possession an immense power for good in this matter. The papers of the University of London have for the last few years generally contained questions upon skin diseases, and practical diagnoses have been demanded from the student at the *vivâ voce* examinations. The example thus set has been followed in some little degree by other bodies. It would do more for cutaneous medicine than the united work of all our dermatologists for years to come, if Examiners would make it a point to secure proficiency in a branch of medicine so much neglected,—not a mere book description, but such as would tend to rescue men from being satisfied with the formulæ of the empiric; for it is indubitable that the only successful dermatologist is he who is conversant with the intimate alteration of tissue and function in diseases of internal organs.

Of one thing there need be no question, that the present administration is not fair to the poor; sufficient attention is not paid to their cases, and most of them are voted a nuisance. At the present time, *itch* is more prevalent than it ever was; it is abroad amongst the upper classes to a very considerable extent, and I firmly believe this is due in some measure to errors in diagnosis, by which the disease remains unchecked and thus propagates itself; that is to say, a large amount of prevention is at hand, but wholly unused by us.

The general hospitals have refused all overtures that have been made to adopt special means for the treatment and study of skin diseases, and there seems a deeply-rooted objection to allowing the systematic admission into the general hospitals of skin cases, but the time has now arrived when the establishment of special skin departments, or of large special institutions, is urgently called for, which shall fulfil two objects,—supply the wants of the poor, and furnish a field for scientific study.

And when we turn to the present system of treating skin diseases, how clearly and forcibly does this necessity appear. There never was a greater empiricism than that which governs matters cutaneous. What single rule, based on physiological or pathological considerations, guides us in treatment. Be the case what it may—an inflammation, a nervous or a parasitic affection, a hypertrophy, or an ulcer, from a wart to a cancer, a mere irritation to a wholly raw skin—we give arsenic, or change to a mercurial course, and, topically, zinc or mercurial ointments. We have no notion of the way in which arsenic acts. Some say, as a general nerve tonic; others, locally upon diseased tissues, or through some special action upon the blood.

Some recent experiments seem to point to something a little more definite. MM. Bergeron and Lemaitre found that arsenical compounds are decomposed in the system, the arsenic being carried off by the sweat. This involves the question of the selective power of different tissues for certain drugs, which we must discuss before we can arrive at any conclusion as to the action of arsenic. For my own part, I am disposed to think that the good effects of arsenic are due to general tonic effects and the local action of the metal upon the process of cell-growth in diseased structures; and were it possible to introduce the arsenic from without, a much better and a more speedy result would follow. It may be in my power, I hope, to lay the results of some experiments upon this point before the Society at some future date; but I do not hesitate to remark, that I have little faith in the wondrously vaunted virtues of arsenic. Gibert long ago showed that he could cure his cases of psoriasis better without than with; and it seems to me that we do not pay sufficient attention to local tissue actions. Many diseases which we try to cure by arsenic are practically local diseases.

It has never yet been sufficiently, if at all, taught that a local disease may arise out of a general disease, and in the end become practically a purely local matter. Take lepra; the hypertrophic condition of cuticle, of course, results from a peculiar action of the cells in relation to a special character of blood; but if the dermal cells become habitually disposed to proliferation, this may take place although the blood state has been nearly corrected as regards the original vice upon which the whole disease depended. Now, if a father beget a child, he transmits the local expression of the disease, which has become of greater importance than the blood state; therefore it is that in some hereditary diseases the local tissue changes are the most important, and local treatment the essence of cure. Hence it is that in hereditary lepra the local mischief is often obstinate, whilst the general health appears so good. I have called attention briefly to this point in my work.

Hebra seems to recognise fully the influence of different kinds of cell-growth, for he is the champion of the local treatment of skin diseases. Let me give some illustrations of different kinds of tissue action, to which little attention is paid. In the exanthemata you have the superficial layer of the cuticle perishing and falling off in the matter of desquamation, a hyperæmic condition of cuticle, and a torpid action of the glands, as evidenced by the deficient secretion of sebaceous matter, and the loss of the hairs. All these damages are repaired by the reformation of new cuticle; but in order to secure this end the skin should have rest; and the action of the air, of external irritants, on the unprotected layer, is often followed by considerable deviations from health. We want, if possible, to give the skin rest; and, practically, the application of a layer of grease to form a kind of sheath, preventing oxidation, and checking too great evaporation or even absorption, is found to be most beneficial —it gives *rest.* This practice has as yet not found favour with

Englishmen. We have, secondly, a batch of cases in which too much work is thrown upon the skin, in consequence of the function of some organ which acts in relation being deficient. Nothing is more common than in eczema of the legs to have a deficient action of the kidney; the blood is charged with effete matter; the drain of fluid from the kidneys is lessened; the skin takes on what appears to be an eliminative action instead, and the legs suffer most on account of the influence of gravitation. This is one of the most common facts in pathology, but it is one of which we make little use. Again, in eczema infantile, it is by no means uncommon for diarrhœa of a peculiar character, with which I am acquainted, to replace the skin mischief, and *vice versa*. Again, how essentially is the disease connected with a faulty assimilation and digestion and improper food. All this teaches us the value of ascertaining and respecting the relative action of organs, and the value of purgatives, diuretics, and digestors.

Then, again, how often is deficient action of the perspiratory function followed by some such change as lichen pilaris or disturbance of the sebaceous glands; yet how seldom do we think of securing a healthy action of the skin as a whole in cases of skin mischief. How universal is the plan of pouring in arsenic into the system of all skin patients, to remove so many and varied ills—of liver, of stomach, of kidney, of nerves—things the most dissimilar, which are at the bottom of skin mischief? And I venture to say, that in a very large number of cases the cure can be effected much more expeditiously by the adoption of means other than the administration of arsenic, based upon a just calculation of the condition of the patient as a whole, often by attacking some other part than the skin, or especially by the employment of local remedies. Moreover, I find that to cure most cases arsenic must be given for a very long time, and during this time the seeds of much after mischief are sown. The same holds good with the free use of mercury. Obstinate and lasting dyspepsia is often set up, according to my own experience. I have imbibed a certain horror of the excessive use of arsenic now in vogue. I have seen too many failures not to know that the fulsome praise bestowed upon its virtues is overdrawn. Arsenic is a general tonic, and acts as such in a large number of cases; but I believe other tonics are less harmless and do quite as much good, and perhaps more speedily. Again, the action of baths —saline, mercurial, iodine, acid, and others—of their action, directly or indirectly, upon the skin or the blood or secretions, what do we know? Nothing.

Lastly, How little are the commonest modes of local treatment based upon true science. The action of fatty substances and liquids is very different. Do we ever give a thought as to the influence of oxidation in promoting changes in diseased skin?

Take ordinary eczema: with what object do we use zinc ointment? We apply it all through the disease; but there is an inflammatory stage where warm applications tend to help over the difficulties of the impeded and engorged circulation. There is a second stage—*the secretory*—in which it seems proper that we do not, if the secretion be plentiful, check it too suddenly by local measures, but in the quiescent stage gives light astringents. Then we have the *third* or scaly stage. Upon what principle do we go in treating these? None.

Indeed, sir, these are but two or three remarks that remind us that neither have we any definite ideas as to treatment, in reference to general remedies or the local applications we prescribe. This ignorance is the more reprehensible, since we have the diseased organ under our very eyes, and understand its behaviour *in health* better perhaps than that of any other organ.

I have attempted the unpalatable task of pointing out,—

1. How little attention is paid in England to the subject of skin diseases.
2. How unfair the present state of things is to the student and the public at large.
3. That we are in no little danger of admitting a considerable influence for error, in the wholesale and blind acceptance of foreign views, which are not wholly consonant with the experiences of our own country.
4. That the establishment of some clinique would be a boon to the student and practitioner.
5. That the public has just ground for censuring us in neglecting the study and treatment of a class of maladies that affect the poor very materially and extensively.
6. That skin diseases are intimately connected with profound alterations in the system at large, and form a subject that may fairly engage the attention of the most competent amongst us.

ARTICLE II.—*Case of Traumatic Tetanus successfully treated by the direct Application of Infusion of Tobacco.* By JOHN B. JUNOR, L.R.C.S.E., Peebles.

ON the 21st of August last, I was called to see M. M., a girl of nine years of age, who had got her left leg severely injured by a large cope-stone falling on it.

There was a very much lacerated and contused wound of the calf of the leg, extending from immediately below the knee to beyond the ankle, eight and a-half inches long and above four broad, laying bare the posterior tibial artery in the whole of its length. I brought the edges of the wound together by four stitches; but, as was to be expected, no union took place. In the course of a few days, two large sloughs came away, after which water dressing was applied. Healthy granulations sprung up, the child's appetite and general health continued good, and all seemed to be going on well.

When I paid my visit on the 31st, I noticed a peculiarly anxious expression of face; but as the wound had been newly dressed, I attributed it to pain. Next day, she complained of her jaws being stiff, which excited my suspicions, and I ordered fomentations. When I next saw her, she could open her mouth only to a trifling extent, and the muscles of the neck and abdomen were very rigid. I ordered eight drops of chlorodyne (which was the sedative I used throughout, except on two occasions, which I shall allude to at the end, not to interrupt the narrative) to be given every four hours, fomentations of a decoction of poppy heads to be applied to the abdomen, also linseed-meal poultices, made with the same decoction, to the wound.

The chlorodyne procured a little sleep, but the tetanic symptoms increased. The rigidity gradually extended over the body, and the paroxysms became more severe and frequent.

I now applied a belladonna lotion to the wound, and raised the dose of chlorodyne to ten drops, which produced a decided amelioration of the sufferings; the mouth opening to a greater extent, and the muscles of the abdomen becoming less tense. This, however, did not last long; all the symptoms again gradually increasing in intensity.

On the 5th of September, six days after the first indications of tetanus appeared, I applied to the wound stupes of an infusion of cavendish tobacco, one-half ounce to the pint of water.

The state of matters at this time was as follows: The muscles of the jaws, neck, back, legs, abdomen, and arms, were quite rigid, the legs widely separated and drawn up, and the hands twisted round. The paroxysms were very severe and frequent, causing contortions of the face (the tongue was frequently bitten), violent shaking of the whole body, and difficulty of breathing from spasm of the muscles of respiration. The sufferings of the child were extreme!

Within two or three hours after the application of the tobacco, the improvement was most marked. All the rigid muscles became, to a considerable extent, relaxed; the mouth could be opened about half an inch, the paroxysms were less severe and frequent, and a good deal of sleep was obtained.

The tobacco caused no sickness; but the appetite, which had been hitherto good, completely left the child, and the efforts to induce her to take food or stimulants invariably induced a paroxysm. I had recourse to beef-tea injections.

After applying the tobacco for three days, as the pulse became very weak and the strength was going rapidly down, I went back to the belladonna lotion. The appetite returned, but, unfortunately, the tetanic symptoms became worse also, which caused me again to apply the tobacco (this time one ounce to the pint), with, as on the former occasion, favourable results, but to a greater extent, and without interfering with the appetite. The paroxysms were comparatively trifling and at long intervals; the muscles, however, of the back, legs, and arms continued rigid, and the body, when lifted, was as stiff as a board.

On the 23d of September, as the wound, which had been rapidly healing, was looking rather unhealthy, I removed the tobacco, and applied water dressing. About three or four hours after, I was sent for in great haste, as the parents thought the child was dying. I found her suffering from a more violent paroxysm than she had yet had, suffocation seeming imminent from the spasm of the respiratory muscles. She had been getting rapidly worse from the time of the change of the dressing. I at once re-applied the tobacco, and in the course, I may say, of a few minutes, the paroxysm passed away, and matters returned to their former state.

Ten days after, for the same reason, I ventured to substitute the water dressing, again the paroxysms returned, although not so severe, and again the tobacco dispelled them.

I put a piece of lint, wet with black wash underneath the tobacco cloth, and the wound continued to heal kindly till the termination of the case. The pulse was quick throughout, about 120, but never very weak, except when the child refused to take any nourishment.

About ten days after I began the use of the chlorodyne, as it was paralyzing the expulsive powers of the bladder, I gave tincture of Indian hemp instead, fifteen drops every four hours. This had not the same sedative effect, sleep almost left, and the tetanic symptoms rather increased. In about forty-eight hours, the bladder recovered its power, and I returned to the chlorodyne. On an after occasion, I was again obliged to cease giving it, and tried sol. mur. morph., eight drops every two hours, without any soporific effect. I resumed the chlorodyne as soon as I could, and as it was not necessary to give it so frequently, I did not require to try any other sedative.

After the first eight or ten days, I felt satisfied that the tetanus would not be cured till the wound was healed, and my object in treatment was, to keep the paroxysms as far as possible under control, and maintain the strength of the patient by nourishment and stimulants until that was accomplished. The result proved that I was right. As the wound contracted, the rigidity of the muscles abated; but it was not till the 9th of October that they were sufficiently relaxed to allow the child to sit up in bed. The spasm left the injured limb last. To-day, 30th October, I found her by the fireside, and the wound completely cicatrized.

There can be no doubt that, in this case, the recovery of the patient was due to the tobacco application. The giving up of the use of it was always followed by an aggravation of the tetanic symptoms, which were at once relieved by resuming it.

The suggestion of Mr Tyrrel of Dublin, to produce a large raw surface by blisters or otherwise, in idiopathic attacks, is strengthened by this case.

I tried ineffectually to get nicotine in Edinburgh; but as the remedy was a very severe one for so young a patient, and the case was promising well, I did not think it necessary to send to London for it.

The superiority of chlorodyne over the other sedatives which I used was very decided; and I was so satisfied with it, that I would not have made any change had it not seriously diminished the expulsive powers of the bladder. I find that chlorodyne produces this effect, when taken for a length of time in frequent doses.

ARTICLE III. — *On a Mode of Treatment for Ununited Fracture.*

By JOHN DUNCAN, M.A., M.D., F.R.C.S.Ed.

THE methods of treating ununited fracture are so various that it is sometimes difficult to choose the most safe, certain, and suitable. The more efficacious are dangerous, because they imply the formation of a compound fracture; the less dangerous have the disadvantage of being uncertain in their action. Two of the most simple plans have, however, enjoyed a certain measure of success. Union has not unfrequently followed subcutaneous incision of the connecting medium, as recommended by Professor Miller; and Mr Bickersteth has effected several cures by transfixing the fragments with a simple metallic peg.[1] The former depends for its success on the irritation it causes; the latter, on irritation combined with the absolute immobility which is obtained by this ingenious operation. Their failures on the other hand seem to me to arise from the excitant being in the one misapplied, in the other insufficient. The periosteum is the true point for attack, and to pierce will not always induce it to form a sufficiency of bone.

While residing in Berlin, I was much struck by the result in some of Langenbeck's subperiosteal operations. A resection of the ankle joint more particularly impressed me. The joint had been injured by a bullet in the Schleswig-Holstein campaign, and in its excision as much as was possible of the periosteum covering the malleoli had been scraped up and left *in situ.* The effect of this was the production of two malleoli, so exuberant as seriously to inconvenience the patient. The power of the periosteum to produce bone was of course no novelty to one familiar with experiments on the subject, and the operations for necrosis or cleft palate. But I was astonished at the exuberance of the growth; and the case recurred to my mind and suggested a mode of treatment, when the following ununited fracture came under my observation.

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[1] Mr Paget has recently recorded the cure of an ununited fracture of the humerus by this means.—*Lancet,* 1st Dec. 1866.

To Dr Crawford of Peebles I am indebted for the early history of the case.

On the 1st September 1865, J. V., a working man about 60 years of age, sustained a compound fracture of the fore-arm. He was at work in a saw-mill, when a large portion of a log flew out from the circular saw. It struck his arm, which he raised in self-defence, grazed his forehead, and was broken against the wall. As might be supposed, Dr Crawford found the arm very severely bruised, the ulna largely comminuted, and a lacerated wound opposite the site of fracture. Pasteboard splints were applied, and the arm was laid on a pillow in an easy position. Through an opening in the pasteboard the wound was dressed with water, and it healed in a fortnight with but little suppuration. The splints were then accurately adjusted, and the patient was allowed to rise.

On the 22d of September, the bandages were tightened as the swelling had greatly diminished; but the man feeling pain from the pressure, took down the whole retentive apparatus, re-applied it himself, and presented himself to the doctor on the 28th, with the hand dangling loosely from the sling. The limb was again made fast, and by the 23d of October, or about eight weeks after the accident, the fracture seemed firmly enough united.

He was then advised to exercise the arm,—at first passively, and afterwards by easy work; but rejoicing in his recovered liberty, he indulged occasionally in wheeling barrowfuls of firewood from the saw-mill to his house. Notwithstanding this somewhat rough usage, he experienced only great weakness of the arm, till February 1866, when he also observed that a clicking noise accompanied certain motions. This was at first supposed to arise from roughness in the tendon sheaths; but shortly, diagnosing want of union, Dr Crawford gave the man a note to my father. He delivered it on the 12th of March, having continued to work as he was able till that time.

I then saw the case. On examination it was found that the upper fragment was slightly riding, that certain movements produced the clicking sound, and that, when the muscles were thoroughly relaxed, considerable mobility could be detected. It was thought yet possible that absolute rest, with a firm pad over the fractured part, might suffice to effect a cure. This was explained to the patient, and my father wrote to that effect to Dr Crawford; but in a fortnight the man again turned up, not having gone to the doctor or abstained from using his arm. I now applied the pad with pasteboard and a starch bandage, after freely rubbing the ends of bone against each other. Six weeks of perfect rest produced, however, no improvement, and I accordingly suggested the subcutaneous separation of the periosteum from the bone. As my father thought the experiment worthy of a trial, I inserted a tendon knife at the seat of fracture opposite the edge of the ulna, and scraped up the periosteum for an inch on two sides of the upper fragment. The arm was again put up in pasteboard, and the starch bandage reapplied. In three weeks, when the patient returned to show himself, there was considerable thickening of the upper end, but the mobility was not altogether removed. I again inserted a blunt tenotomy knife, and scraped the periosteum from as much of the circumference of the lower fragment as could be reached from a single puncture. I endeavoured also to repeat the operation on the upper, but found it impossible on account of the multitude of osseous nodules there developed. I therefore contented myself with again applying the starch bandage. A month passed and the man again presented himself, this time with the bone much thickened at the fracture, and perfectly firm. After two or three weeks farther rest, he began to use the arm; and not long ago I received a letter from Dr Crawford, who states that the arm remains perfectly sound.

On the cause of the want of union in this case I have nothing to say. In a man of that age, somewhat wilful and opinionative, non-union is liable to occur under the best treatment, and certainly no better than Dr Crawford's could have been pursued. But I have thought it well to bring the case before the Society, as it seems to me that the subsequent measures might be adopted in a large proportion of ununited factures, either alone or in combination with Mr Bickersteth's operation. The immobility maintained by the metallic peg might be necessary for some, and it might be rendered still more complete by the insertion of two pegs, as was suggested to me by Dr Matthews Duncan. But I am inclined to think that starch or plaster of Paris would be amply sufficient in most cases as it was in this. I used starch because it was at hand at the time, and I could thus allow the patient to return to Peebles by the next train. Had he been resident in town, I would have employed gypsum by preference.

I believe the operation could be safely and easily used in any form of ununited fracture. Even in applying it to the thigh, where the condition is not very amenable to treatment, I can see but little difficulty and no danger. It is not essential that all the periosteum be separated, and subcutaneously that of course is impossible;[1] but many shreds could be scraped up from the bone, and in each of them osseous deposits would take place, and under perfect rest would unite together.

Without farther experience, however, the value of the operation cannot easily be estimated; but I think it may be worthy of another trial. The suggestion rests at all events on sound surgical principles, and has stood the single test to which it has yet been exposed.

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[1] If necessary, a small incision could be made, and one of the usual periosteum scrapers introduced to effect a more thorough separation.

ARTICLE IV.—*Short History of the Outbreak of Cholera which recently prevailed at Slateford, near Edinburgh.* By JOHN BALFOUR, Esq., I.G., Surgeon, Leven.

HAVING been requested by the parochial authorities to aid in combating the recent outbreak of cholera at Slateford, and having been able, by personal inquiry and investigation, to ascertain the complete history of the disease, I am desirous of putting the whole on record as containing many curious and interesting facts.

The first case occurred, on the 11th October, in a man who had been driving esparto grass from the London wharf, Leith, to Kate's paper-mill, a short distance above Slateford, on the Water of Leith. This man recovered. He acknowledged to me that he had suffered from diarrhœa two days before the symptoms of cholera supervened, but assured me that he had not been in any of the wynds or in any suspicious place in Leith where cholera could be supposed to prevail. It must here be noted that there has been no cholera at Kate's paper-mill, so that the contagion was probably not connected with the esparto.

On the 19th October, a woman living in the same block of buildings, and daily passing the door of the above patient, but having no other communication with him, was taken ill, and, after what appears to have been a severe attack, recovered.

The third patient was the daughter of No. 1, a girl of 7 years, who succumbed to the complaint after only 19 hours' illness. She was taken ill about 1 A.M. of the morning of the 20th.

The disease was now fully lighted up, and I may leave its general progress to be made out from the Table which is annexed.—(See pp. 692, 693.)

When subsequently making a house-to-house visitation of the block of buildings inhabited by the majority of those attacked (15 out of 20), I was surprised to find that the houses were remarkably good, far above the average, new, well ventilated, and, upon the whole, very cleanly kept. The drains were stated to be of glazed tile pipe, properly laid, and have since been ascertained to be without obstruction and delivering freely. The water supply, however, appeared to be of a very doubtful character, and the two wells from which it was drawn were at once closed. Plans are annexed of both. No. 1 was that from which all the residents, except Mr Veitch's family, were supplied. It will be seen, that it was within twenty-one feet of the Water of Leith, in this place a very sluggish stream, being dammed-up for manufacturing purposes,—that between it and the river were a dung-heap and two privies, and a few feet farther up the stream was a pig-stye.

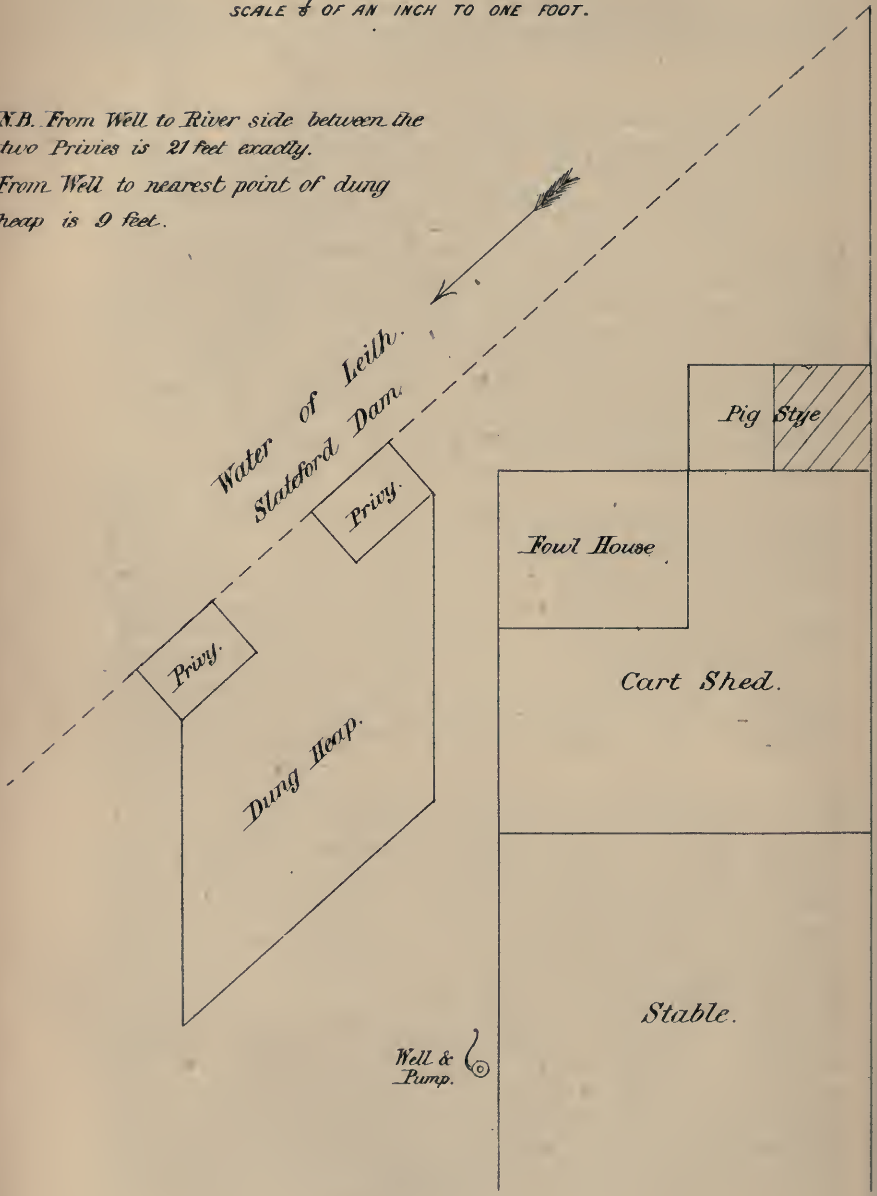
Nº 1.

PLAN OF WELL ON VEITCH'S PROPERTY.

SCALE ⅛ OF AN INCH TO ONE FOOT.

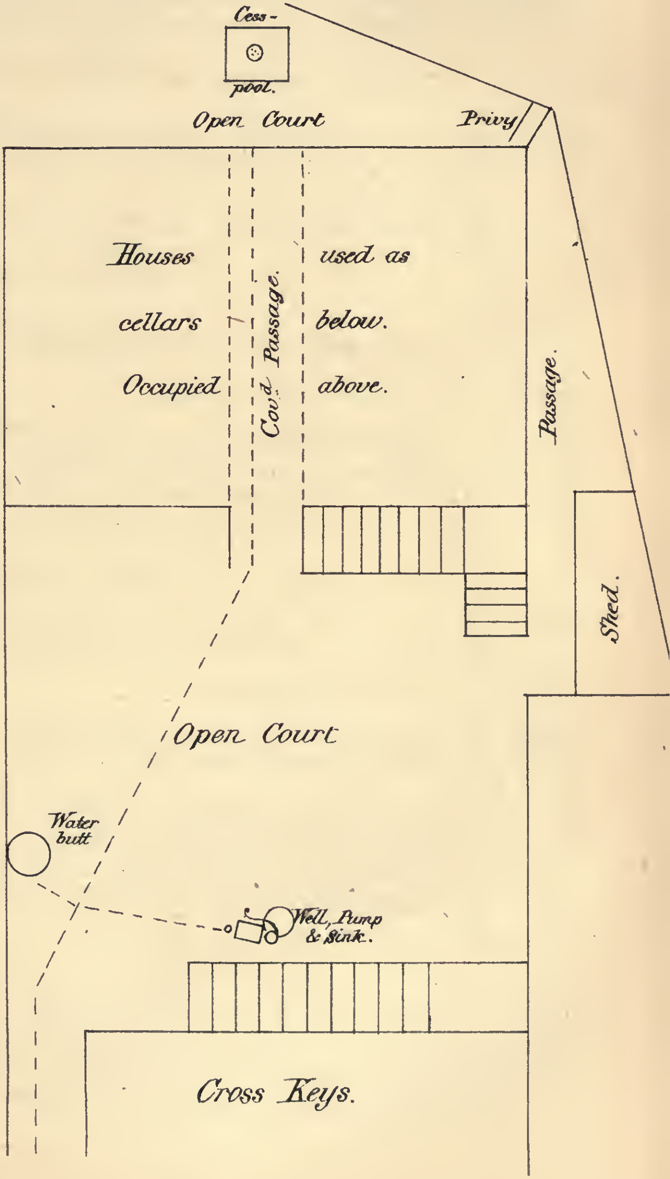
N.B. From Well to River side between the two Privies is 21 feet exactly.

From Well to nearest point of dung heap is 9 feet.



Nº 2.

PLAN OF WELL & DRAINS AT THE BACK OF THE CROSS KEYS.



From the well to the cesspool is 33 feet. The drain from the cesspool comes along the passage of the house behind, on emerging from which it makes a bend & proceeds direct to the passage between the Cross Keys and the next premises (Mr Veitch's property) as it reaches this it is joined by two pipes, one from the rain water butt, the other from under the pump. These drains are said to have been all laid with glazed tile pipe last winter, & to have been puddled in the neighbourhood of the well.

No. 2 is the well at the back of the Cross Keys public-house. There is a cesspool within thirty-three feet of this, and the pipe by which the overflow is carried away passes within eight or nine feet of the well. Samples of both wells were drawn by myself and forwarded to Dr Crum Brown for analysis; and I understand that both have been declared to be so largely contaminated with nitrates that it would have been safer to have used the water of the river itself, though that it is so polluted that no fish can live in it.

Dr Crum Brown's Report, since received, is as follows:—

Analysis of the Water in the Wells at Slateford, by Alexander Crum Brown, M.D., F.R.S., Minto House, Argyle Square.

No. 1. Mr Veitch's well. No. 2. Cross Keys well.

No. 1 contains in the gallon—

Solid fixed mineral matter, ...	68·6 grains.
Organic matter, including nitric acid contained in nitrates,	18·0 „
Total solid matter, .	86·6 grains.

Nitrates and *nitrites* were present in large quantity, indicating that the organic matter is of animal origin, and still undergoing decomposition. The fixed mineral matter consists chiefly of muriate and sulphate of lime and soda.

No. 2 contains in the gallon—

Fixed mineral matter, ...	27·0 grains.
Organic matter, including nitric acid of nitrates, . .	9·2 „
Total solid matter, .	36·2 grains.

Nitrates were present in large quantity, indicating that the organic matter is of animal origin. The fixed mineral matter is of the same nature as in No. 1.

The two waters are both unsuitable for domestic use. No. 1 is much the worst, as containing a larger quantity of total solids, a larger quantity of organic matter, and *nitrites* as well as *nitrates*.

Both waters were pure to the eye, and without sediment.

Alexander Crum Brown.

The foregoing analysis indicates at once the danger that may lurk in water apparently pure, and, coupled with the history of the outbreak of cholera already related, the paramount influence of impure water in localizing an epidemic of that fatal disease.

Table showing the Progress of the Outbreak of

No.	Names.	Age. Yrs.	Date of Attack.	Place of Residence.	Result.	Date.
1.	David Laing,	39	OCTOBER 11, early.	Veitch's Buildings	Recov'd	OCTOBER ...
2.	Mrs Sommerville,	49	19, 2.30 P.M.	Do.	Do.	...
3.	Eliz. Laing,	7	20, 1 A.M.	Do.	Died	20, 8 P.M.
4.	T. F. Pettigrew,	$1\frac{1}{2}$	21, 3 A.M.	Do.	Do.	21, 9 P.M.
5.	Eliza M'Kinnon,	18	21, 5 A.M.	Do.	Do.	24, 9.50 P.M.
6.	Thos. Anthony,	20	21, 9 A.M.	The Barracks	Do.	25, 5.30 A.M.
7.	Alex. Veitch,	39	22, 3 A.M.	Veitch's Buildings	Do.	22, 12.50 P.M.
8.	Mrs Laing,	38	22, 9 A.M.	Do.	Do.	23, 5 A.M.
9.	Mrs Kinnaird,	55	22, 6 P.M.	Inglis' Green Cottages	Do.	23, 8.50 P.M.
10.	Annie M'Donald,	7	23, 8 A.M.	Veitch's Buildings	Recov'd	...
11.	Andrew Veitch,	4	23, 4 P.M.	Do.	Died	24, 5 A.M.
12.	Agnes Veitch,	41	23, 4 P.M.	Do.	Do.	24, 5 P.M.
13.	David Bennet,	42	23, 5 P.M.	Do.	Do.	24, 4.30 A.M.
14.	John Pettigrew,	40	24, noon.	Do.	Do.	27, 4 A.M.
15.	John Forbes,	31	24, 6 P.M.	Do.	Do.	25, noon.
16.	Andrew Veitch,	37	25, 7 A.M.	Separate locality	Do.	25, 11.30 P.M.
17.	Elizabeth Swan,	29	26, 6 A.M.	Veitch's Buildings	Do.	28, 3 A.M.
18.	Janet Veitch,	14	26, 7.30 A.M.	Do.	Recov'd	...
19.	John Hutton,	14	26, 7.30 A.M.	Inglis' Green Cottages	Do.	...
20.	Mrs Robinson,	45	27, 5 A.M.	Nurse in hospital	Died	27, 3 P.M.

Cholera at Slateford in the Month of October 1866.

REMARKS.	No.
Diarrhœa for four days previous to attack. Did not get out of bed on morning of 11th. Had been driving esparto grass from Leith: no rags for months. Had been on the London wharf, Leith, but in no suspicious locality.	1.
Diarrhœa for two days before attack, but kept at her work.	2.
Daughter of No. 1. State of bowels not known.	3.
Ill for eight or nine weeks with whooping cough, and latterly bronchitis. Much reduced.	4.
Lived in same house as No. 2. Complained of sore throat the day before attack.	5.
Servant of No. 7, and having occasion constantly to be near and about the affected houses. Ill with diarrhœa on 18th, 19th, and 20th. Was convalescent, but partook of a full meal of sausages, which caused relapse.	6.
Master of first patient, and owner of block of houses chiefly affected. Ailing with sore throat some time before attack, but said to have recovered. No previous diarrhœa.	7.
Wife of first patient. State of bowels not known.	8.
Unconnected with other patients, but in a state of terrible alarm. At 6 P.M., on hearing of the death of a young woman, had diarrhœa. Got a glass of brandy, and afterwards took tea. Vomiting and purging set in at midnight.	9.
Diarrhœa several days previous to the attack.	10.
Son of No. 7. Whooping cough for some time. Diarrhœa several hours before attack.	11.
Wife of No. 7. Bowels reported costive previous to attack. During attack, pretty free purging, but almost no vomiting.	12.
Diarrhœa at least two days before attack.	13.
Diarrhœa for several days previous to attack. Worse on 24th, but went in to Edinburgh, and returned vomiting and purging.	14.
Diarrhœa at least four hours previous to seizure.	15.
Brother of No. 7, and living in a separate locality, but of course much among his relations. Greatly alarmed. Seen on the night of 24th, when he was dozing under the influence of opium. Drank largely of cold water at 2 A.M., causing uneasiness and borborygmus. Took lead and opium pill. Seen at 3 A.M., when there was nothing appreciably wrong. Decidedly ill by 7 A.M.	16.
Sister-in-law of No. 13, with whom she chiefly lived, though she had also another house. Was attacked on the 24th, but was doing well. Left the hospital unnoticed on the night of the 24th. Very ill on readmission.	17.
Daughter of No. 7. Diarrhœa the day before. Moderated by lead and opium pill.	18.
Complained of headache the day before attack. At night peeped into hospital window, and got a fright. Bowels became affected about 11 P.M. Mother gave a little brandy and laudanum.	19.
Nurse in hospital. Was the worse of liquor on 26th, and had to be spoken to repeatedly. Attended one of the patients part of the night, and was fairly ill next morning at 5 A.M. Little vomiting or purging.	20.

I have now to note one or two singular facts, and one is, that although the inhabitants of the Cross Keys used greatly contaminated water, and lived next Mr Veitch's property—there being only a narrow passage between the two houses—yet no case of cholera, nor even of diarrhœa, occurred among them.

Another singular fact is, that though several cases of cholera occurred in other parts of the village (*vide* Nos. 6, 9, 16, 19, and 20), yet in no case was there more than the one case of cholera in the house—the disease did not take root; and some of these localities, especially that called The Barracks, consists of houses of the worst construction, old, very ill ventilated, and fearfully overcrowded.

The influence of fright in aiding the disease is strikingly exemplified in No 9, where a woman, apparently in perfect health, on being told of the death of a companion, changed countenance, was at once copiously purged, and died in about twenty-seven hours. It is probable that fright also had much to do with the attack of No. 19; but as he had been peeping into the hospital window, that is not quite so certain. I was assured by the husband of No. 9 that she had not been near the affected part of the village, or had held any communication whatever with any of those seized.

Another case occurred, in addition to those recorded in my table, at the village of Bogsmill, about half-a-mile to the west of Slateford. It was treated successfully by Dr Gordon, the parish surgeon (who laboured unremittingly at Slateford during the early part of the outbreak, until quite worn out); but not having been seen by myself, is only alluded to as another instance of the disease having been imported into a village without any further spread.

I should perhaps also allude to Mrs Robinson's case (No. 20), a nurse. She was, of course, employed in attending on the sick; but during the whole of the 26th, she remained in a house where a death had occurred, and where she got a large supply of spirits. I was much displeased at her conduct, and would have sent her back to Edinburgh that night, but she was not in a fit state to proceed. She was seized with the worst type of cholera about five A.M. next morning, and died in ten hours.

The type of the disease throughout the outbreak was of the most severe form. The most striking symptoms being failure of the animal heat, with blueness of the skin, and sinking of the pulse. Cramps, vomiting, and purging were all generally present in a greater or less degree, but by no means to a severe extent; and the patients sunk as if blood-poisoned.

In addition to the most common methods of treatment, I tried, in two cases, hypodermic injection of a solution of the chlorate of potash, first, I believe, recommended by my friend, Dr Sutherland of Patna, in Bengal. The cases were 11 and 14; and in both it appeared to be of some service, especially in the second; where the clammy perspiration dried up, the skin became warm, the pulse, after having ceased at the wrist, became distinct, and I had, for a couple of days, much hope of saving the case; but he went back, and ultimately sank, in spite of all my endeavours. In case any one should wish to try this plan, I may state that I used a nearly saturated solution cold, and injected about one ounce at a time, being compelled, however, from having only the usual hypodermic syringe, to make eight punctures for each ounce. The injection was repeated every two hours. I think that something may be made of the hypodermic method, as the drugs used in this way are rapidly absorbed, and, of course, produce their effect on the blood and system.

During the outbreak, I treated a large number of cases of diarrhœa, chiefly of a choleraic character; and I believe that more good was done by immediate attention to these than by actual treatment of the cases which had fairly lapsed into cholera. I carried the medicines with me always, and, by day or night, was ready to dispense them, or visit any one reported ill, at a moment's notice.

I also enforced at once the order for destroying all contaminated bedding and clothing, and succeeded in having it effectually carried out, which had scarcely been done before my arrival, from the difficulty in getting people to obey the orders which had been given. This, I consider, had no little effect in staying the progress of disease. I also did all in my power to establish confidence, and put a stop to the continual dosing with whisky or brandy, which I found to be extremely prevalent, even with generally sober men, in consequence of their being possessed with the idea that they could thus fortify themselves against disease.

A somewhat singular case of non-contagion occurred near the end of the outbreak. Early on the morning of the 28th October, a man who had been drinking hard all the week, and who had had an extra dose on Saturday, after sleeping for hours in a common stair, and then wandering for some time outside, until he was wet to the skin, and thoroughly chilled, came to the hospital door, declared that he was very ill, and must be admitted. On seeing him almost immediately, I at once made out the case; but, however anxious to get rid of him, there was no possibility of doing so. I had him therefore undressed and surrounded with hot bottles. Next day he suffered much from vomiting, but evidently only from the effects of the debauch. Sinapisms and sedatives were of use. The vomiting was stayed. He ran nearly into delirium tremens, but escaped that also, and was discharged in about a week recovered, though still by no means strong, and promising amendment of his habits. One would think that this man did everything in his power to bring on an attack of cholera, yet he escaped.

ARTICLE V.—*Case of Aphasia with Right Hemiplegia; Death; Autopsy; Lesion of a large portion of the left Cerebral Hemisphere, including the External or Inferior left Frontal Convolution; Embolism of Branches of the left middle Cerebral Artery.* By R. E. SCORESBY-JACKSON, M.D., F.R.S.E., Fellow of the Royal College of Physicians, Physican to the Royal Infirmary, Lecturer on Materia Medica and on Clinical Medicine, Edinburgh.

AS in the last number of the Journal, I have again merely to record a case which presents some interesting features. It does not require any introductory remarks.

James A., æt. 48, married, farm-servant, native of Scotland, resident in Mid-Lothian, was admitted into Ward VII. of the Royal Infirmary on the 20th of September 1866. He was sent from Chalmers' Hospital, in consequence of having disturbed the other patients there, and was at first admitted into No. X.—the strong ward of the Infirmary—as an unmanageable patient; but in two days he was transferred to No. VII., where he remained until his death, which occurred on the 22d of October. The following history and description of the case was chiefly written by my friend Dr Roberts Thomson, then the resident physician in charge of my wards, now of Bournemouth.

History.—Patient has generally enjoyed excellent health. Last spring he complained a good deal of pain in the small of the back, but a plaster which was worn afforded much relief. In June last, he began to drive a milk-van into Edinburgh; and one day, in the early part of that month, he was thrown out of the van into the street. He received two wounds, but both were very slight; one over the right olecranon, the other on the left temple—upwards from the outer angle of the orbit. This accident did not prevent his working, but ever after he complained of pain in the head—a pain which was described by his wife as a general headache, not confined to any particular spot. This pain was seldom absent, but varied in intensity, being always worst in the morning. Before the accident he used to complain sometimes of a pain in the back of the neck, and this has been much worse since the fall. In this condition he remained, doing his work constantly, until the beginning of August, when one morning he was suddenly seized with the hemiplegia and aphasia, from which he still suffers.

The history of the attack, so far as it can be ascertained from his friends, is the following: He left home between four and five o'clock in the morning in charge of a cart, and travelled to Edinburgh, a distance of five miles. He reached town in his usual health; and, having an hour to wait, went to the house of a friend, who observed nothing at all unusual about him. Some coffee was offered him, which he began to drink; but whilst doing so, his friend observed him raise his right arm as high as his head.

Having done this, he arose and rubbed his right leg, all the while muttering something which could not be understood. He then fell on the chair; and, finally, on the ground, on his left side. On attempting to lift him, it was found that the right arm and leg were quite powerless. There was no loss of consciousness at any time during the seizure.

A medical man was called, who advised his immediate removal to the hospital. He was taken to Chalmers' Hospital, where he remained five weeks, and was then transferred to the Royal Infirmary.

State on admission.—Circulatory System.—Cardiac action is very irregular and tumultuous, but at the same time weak. The sounds are altered in character, but no distinct murmur is audible either at base or apex. The radial pulses are equal in strength. Pulse averages 75, irregular, weak, compressible. *Respiratory System.*—The respiratory murmur is feeble generally over the chest, but is unaccompanied by any dry or moist sounds. There is no dulness on percussion, nor is there any cough or expectoration. *Nervous System.*—There is loss of motion on the right side, both of the upper and lower extremities. The right side of the face is also slightly paralyzed. There is apparently, also, impairment of sensibility on that side, but this it is very difficult to determine. The arm and leg on the paralyzed side are in a semi-flexed position, the hand is pronated, and the fingers are flexed on the palm, but not rigidly. Great pain seems to be experienced whenever the arm is moved, however gently it may be done, and whether the movement be one of flexion or extension. When asked to put out the tongue, he opens his mouth, but cannot protrude the organ. It lies deep in the mouth, in the mesial line, and is not drawn to either side. Once he protruded it slightly for about a second, but immediately it slid back to its usual position. When asked to nod his head, patient turns it from side to side; and when told to move it up and down, he either does not move it at all, or still turns it from side to side. The head can be rotated apparently without effort, and this is invariably done when the nurse is preparing his food, in order that he may see what she is doing. The only articulate sounds which he can produce are "No," "Yes," and "Oh dear;" the first is most frequently used, the second occasionally, and the third only as an expression of the sense of pain when the paralyzed arm is moved, or when the nurse is dressing his sores. Once or twice other words have been pronounced: thus, his wife one day asked him if he "would like a smoke," to which he replied "sometimes." The word "sair" (*i.e.*, sore, or painful) has also been uttered several times. Yes and No seem to be used indiscriminately in reply to questions. When Christian names were repeated to him, and he was asked to claim his own, he assented to several, but he never said no to "James." When asked his age, he assented to every number up to seventy. The irides are very sluggish; the right pupil is somewhat smaller than the left. The senses of hearing and sight seem to be unimpaired. *Integumentary System.*—Patient is considerably emaciated. There are several large bedsores; one over the right elbow, one over the sacrum, and one on the back to the right of the vertebral column, about the level of the eighth rib. *Digestive System.*—Tongue moist, and covered with a slight fur; appetite very great; no dysphagia; considerable thirst. Bowels open every day; the evacuations are passed in bed, no attempt being made by the patient to communicate his wants to the nurse. No increase of hepatic dulness. *Genito-Urinary System.*—Urine is apparently of average quantity, but much of it is passed in bed. It is of an amber colour, and acid reaction; it contains no albumen, very slight phosphates, and the normal quantity of chlorides.

Was ordered nourishing diet, with six ounces of sherry; strict attention to be paid to cleanliness, and suitable dressings to be applied to the sores.

1*st October.*—Since admission, patient has been very much in the same condition. His appetite is as great as ever, but he does not gain strength. Motions still passed involuntarily. The bedsores are not healing. There is no change in his attempts to speak; "No" is the exclamation most commonly used.

10*th.*—Patient is weaker, but in every other respect the same as when last reported. The bed has to be changed many times a-day; for, although the patient seems to make no attempt to prevent the excreta passing into the bed, he, nevertheless, feels uncomfortable when it is soiled, and moans till he is changed.

15*th.*—The bedsores are more unhealthy-looking, and patient seems to suffer more from them. Moaning, as an expression of suffering, is more frequently heard, and there are oftener the signs of anguish upon his face. Paralysis and loss of speech as before.

18*th.*—Patient seems to be suffering more than ever. Last night the other patients in the ward were kept awake by the groans which escaped him. To have a draught at bedtime, containing fifteen minims of solution of hydrochlorate of morphia.

19*th.*—Has not slept; moaned a great deal. Face is very sunken-looking. Eats and drinks as usual. Ordered twenty minims of the morphia solution.

20*th.*—Slept almost the whole night. To-day seems weak, and does not care to make any exertion. Flies crawl unnoticed over his face, which is often contorted with an expression of pain. More sunken-looking than yesterday.

22*d.*—Last night draught was repeated, and patient spent a very fair night. At morning visit, he was found lying on the back, with the eyes closed; breathing soft, but a little laboured. Pupils semi-contracted. When spoken to, he paid little attention, and scarcely seemed to hear his name when pronounced. Said "ay" and "no" to one or two queries which were put to him, but most of them were unheeded. Considerable mucous rales in both lungs. Ordered a draught containing fifteen minims of tincture of belladonna.

7 P.M.—On entering the ward at this hour, the resident physician found that the patient had been dead for three-quarters of an hour. The nurse and patient's wife agree in stating, that from mid-day visit patient gradually sank. The breathing became very stertorous and laboured. On a question being put to him by his wife, a little while before his death, he answered "ay," but after that said nothing, nor did he attempt to communicate with those around him.

Autopsy.—The body was examined seventeen hours after death, weather rather mild. The examination was conducted by Dr Grainger Stewart, pathologist to the Royal Infirmary: Dr Sanders, Mr Turner, Dr Thomson, and myself being also present. The brain was examined only externally at that time, as it was thought from the appearance and quagginess of the left hemisphere, that it would be better to harden it in spirit before cutting into it. The examination of the brain was resumed on the 26th, by Dr Stewart, Dr Sanders, Mr Turner (to whom I desire to offer my thanks for his careful and accurate anatomical description of the parts concerned), Dr Thomson, and myself; the investigation was most carefully conducted, and Dr Thomson on both occasions made notes *pari passu* with the examination. The following is Dr Grainger Stewart's report of the autopsy: "The body was moderately well nourished. The pericardium was natural. The heart was of natural size; its muscular substance pale, its valves somewhat thickened at the margins; on the mitral valve there were numerous vegetations, and loose fibrinous masses. Both lungs were adherent and congested throughout; the right was in a state of solid œdema. The liver was congested; many of its cells fatty; it weighed 3 lbs. 12 oz. The spleen was pulpy and enlarged; weighed 8 oz. The kidneys together, weighed 8½ oz.; in the left there were several conical masses in different stages. Some were deeply congested and solidified, others were fawn-coloured, with a red margin; while a third, corresponding to a depression on the surface, was more white and fibrous. The organ was otherwise natural. In the right there were a few minute red patches, which appeared to have resulted from minute embolisms. The stomach and intestines were natural.

The skull-cap was natural. The dura mater was firmly adherent to it. The arachnoid was at some points thickened; there was a little milky fluid in the cavity, but this appeared to have escaped from the brain in the act of removal. The vessels of the base were atheromatous at several points, but there was no embolism, excepting in some of the branches of the middle cerebral artery of the left side. There were at several points of division decolorized, firm clots, and from them in the line of the current there extended dark solid coagula. The outer surface of the left hemisphere was flattened, and distinctly fluctuating on pressure, in an area extending from between two or three inches in front of the fissure of Rolando to the posterior boundary of the parietal lobe, and from within an inch of the great longitudinal fissure to the inferior margin, and round to the base. At the base it extended to the tip of the temporo-sphenoidal lobe, but very slightly affected the inferior temporo-sphenoidal convolution. On opening up the left Sylvian fissure, the opposed surfaces of the hemisphere were found softened, and of a yellowish colour. The insula was so softened that the sulci were obliterated, and the convolutions could not be defined. This condition and discoloration extended from the insula to the inferior frontal convolution, and embraced the whole of its posterior extremity, extending backwards through the ascending frontal convolution, and as far back as the supra-marginal and angular convolutions. The corresponding parts of the right side were natural. Over the diseased surface the membranes were so connected with the substance of the brain that they could not be removed without tearing it.

On cutting into the brain, the extensive fluctuating and collapsed portion of the left hemisphere was found softened, the degree of softening varying in different parts; in some a creamy-looking fluid, in others of a consistence like that of switched cream, and in others presenting the characters of fawn-coloured softening. It was most advanced in the anterior part just above the insula, and the inferior frontal gyrus. The posterior part was of the consistence of pulp. The softening extended from the insula and the inferior frontal convolution as far inwards as the corpus striatum, the whole of the extra-ventricular portion of which was involved. The anterior end of the intra-ventricular portion also presented some softening. The superior and middle frontal gyri, the upper ends of the ascending frontal and parietal gyri, the posterior parietal lobule, and the anterior end of the inferior frontal gyrus, were not softened. On microscopic examination, the softened parts were found to contain innumerable compound granular bodies, and numerous flakes of red and reddish-brown pigment. The right side of the brain was healthy."

Remarks.—In the history of this case we have two starting points, either of which we may assume to have been the primary cause of the fatal changes which took place in the left side of the brain-substance,—heart-disease, and the accident which caused the wound on the left temple. It is not in all cases necessary to look for a remote cause of cerebral embolism,—for this, it can scarcely be doubted, was the immediate precursor of the cerebral lesion,—since whatever produces retardation of the circulation tends to solidification of the blood and the occlusion of vessels; so that even a roughened atheromatous condition of the vessels of the brain may alone be sufficient to give rise to plugging and consequent arrestment of nutrition of the brain-substance. The vessels at the base of the brain were in this case atheromatous at some points, but not nearly to the extent met with in hundreds of cases in which there has been no arrest of cerebral circulation; besides, if the coagulation of the blood had been due to that local cause only, it is probable that the trunk of the middle cerebral artery would have been occluded. This of itself, then, is not likely to have been the sole cause of the cerebral lesion, although, as I shall state presently, it may have been accessory. Moreover, atheromatous degeneration of the vessels of the brain could be merely a suspicion in the history of the case, as it was unattended by any of those objective signs whereby we pronounce such changes probable.

The first point that attracts our attention in the history of the case, then, is the precipitation from the milk-van, early in the month of June, whereby the patient received two injuries, one on the right elbow, the other on the left temple. These wounds were both of slight character, and possibly may have had nothing whatever to do with the subsequent alarming symptoms; but the accident is not on that account to be dismissed without consideration, and that for several reasons. In the first place, it matters little in the present case, perhaps, except as a subject of pathological interest, whether the accident were the cause of the brain-softening or not. Here we have simply a poor man driving his master's cart, from which he was suddenly ejected, possibly by his own carelessness, and there was nobody to blame. But, suppose the case had been that of a first-class passenger who had received precisely similar wounds in a railway accident, and that these wounds had been followed in two months by right hemiplegia and aphasia, and by death in two months more, what would the "medical expert" have said in the witness-box with reference to cause and effect? Would this not have been strong evidence for the bereaved family—"This accident did not prevent his working, but ever after he complained of pain in the head—a pain which was described by his wife as a general headache, not confined to any particular spot. This pain was seldom absent, but varied in intensity, being always worst in the morning. Before the accident he used to complain sometimes of a pain in the back of the neck, and this has been much worse since the fall. In this condition he remained, doing his work constantly, until the beginning of August (*i. e.*, two months after the accident), when one morning he was suddenly seized with hemiplegia and aphasia." Whatever might be said of the claimant's evidence in the case of a plea for *solatium*, no one can suspect the poor widow in this case of having exaggerated her husband's symptoms, and yet, moderately as the statement is made, there does seem to be a close connexion between the fall from the milk-van and the death of the patient, at least in the relation of time, if not in that of cause and effect.

But, again, although the external wounds were apparently trivial, a very serious internal injury might have been produced, even if the external wound had been imperceptible. About a year before the admission of this case, a man was brought to the same ward, under the following circumstances, briefly:—On arriving at the ward, I was told that a case of apoplexy had been brought in, and that the man, his friends said, had been subject to fits. When I approached the bedside, I recognised a former gratuitous epileptic patient, and I was surprised at his state, which was doubtless one of pure apoplexy. I felt convinced, however, that the hæmorrhage was not of spontaneous origin, and consequently I examined his head carefully for a sign of external injury. On the right side of the head I found a small scalp wound situated behind the ear and above the mastoid process; it might very properly be called a *slight* wound. This confirmed my belief that the patient had been injured by a fall in one of his fits, producing traumatic apoplexy. I immediately consulted my colleague, Dr Gillespie, on the surgical aspect of the case; he saw the patient at once, and, after examining the wound, very naturally concluded that surgical interference was unwarrantable, as there was no indication of fracture. The patient died seven hours afterwards. An examination of the body was made by Dr Grainger Stewart, from whose report I extract the following:—"The external table of the skull was not fractured; there was a slight fissure of the internal table, for about an inch along the line of the groove for the middle meningeal artery, and at this point there was a little effusion of blood into the diplöe. A clot, weighing 4¾ oz., lay between the dura mater and the skull; the dura mater was not ruptured, nor was there any hæmorrhage into the arachnoid cavity. The clot was opposite the fracture and the external wound."

Here, then, was the case of a man who, whilst making a purchase in a shop, fell down in an epileptic fit, striking his head against some hard substance either as he fell or on the ground, producing thereby traumatic apoplexy, from which he died. In this case the symptoms followed the accident immediately, and there could be no doubt as to the relation of cause and effect; there was speechlessness, but, of course, it was that of profound stupor, and not such as we understand by aphasia. I mention this case, not because it resembles the one at present under consideration, but simply as an illustration of the necessity for adequately estimating the importance of even *slight* wounds of the scalp. What the symptoms would have been in this case if the fracture of the vitreous table had not wounded a vessel, but had merely been sufficient to irritate the membranes in its neighbourhood, I do not know; not improbably some such pain in the head as has been described in the history of A. One other point I may venture to refer to before leaving the case of traumatic apoplexy. After his fall in the shop, and whilst still in a state of insensibility, the man was put into a cab and jolted home, a distance of three miles; two days afterwards, he was put into another cab and jolted to the Infirmary, three miles more. These were most unfortunate circumstances; had the man been at once brought to the hospital, which could have been done in five minutes, from the place where the accident occurred, his chance of recovery would have been vastly increased. No practice can be more vicious than that of unnecessarily moving an apoplectic patient, whether idiopathic or traumatic. But in the case of James A., the post-mortem examination did not discover any remains of a traumatic lesion, and it would be very difficult to connect the wound on the temple with the softening of the brain.

With reference to the wound over the right olecranon, it would have been unnecessary to say more, had that not also been followed by a serious consequence. It is curious, that whilst the situation of the wound on the temple was close to the cause of death, the wound on the elbow became the seat of a very painful bedsore, whereby, in common with the others, the patient's last days were made wretched. Whether the pain which was expressed by the look of anguish and the utterance of the words, "Oh dear" and "sair," when the right arm was moved, was entirely due to the sore over the olecranon, it is difficult to say; and also whether or not the flexed position was assumed merely as a protection or for the relief of that sore. Similar expressions of pain are commonly heard in cases in which, with contracted limbs on the hemiplegic side, there is neither aphasia nor bedsore. A case under my care, in the same ward, last summer,—in which there was left hemiplegia, with an indisposition, but not an inability to speak, and in which the paralyzed arm was flexed and rested upon the chest as in the present case, but with no bedsore on the arm,—exhibited precisely the same look of anguish, uttering at the same time an expression of pain, whenever the arm was moved. An autopsy of this case showed softened patches in the right hemisphere.

We pass, then, from the milk-van accident to consider the second starting point, the heart disease. There was an alteration in the heart's sounds, with a probability of mitral regurgitation, but this was difficult to determine, for he suffered so much from his bedsores that it was inexpedient to move him, and therefore the sounds were always feeble, as he lay constantly on his back and without exertion. The heart's action was very irregular, so that instead of the ventricular walls pressing evenly and steadily upon the blood, no doubt there was a sort of splashing or churning movement. The post-mortem examination showed us that "on the mitral valve there were numerous vegetations, and loose fibrinous masses." The next question for consideration, then, is whether a clot, or several clots, had been formed in the circulation of the blood, whether they found an impediment to their progress in the branches of the left middle cerebral artery, and whether some of these clots were also carried to the kidneys, in which there were post-mortem signs of embolism. I leave this as a query; because to endeavour to solve it would involve the whole question of embolism. The only remark that it is necessary to make is this, that circumstances were eminently favourable to the formation of blood clots, namely, the slow, irregular feeble action of the heart, and the roughening of the blood passages by vegetations and atheromatous deposits; and here it is that the atheromatous patches in the cerebral vessels may have been accessory to the formation of coagula, that is supposing that the obstructive particles were formed in the circulation, and were not detachments from the mitral valve. In order to work out this question satisfactorily, it would also be necessary to know the state of the heart previous to the milk-van accident, as well as during the two months after it, of which we have no history.

Another interesting point in this case is the situation of the cerebral lesion, with reference to the localization of the faculty of articulate language. There is nothing in the case calculated to refute M. Broca's opinion, nor, on the other hand, does it definitely, so far as a single case can do, establish the theory; but it affords one remarkable, though not conclusive, support to it. M. Broca's position I understand to be this, generally, that lesion of the third, inferior, or external left frontal convolution involves the loss of articulate language, or otherwise, that the cerebral faculty of speech exists in, and is confined to, that convolution. Now, in the case of A., although much more of the hemisphere was implicated in the disease,—and therefore it is not an exact corroboration of Broca's views,—it is certain that the particular convolution referred to was involved in the lesion: but more than that, M. Broca states that lesion of *the posterior third* of that convolution annihilates articulate language, and in this case we find that *the anterior end of the inferior frontal gyrus was not softened*, so that there was, positively, true aphasial speechlessness, and destruction of the posterior third of the third, .external, or inferior frontal convolution, whilst the anterior part of it remained intact. Hence one of three things may be surmised,—either that the anterior and posterior parts of the external left frontal convolution are not identical in function, or that the posterior part of the convolution being disorganized, the anterior part, although still retaining its vascular connexion, is cut off from its nervous communication with the parts over which it was destined to preside; or finally, that the functional activity of this convolution, beginning at its posterior extremity, is increased, in proportion to the acquisition of language, by an extension of its power towards its anterior end, so that whilst in the case of an illiterate man only the posterior part of the convolution would be in a state of activity, in the case of a linguist the entire convolution would be employed. It may be that a man acquires the ordinary power of speaking and writing his own language by the aid of the posterior third of his left frontal convolution, that by exercise in the art of speaking and writing, more and more of the convolution is utilized, whereby he not only acquires commensurate ease and elegance in the use of that language, but, moreover, adds to it other languages, dead and living. This is at present, of course, mere speculation, but that physiology and pathology are leading us to some such discovery is more than probable. There are hundreds of cases on record, in which the facts of forgetfulness of language, either native or foreign, or of forgetfulness of parts of a language, as of names, nouns, or numbers, is mentioned without any explanation being attempted, or, indeed, possible until physiology or pathology had studied the question of the relative capacities of healthy and disorganized brains. To select one case out of many, Dr Beattie, quoted by Dr Abercrombie, mentions the case of "a gentleman who, after a blow on the head, lost his knowledge of Greek, and did not appear to have lost anything else." Where was that gentleman's Greek deposited that it could be blotted out by a single stroke, whilst his native language and all else remained?

Another point to be noted in this case, as in most cases of aphasia, is, that from beginning to end there was no loss of consciousness, in which circumstance there is a distinction between such cases and those of hemiplegia with apoplexy. Hemiplegia is, however, not an essential accompaniment of aphasia. In some cases the hemiplegia which attends aphasia, and which is almost invariably right-sided, is only temporary; its permanence and completeness in this case are explained by the extent to which the corpus striatum was involved in the lesion.

Another point which it is important to observe in this case, is the extensive lesion in the island of Reil, a fact which lends support to the view suggested by my colleague Dr Sanders (*vid. Lancet*, June 16, 1866), namely, that the insular gyri may be the seat of the supposed faculty of language, or cerebral organ of speech.

It might be asked, since he could not protrude the tongue, was the speechlessness not due to peripheral paralysis of that organ? Although he could not, or, except on one occasion that was witnessed, did not protrude the tongue, he could move it in the mouth, and did so whenever he uttered the words, *yes*, *no*, *dear*, and *sair*: besides, he could have spoken tolerably well without using the tongue if that had been the only obstacle, for he had command of his lips, and when he was moved in bed the expression of pain was observed all over the face, including the somewhat paralyzed right side.

The loss of the memory of words was in this case as nearly as possible complete. There was no vain babbling, no transposition of words, no misnaming; *yes*, *no*, *ay*, *oh! dear*, and *sair*, were really the only words that he could command, and of these the three former appeared to be used with so little discrimination, that the attendants attached no value to them. It was difficult to estimate the intelligence of the patient, for he could neither express his thoughts in words, nor, in consequence of his bedsores, by gestures, at least with comfort. My impression, however, is that his intelligence was probably but slightly impaired, and I have little doubt that he comprehended all that was going on around him. There was no emotional language, as distinguished from intellectual language; I believe the patient never used any of the explosives or other emotional expressions so well described by Dr Hughlings Jackson in his admirable papers on aphasia in the *London Hospital Reports*, the *Ophthalmic Journal*, and other medical publications. He would never repeat any word that was suggested to him, even if he had voluntarily pronounced it immediately before, except the word "no," which he employed much more frequently than any other, and would sometimes appear to repeat, if urgently requested to do so, though in reality it was probably nothing more than his answer to each request. The word was always uttered in an indifferent tone, never as an emphatic negation. I believe no attempt was made to get him to write, his very helpless state from paralysis and pain of the bedsores being quite sufficient, independently of the aphasia, to prevent that.

ARTICLE VI.—*Hæmorrhage from Fibrous Tumour of the Uterus: its Nature, Source, and Surgical Treatment.* By J. MATTHEWS DUNCAN, M.D., Honorary Member of the Hunterian Medical Society.

(*Concluded from page* 641.)

I NOW proceed to consider what can be done for the diminution or arrest of the various bleedings I have described. And I premise that, in this place, only large, grave, dangerous, and recurring hæmorrhages are considered. It is only in such that surgical interference is at present even proposed.

I shall only enter upon such operations as I have myself performed. The gouging of Mr Baker Brown I have never resorted to. Hitherto, it has appeared to me a less satisfactory operation than the partial or complete avulsion which I shall hereafter describe. Farther, I must take care to inform my professional brethren that I now merely describe what I have done, tentatively, seeking for the relief or safety of grievous sufferers. I have no matured plan of proceeding; indeed, in the case of Miss C. (Case XIV.), I do not know exactly what I did, except that I removed one small tumour and part of another, and that the patient was thereby virtually cured. I describe my proceedings with the view of contributing to the progress of this department of surgery. The cases, in which I have performed the graver and more difficult operations, were such in severity and present danger as to justify almost any proceeding that a well-informed practitioner could devise. And I have the good fortune to be able to say that the surgical or other interference, which I have attempted or completed, has been done without the least permanent injury of the patients. So far as I know, every woman on whom I have operated or attempted to operate for fibrous tumour, still lives.

Speaking of spontaneous or natural elimination of fibrous tumours, an eminent obstetrical author says,—" Attempts have been made by Amussat, Maisonneuve, Atlee, and others, to imitate by the knife this process of spontaneous or natural elimination; but we believe it to be, from the cases which we have seen, an operation that, as at present practised, will very rarely indeed succeed, and which ought to be very rarely, if ever, undertaken."[1] My own views are opposed to the tenor of those just cited. I am disposed to regard as the proper line of practice, in those cases of fibrous tumour which involve the patients in great and imminent danger, and whose evils medical treatment is unable to avert, to apply such surgical resources as we seem to possess. These resources are various: each may itself prove efficacious or sufficient: or one may lead to another, till, fortunately, the last operation is accomplished, the removal of the offending tumour. I shall describe these operations in succession as separate operations; but if the whole history of a protracted and inveterate case is held in view, they may be regarded as successive steps in the same operation, complete removal of the enucleated tumour being the final result. In this respect they may be compared to the successive steps of a difficult craniotomy case, each of which may, in itself, prove sufficient; namely, the operation of perforation, of the crotchet, of removal of the calvarium, of artificial extraction of the fœtus.

Cases, which have come under my observation, appear to me to justify the opinion that surgeons have looked for too rapid attainment of the final removal of the tumour; that, while in some cases, enucleation may possibly be performed at a sitting or in the course of a few days; in other cases, it may, without disadvantage, occupy months or years. An opening is artificially made in the uterine wall or forms spontaneously,—the analogue of a rigid os uteri; the process of expulsion, spontaneous or artificially assisted, goes on slowly, the analogue of a tedious labour. It is natural to suppose that the denudation and slow expulsion of a considerable tumour should be very dangerous, apart from the loss of blood accompanying the process. Experience appears to me to negative this opinion. But even were it otherwise, art would not be quite without resources, for, as Lisfranc has suggested, the enucleated portion,[2] or successive portions, may be removed, and the remaining immovable part waited for. I have operated in cases (of which IV., V., and VI. are examples) where the tumour had been for a long time, partially, or almost completely, enucleated, and in which there was little bleeding, little discharge of any kind, and little constitutional disturbance. The case of Miss C. (Case XIV.) affords an example of the partial removal of a fibrous tumour, with only beneficial results. Describing a case in which incision of the os uteri seemed to be useful in restraining hæmorrhage, Dr M'Clintock remarks,[1]—"On three different occasions, Dr Denham and I, assisted by Dr Churchill, Dr Halahan, and Dr J. R. Kirkpatrick, cut away portions of the tumour. The last of these operations was followed by some uterine tenderness, and slight febrile disturbance. Calcareous matter was found in the portions of tumour last removed. So far it does not appear that any benefit of a curative kind has resulted from these measures; but her health is so far improved that she has been able to follow her business as superintendent of a large millinery establishment. The menstruation is considerably in excess, but otherwise there is no hæmorrhagic discharge. The fibroid seems somewhat increased in size."[2] In the *Medical Times*,[3] Mr Jonathan Hutchinson relates a case in which he removed several portions of a fibrous tumour (not having ordinary characters), in size from a walnut to an orange, the case coming again to demand treatment. It is only fair to add that Mr Hutchinson, who has paid attention to the matter, regards[4] the process of spontaneous enucleation (the encouragement of which we recommend in certain cases in this paper) as having attendant risks probably as great as when the surgeon interferes. The cases already related in this paper, and its whole tenor, are, I hope, sufficient to produce the conviction that Mr Hutchinson attaches far too great danger to the process. There can be no doubt that it is often extremely dangerous, as numerous recorded cases, and some which I shall myself relate, show. But I am sure that many recorded cases proved fatal because the surgeon did not assist the spontaneous expulsion in a bold and opportune manner. If surgeons had distinguished false or spontaneously enucleated uterine polypi from true fibrous uterine polypi, the prevalent opinion as to the great danger of spontaneous enucleation would not have come to be entertained.

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[1] Simpson's Obstetric Works, i. p. 117.

[2] He says (Maladies de l'Utérus, p. 512), " C'est encore cette circonstance d'organization du polype qui fait que, lors même que la section ou la ligature ne peuvent pas être portées jusqu'à la naissance du pédicule, on ne doit pas pour cela renoncer à l'opération. On enlève le plus possible de la tumeur, le reste se détache ou tombe en suppuration; . . ."

As I have already intimated, the operations which I have practised all lead on or tend to enucleation and complete separation, as the ultimate result, reached or hoped for, if arrest of bleeding is not otherwise attained. As this result has attendant dangers, the operations are only to be practised in the gravest cases. Nature may be said to resort to this as her own method of cure. In my experience, a fibrous tumour more or less completely enucleated has been very much more common than a true fibrous polypus. I have studied the process of enucleation in every stage, in cases spontaneously and in cases artificially separated. I have never seen nor do I know of any observation of the, nevertheless frequently described, change of an interstitial fibrous tumour, or one embedded in the uterine wall, into a proper fibrous polypus. I may therefore be allowed to doubt whether such an occurrence frequently or ever takes place, and to hold, in the meantime, that a true fibrous polypus is probably more or less so from the beginning of its history.

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[1] Clinical Memoirs on Diseases of Women, p. 152.

[2] Dr Sims records a case in his Uterine Surgery (p. 113), where cutting off a part of the tumour was followed by death. But he ascribes this event to the hæmorrhage (apparently meaning not that produced by the operation). It certainly was not due to anything consequent upon the operation. M. Amussat alleges (p. 13) that a similar result occurred in a case operated on by M. Velpeau. M. Courty (*Traité pratique des Maladies de l'Utérus et de ses Annexes*, p. 832) regards leaving a part of the tumour as fatal. In this he is evidently wrong, as many cases testify. He says,—"Quel que soit le procédé auquel on a recours, il faut s'efforcer de terminer l'extraction. Une pareille opération est funeste si elle reste incomplète."

[3] *Medical Times*, July 25, 1857.

[4] *Ibid.*, August 15, 1857.

This opinion does not involve so great a difference from current views as might at first sight appear. For a submucous fibrous tumour, or one having a layer of muscular tissue covering it, as it grows bulging into the uterine cavity, may, from the first moment of its history, be more or less polypoid, that is, having at least a major part of its bulk projecting from the general contour of the uterine wall into the distended uterine cavity. The descent or increased projection of such tumours may be a part of what authors mean to describe when they speak of a fibrous tumour becoming a polypus. This change I willingly admit, but I do not consider the description of it referred to as an accurate description. So far as such cases go, I agree in substance, though not in terms of description, with the ordinary accounts. It is in regard to the alleged change of an embedded tumour into a true polypus that I desiderate evidence, having, in my own experience, never witnessed any such process, and having never read a carefully detailed report of its occurrence.

The anxious practitioner can have, in any case, little hope of benefit from medical treatment pursued with a view to diminish, or produce absorption of, the tumour (not mere diminution of it, such as may result from absorption of œdematous infiltration). Indeed, while it is certain that absorption of a fibrous tumour is extremely rare, there is room for doubting whether such an event ever happens. I may here mention that I have been, as it were, forced against my judgment, by the evidence of a single very remarkable case, to admit the possibility of the complete removal of a large fibrous tumour by absorption.[1] During its progress the patient took small doses of iodide and bromide of potass almost constantly for years; but extensive expe-

Case VII.

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[1] For cases of absorption, see M'Clintock's Clinical Memoirs, p. 134; see also Aran, Maladies de l'Utérus, p. 858, for a case of Scanzoni's. Cases of atrophy and absorption after delivery require separate study, as occurring under special conditions. Several cases of this kind are on record. See also, Courty, *Ibid.*, p. 828.

rience with these remedies does not lead me to attribute to their use this singular good result. The tumour was as large as the fœtal head at the end of pregnancy. It was as easily and perfectly diagnosed as any case could be. There was no doubt ever thrown upon the nature of the case by any of the experienced practitioners who examined it. It had every character and symptom of a fibrous tumour. The patient was long in the most aggravated state of anæmia. Now, there is as certainly no uterine tumour, as there was certainly one formerly. The only method of escape, for me, from the evidence of this case in favour of complete absorption, is, the supposition that the tumour may have become spontaneously enucleated, separated, and discharged, without the consciousness of the patient. This alternative, I confess, considering the cleanly habits and careful truthful character of my patient, seems more unlikely than the other. Whatever may be the truth regarding this individual case, every one will admit that it is unreasonable, in the present state of therapeutics, to expect absorption of a fibrous uterine tumour.

There is another termination of a fibrous tumour, worthy of study in connexion with surgical interference, namely its disintegration, decomposition, and expulsion,—a process described with care by Dr West and others. This is an uncommon ending of a case of this tumour. I have seen more than one example of it, but I believe it to be much rarer than enucleation. I agree with West and M'Clintock in regarding the process as not inflammatory in any stage, so far as the tumour is concerned.[1] It is this termination that Mr Baker Brown seeks to attain by his gouging operation. As I have already said, I have never practised this kind of interference, preferring in the meantime to push my efforts in another direction. But I must remark, that this treatment, as illustrated by Mr Baker Brown's cases, published in the Transactions of the Obstetrical Society of London, demands the studious attention of the practitioner.

Dilatation of the Cervix Uteri.

This is the first operation. It may be resorted to with a view to examine the uterine cavity, and the relations of the tumour.

It may be done by the knife or scissors. This method is applicable only when the tumour, or its lower part, is at or near the os uteri, the cervix being developed as in the end of pregnancy. The scissors is best used without any other instrument. The knife, having its edge blunt or guarded up to near its point, may be used in the same way; or a common straight bistoury may be used through a speculum exposing the cervix, or with the aid of the duck-bill speculum of Marion Sims. The extent of the cutting varies; sometimes one, sometimes two fingers sometimes even the hand can be passed through the artificial opening. The wounds so made may heal up, to a great extent at least. Little bleeding follows this operation.

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[1] Amussat describes the appearances of an inflamed fibrous tumour. See p. 7 of his Mémoire sur l'Anatomie pathologique des Tumeurs fibreuses de l'Utérus et sur la possibilité d'extirper ces tumeurs lorsqu'elles sont encore contenues dans les parois de cet organe. See also, West on the Diseases of Women, third edition, p. 266.

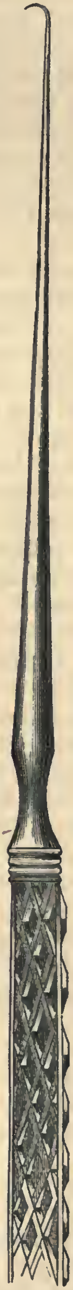
Mr Jonathan Hutchinson says, without qualification, that the danger of hæmorrhage from incision is very slight. But I cannot view the matter so, believing that if, in cases adapted for dilatation by tents, incisions are used, then profuse hæmorrhage may occur, an event to be most carefully shunned. Dr Marion Sims gives a good example of this bleeding in his work on Uterine Surgery (p. 111).

The dilatation is best effected by tents, when the upper part of the cavity of the cervix uteri is not opened up as in the end of pregnancy. Sea-tangle tents are to be preferred, and sometimes they produce all needful opening of the cervix. But when a large amount of dilatation is desired, I have found tangle-tents inefficient, and then, after the tangle-tent, a common conical sponge-tent of suitable size is to be resorted to.

These operations have frequently appeared to me to have, for one or for many succeeding periods, a decided effect in abating the amount of the bloody discharge. Speaking of the use of sponge-tents, Mr Grimsdale remarks,—"They might, as I had observed in several similar cases before, produce such an altered condition or action of the lining membrane of the cavity as should for a considerable time, probably many months, check the tendency to hæmorrhage."[1] Similar opinions as to the beneficial effects of incision of the os and cervix have been published by Baker Brown and M'Clintock.[2]

Before leaving this topic, I have to mention an ingenious suggestion by Marion Sims, which I have found sometimes, in cases of the kind now under consideration, but still more in cases of other descriptions, to be very valuable, especially when the introduction of the first tents (or of a probe) is difficult or, without it, impracticable. It consists in fixing or gently pulling one lip of the uterus by a delicate tenaculum hooking the margin of the cervix. (See woodcut.) It is held by an assistant. Dr Sims uses his duck-bill speculum in performing

Fig. 3.



Tenaculum for fixing Cervix Uteri.

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[1] Liverpool Medico-Chirurgical Journal, vol. i. p. 68.
[2] Obstetrical Transactions, vol. vi. p. 22.

the operation. It can be as well done without it. The tenaculum is used without causing pain; indeed, the patient seems unaware of its application.

The following case (VIII.) illustrates the advantage of dilatation of the cervix. It is interesting on account of the cure of the bleeding, and, to the patient's feeling, the removal of the tumour. It is chiefly extracted from my ward case-book.

CASE VIII. Mrs D., æt. 48, has been twice married, and had a child ten years ago. She began, three years ago, to suffer from large discharges of blood, per vaginam, which have returned monthly. At the same time she first observed a tumour in the lower belly, and it appeared to her to be as large then as it is now. Latterly, the bloody discharge has been almost constant for some months.

She is excessively anæmic, complains of ringing in the head, palpitation and want of breath. Auscultation discovers an anæmic bruit in the heart and great vessels. There is in the lower belly a rounded hard tumour, rising half way up to the umbilicus and extending from one inguinal ring to the other. The uterine bruit is well heard over it. Per vaginam, the whole brim of pelvis is felt to be occupied by a hard rounded mass, which combined external and internal examination identifies with the tumour in the hypogastrium. A probe introduced shows the uterine cavity to be behind the tumour.

Dr Duncan, by means of a sponge tent, largely dilated the cervix and lower part of body of uterus, with a view to incising the tumour; but this was found impossible, as, by no manipulation could he reach a proper part of the tumour, a vesical sound always showing a sort of diverticulum of bladder between his knife and the mass.

Subsequently to these manipulations, and under the use of mineral acids and ergot during the continuance of bloody discharge, of iron and quinine in its absence, she became so greatly improved as to demand her dismissal, after about a month's residence in the hospital. After six months she returned, looking well and robust, but complaining of rheumatism. She said her discharges had ceased and that the tumour was gone. An examination discovered the tumour still present, but apparently so diminished in bulk as to be in the brim of the pelvis, so that it could now be only with difficulty felt by pressing the hand firmly above the pubes. Per vaginam, it was easily found.

It is now two years and a-half since she last came to the hospital; and she promised to return, should any of her disorder again appear.

Incision of the Tumour

Is resorted to after dilatation of the cervix. This operation appears to me to be useless except while the tumour is still covered by uterine tissue, enucleation not having commenced.

It is done by an ordinary sharp-pointed bistoury, guarded by lint or otherwise, except for half-an-inch at the point; and the duck-bill speculum may be used or not according to circumstances. While the operation is performed, the uterus may be fixed by pressure from above, or by the use of a hook or tenaculum fixed in the cervix uteri. The operation is performed through the dilated cervix uteri. An incision is made of depth and length according to the size of the tumour, and the possibility of safely cutting, without implicating neighbouring parts. I have made incisions which I judged to be an inch in length, and at least half-an-inch in depth. I have not seen alarming hæmorrhage follow the operation.

I believe these incisions generally heal up, to be opened again, in some cases, by ulceration, should enucleation take place. This healing up may lead to obliteration of some of the sinuses, and so to a restraint of hæmorrhage, for a time at least. In the case of Miss M. (Case X.), I believe that the incisions made did not heal up, but that the tumour was very quickly propelled against the opening, so as to commence enucleation at once.

I have preferred incision of the tumour to destruction of its covering by caustics, as has been repeatedly practised.[1]

Incision of the uterus and tumour has been often made the first step of the operation of enucleation, which has been proceeded to forthwith or after brief delay. The plan I at present am disposed to prefer, is to make the incision or incisions and, trusting to their beneficial effects, to wait for a period of months or longer, unless the case is extremely urgent or presents characters peculiarly inviting immediate farther interference.

The operation, like the former, is sometimes followed by diminution of the hæmorrhages. I have not seen Dr Atlee's writings on this subject, and I can only give an extract at second hand from them, showing his plan and opinion, both of which appear, to say the least, to be very imperfectly described. "Dr Atlee (it is said) has described another procedure by which he says the hæmorrhages attendant on these tumours have been 'invariably arrested instantaneously;' and this method is, 'during hæmorrhage, to pass the bistoury along the vagina into the cavity of the uterus, and make a very free incision into the most exposed portion of the tumour."[2] The words, in which the result of the operation is described, are very improperly used, and diminish the value of Dr Atlee's statement.

The next case illustrates the use of this method of treatment.

Case IX. A.W., æt. 34, had a child when seventeen years of age. During the last twelve months she had been losing, per vaginam, a great deal of blood. For the last six weeks it has come away in very great quantity. She is now very pale and anæmic, and so weak as to be scarcely able to walk without support. She suffers much from bearing down and weakness in the back. Examination, per vaginam, at once shows the presence in the uterus of a small fibrous tumour.

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[1] See Simpson's Obstetric Works, vol. i. p. 118.

[2] M'Clintock, Clinical Memoirs, p. 152.

The cervix uteri was dilated by a sponge tent, with a view to the more thorough investigation of, and, if necessary, interference with the tumour. It was found further necessary to cut through the posterior lip of the cervix to obtain access to the tumour. A deep incision, not above an inch in length, was made into the tumour from above downwards.

She left the hospital two months afterwards. There had been, up till that time, no bleeding beyond ordinary menstrual discharge. She believed herself cured, and promised to come back if her floodings returned upon her. She has not been seen again.

Avulsion of the Tumour.

This operation will be easy, in proportion to the amount of the tumour already spontaneously enucleated. Cases are on record in which the operation has been performed without any assistance from nature being waited for. They have been, on the whole, very unfortunate, and my proposals do not imply imitation of this rapid proceeding. It is to this kind of treatment that Baker Brown probably refers, when he says,[1]—"The enucleation of the tumour is an operation so fraught with immediate danger, as well as with the risk subsequently of pyæmia, that I have long since discarded it." The operation of enucleation and avulsion is one frequently carried on spontaneously, and merely completed by art. Of this, Cases IV., V., VI. are instructive examples. It may be merely begun by nature, and effected mainly by art. Of this, Cases XI., XII., and XIII. are examples.

It may be effected by art in all its stages, as in Case XIV., there being, in such, no enucleation antecedent to the artificial enucleation and avulsion.

In a case of probable partial enucleation, there is a question difficult to decide, namely, whether enucleation is really taking place, or whether the tumour is still covered up by a layer of proper uterine tissue. It is important to be able to decide this question, and I know of no simple method of doing it, when roughness of surface and disconnexion of lobes do not indicate it. But I have, in the cases here narrated, proceeded upon the following assumption, an assumption which I think further experience will verify. A tumour bulging, but not partially enucleated, will everywhere, at its junction with the general body of the uterus, present smooth rounded outlines, as at a, fig. 5. But a tumour partially enucleated will project angularly from the general surface of the uterus, as at a, fig. 4.

It appears to me that some advance or propulsion of a partially enucleated tumour will probably form, along with other signs and considerations, such as the size of the tumour, a valuable indication of the feasibility of its removal by avulsion. For unless the tumour is every where surrounded by a comparatively loose layer of fibrous tissue, its movement of advance,[1] will be difficult or impossible.

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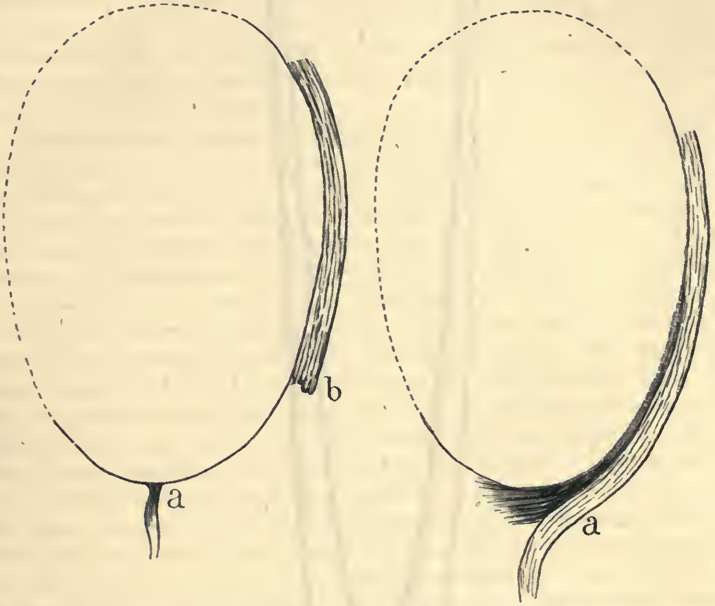
[1] Obstetrical Transactions, vol. i. p. 328. I wish here to add that Mr Hutchinson (*loc. cit.*) prefers the primary enucleation to the slower plan, or secondary enucleation, as he calls it. He is guided by the results of a comparison of collections of cases. But I regard his very valuable tables as composed of too heterogeneous materials to form a good basis for his conclusion.

Fig. 4. Fig. 5.



Showing the angular bulging of a partially Enucleated Tumour, compared with the bulging of one still embedded.

The operation is performed by seizing the tumour with a strong volsella (see fig. 6.), of which the operator should have more than one pair, so that, when he feels his grasp yielding, he may anew seize the tumour without quite losing hold of it and its slipping back. The tumour is to be dragged; and I have applied all the force which the hold of the instruments would stand in some cases, or which the texture of the tumour would bear without tearing in pieces, in another. I have attached whip-cord to a partially extracted tumour, to aid in the pulling. The pulling should be without jerks, and rapid progress of the operation should not be desired. Sometimes, at the end of the operation, a careful twisting of the tumour may be resorted to, when inversion of the uterus is

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[1] To give more precision to this statement, I here subjoin that, as examples testify, a great amount of denudation or enucleation may take place without any movement of advance such as would indicate propulsion of the tumour. This absence of advance may arise from the distal side of the tumour being covered by peritoneum alone, a circumstance which would render artificial enucleation very dangerous if not certainly fatal. See also Courty, *Ibid.*, p. 830.

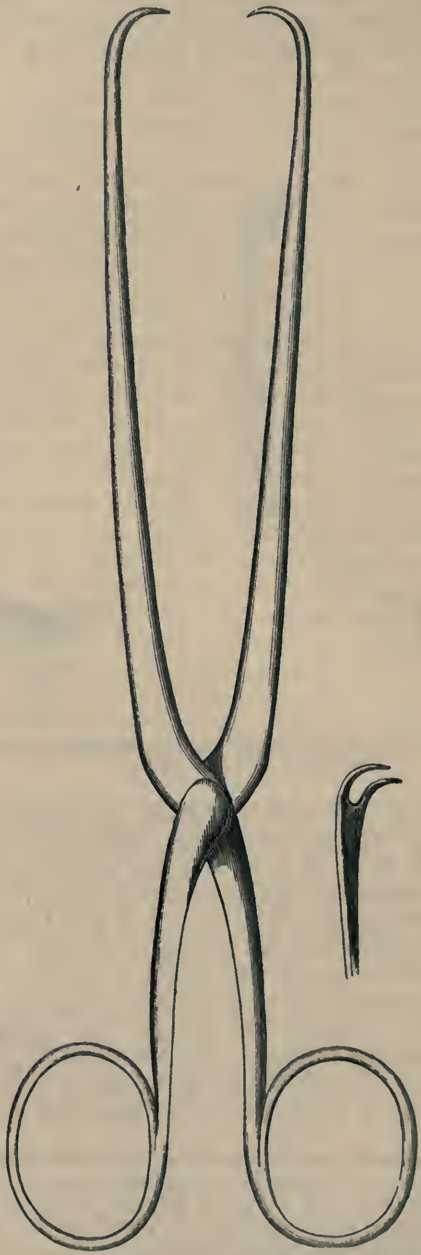

FIG. 6.—Volsella for Avulsion (ten inches in length).

feared, such twisting making an addition to the separating power without increasing the tendency to inversion.[1]

Sometimes the operation is completed by the curved scissors, as in Case XIV., and others. Sometimes the fingers are used in its progress to repel the uterine wall, or assist the enucleating process. Sometimes dragging alone, as in Cases XII. and XIII., completes the operation. In cases of enucleation, authors speak of a pedicle or peduncle, and of dividing it; and I have followed their example. But there is truly no stalk or pedicle, only the remaining attachments of the tumour, which are sometimes drawn out, stalk-like, but only inconsiderably so.

I have not seen hæmorrhage follow the operation. The difficulty of completing it, especially in cases of bulky tumours, sometimes lies in the perinæum (as in Cases VI. and XIV.) The perinæum and orifice of vagina have to be dilated as in labour. An eminent practitioner has recently informed me that, in a case of this kind, in his hands, laceration of the perinæum backwards and through the sphincter ani was produced. Dr Marion Sims records a case in which he incised the ostium vaginæ to permit the extraction of a tumour of this kind;[2] as also Dupuytren.[3] When the tumour is removed, it often is easy to tell, by the character of its surface, how much has been enucleated in the operation of avulsion, and how much had previously been separated. The last enucleated part presents, as Dr Hall Davis describes, a perfectly fresh appearance. When washed it is yellowish white, and attached to it may be found shreds of muscular tissue. The older enucleated portions are somewhat shrivelled, reddish brown, cannot be washed white, and have appearances such as are characteristic and would be expected, but can scarcely be described. The differences have some remote analogy to those observed in a case of unavoidable hæmorrhage, between the earlier and the most recently detached parts of the placenta.

I now proceed to further illustrations of the practice recommended in this paper. In the following case, incision of the cervix and of the tumour were simultaneously practised. The wound in the uterine wall, investing the tumour, seems not to have healed, for the tumour immediately began to descend. Its progress at first was rapid, and was a very interesting subject of observation. Adhesions appeared to form between the enucleated portion of tumour and the adjacent uterine wall, and were repeatedly broken down, to be found again formed on the next examination. The case is chiefly from the ward case-book. Judging from Dr Duncan's letter concerning the case, I expect to be able to remove the tumour when she next comes under my care, spontaneous enucleation being now apparently far advanced, and so much so as to appear to me to show the possibility of its artificial completion.

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[1] See Dr Hall Davis' description of a case: Obstetrical Transactions, vol. ii. p. 21. Amussat has described this twisting or rotation carefully: Mémoire sur l'Anatomie pathologique des Tumeurs fibreuses de l'Utérus. p. 68.

[2] Uterine Surgery, p. 113.

[3] Clinique Chirurg., tom. iv. p. 316.

CASE X. J. M., æt. 38, is unmarried, and has had good health till about a year ago. Then her menses began to be irregular, blood being almost constantly discharged. Sometimes, she says, her loss has amounted to a flooding. She has only recently felt a tumour in the lower belly.

Above the pubes a hard mass, as big as the fist, can be felt. Examination, per vaginam, discovers the os uteri in its natural situation, and nearly of natural size. The cervix uteri is flattened out or opened up, no projecting portion being found. The whole lower part of uterus is a large, tuberous, mobile mass. A uterine probe enters the cavity three inches.

Dr Duncan incised the cervix on either side with a sharp-pointed bistoury. The finger, now easily passed into the uterine cavity, discovers bulging from behind, but not acutely or angularly, a large mass. The lower part of this tumour, opposite the os uteri, was also incised. Little bleeding and no notable suffering was produced by the operation.

Repeated subsequent examinations discovered a gradually increasing descent of the tumour. It projected more and more from the uterine wall, angularly, and descended more and more upon the still expanding cervical portion of the uterus.[1] At the same time it gradually became difficult to feel the mass by pressing the hand over the pubes.

Eight days after the operation, an attack of metritis came on, but was easily subdued.

When she returned home (against my advice), the tumour had so descended that the finger could be passed for at least an inch between it and the posterior uterine wall, while in front lay the uterine cavity.

A little more than a year and a half after the operation above described, my friend Dr J. A. Duncan visited J. M. at her home, and kindly sent me the following report:—"She has had several great losses of blood. The os uteri is much dilated, and the tumour projects through it about half an inch, like a side of an orange. It is much increased in bulk, being felt in the hypogastrium of the size of a child's head."

In the following case, spontaneous enucleation and expulsion, into the uterine cavity, of one of a set of fibrous tumours, was nearly completed before the patient came under my care. Although tumours were left in the womb, the improvement of symptoms and diminution of discharge, consequent on the operation described, were very remarkable. An intra-uterine fibrous tumour, with a broad sessile attachment, was removed.

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[1] At each examination, adhesions were broken down, joining the enucleated mass to the adjacent uterine wall. Such adhesions are described by M'Clintock; and Dr Sims (p. 123) gives a case in which they were found post mortem.

CASE XI. Mrs R., æt. 54, has had no children. She has, for above fifteen years, been weakened by much suffering in the lower belly and frequent floodings. When the floodings are absent, she has constant copious watery discharge. She has consulted many eminent physicians in the diseases of women, but has obtained no distinct benefit. About five years ago, an eminent London obstetrician told her that he would be satisfied if, five years thereafter, she were alive. For the first two months of this year she had unceasing bloody discharges. She is very weak, but does not appear to be anæmic, judging by her face; her pulse is about 80, and moderately firm.

Two small elongated mucous polypi are hanging from the cervix. The os uteri is slightly patent, and the tip of the finger, pressed into it but not traversing it, reaches something presenting above it. The uterus is very bulky, and with its tumours is indistinctly felt, through the abdominal wall loaded with fat, occupying a considerable portion of the belly above the pubis, and especially on the left side. A probe enters the uterus three and a-half inches.

Dilatation of the cervix was effected by first using tangle tents, and then a sponge tent as big as the index finger. This operation occupied a day. The tumour was then seized by a strong volsella and pulled down, great force being used. It could not be brought through the cervix without that part being cut through in three places by a probe-pointed bistoury guarded by lint. After it had passed the os uteri, difficulty was again experienced at the os vaginæ, but this was overcome by a few minutes of patient dragging. Its enucleation was completed without any further interference by knife or scissors. It measured two and a-half by two inches, and its examination showed that, during the operation, only its base had been detached from the uterine wall. At this part some long shreds of uterine muscular tissue adhered to it.

The operation was not very painful, and nothing occurred to interrupt her recovery. The mucous polypi were removed by avulsion. Other tumours were left in the uterus, but so high up as scarcely to be in any way reached by the examining finger. After about five weeks, she left Edinburgh, having been for a fortnight at the end of that time without any discharge either bloody or serous.

In the next case, enucleation was little more than well begun when the operation was performed. No previous incision or other operation had induced the commencement of enucleation, which was therefore spontaneous, so far as it had gone, before my seeing the patient. The case is from the ward case-book, and is one of removal of a fibrous tumour, not having a broad sessile attachment, but still almost entirely embedded in the uterine wall.

CASE XII. Mrs L., æt. 33, married, has had two children, the last four years ago; enjoyed good health till two years ago, when her monthly losses began to be excessive in quantity, and often of long continuance. This state endures, and the loss of blood has been frequently very profuse. She is in a state of great anæmia, very weak, and has much pain in the lower parts of the back and belly.

Dr Dow of Carnock, who sent the patient to Dr Duncan, reports that, at one time, a tumour presented at the os uteri, that then the hæmorrhage was very copious, and did not lessen till the tumour retreated into the uterine cavity.

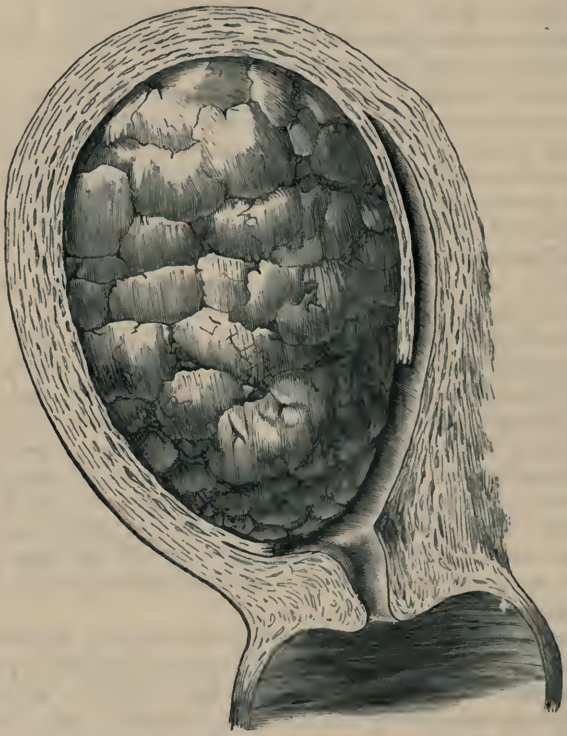

FIG. 7.—Plan of the Uterus and Tumour in Case XII.

Examination, per vaginam, discovers the os uteri patent, admitting the finger. A tumour can be felt, projecting acutely or angularly at its lower margin from the posterior wall of the uterus in which its mass is embedded.

A strong volsella, whose blades were separately applied as in the midwifery forceps, was securely fixed in the lower part of the tumour. Powerful traction was exerted, and simultaneously a slight twisting motion was given to the volsella. The circle of the os uteri, offering an insuperable obstacle to the descent of the mass, was divided by a probe-pointed bistoury. The tumour was now further dragged down into the vagina, and was found to have been deprived of its attachments, except at its base, where some sawing by the finger-nails was required to detach it.

After its removal, which was effected with only an inconsiderable loss of blood, the large rough-walled cavity in the posterior wall of the uterus, from which the tumour had been forcibly torn, was distinctly felt. In the evening preceding the day of operation, two doses of ergot of rye were administered to the patient, and seemed to be the cause of pains in the back during the night.

The tumour is dense, smooth on the surface, and having only one considerable shred of the proper uterine tissue attached to its base. It is oval, measuring 3½ inches in length, and six inches in circumference at its broader and upper end. The part detached before the operation can be easily recognised. It comprises only one-sixth part of the mass.

This woman's next monthly period was moderate in amount, and she left the hospital in improved health.

In the following case, there can be no doubt that, without the operation, life could not have been preserved. The tumour here also was, for the most part, still embedded in the uterine wall when operative interference was resorted to.

Case XIII. Mrs ——, the mother of a family, above 40 years of age, had, about three years before the events now to be recorded, been under my care. She was suffering very greatly in health from uterine hæmorrhages. The uterus was occupied by a considerable fibrous tumour. At this time I dilated the cervix fully with sponge tents, but did not discover anything unusual, or such as to encourage me to interfere further in the case, otherwise than by medicinal treatment. Since this time the history of her floodings has varied much, but in the months preceding the operation to be described it was extremely unfavourable.

Writing to me in January concerning an attack of hæmorrhage in this lady, Dr Adamson of St Andrews says,—"The polypus projected into the vagina nearly the length of my forefinger. I got my finger up by its side so as to reach the thin edge of the os uteri grasping it firmly, and I had to desist from exploring it all round, because it required considerable pressure to get my finger far enough up to feel the os, and the pressure caused great pain of the back and was attended with strong effort to vomit. From the history of this attack of flooding, which was preceded for some time by regular uterine pains like labour pains, and which suddenly ceased with the occurrence of a gush of blood, it is my belief that the polypus was protruded by regular uterine contractions, and that it has been gradually retracted. You will see her after the next period."

The next period was as full of danger as the former two, and Dr Adamson was in fear of an immediate fatal result of the hæmorrhage. He told me he could not expect her to survive another such loss, and the subsequent history of the case shows that his fears were well founded.

In the end of January she came to Edinburgh. Her anæmic condition was extreme. Examination, per vaginam, revealed the presence of a large uterine tumour. The os uteri admitted the tip of the examining finger, and the tumour could be felt above it. Menstruation commenced soon after her arrival, and the loss was quite inconsiderable. The blood discharged was thin and watery, and it ceased to flow on the 2d February. At this time she was allowed to get out of bed; she was feeling pretty well and had no pain. During the period she took in the course of each day thirty grains of gallic acid and a drachm of the liquor of ergot. This last was given with the hope of obtaining the descent of the tumour.

On the 3d February, copious bloody discharges began, and she was at once plunged into the most alarming condition. She had frequent faintings while lying in bed. She suffered no pain resembling that of uterine contraction, but, on examination, the tumour was now found in the vagina, bulky and fully occupying the passage. It had descended as the renewed discharge commenced, for an examination on the 2d February showed the os uteri to be in the condition described as existing at the time of her arrival in Edinburgh.

To give time for preparing the operation, and also to allow of some rallying of the vital powers, should the means be successful, a plug of lint wetted with solution of perchloride of iron was inserted. It proved efficient against the bleeding.

Next day the plug was removed, and the operation proceeded with under the most unfavourable circumstances, the patient being almost pulseless at the wrist. Dr Graham Weir assisted me.

The plug was removed, the tumour seized by volsellæ and dragged. After many efforts and much laceration of the tumour, it appeared to us almost hopeless to proceed. The os uteri could with the greatest difficulty be reached by the finger, and the uterus was partially inverted. It was impossible to say certainly where the tumour ended and uterine wall began;[1] yet some indications could be made out, and at these places the fingers were used to aid in separating the tumour and in pushing back the uterine wall. At last, after laborious perseverance, the mass came away. The hole from which it was excavated was in the anterior wall of the uterus, and presented a very ragged surface. Very little blood was lost in the operation, and no plug was used after its completion.

This patient made a slow but uninterrupted recovery. A copious purulent discharge flowed for nearly six weeks. This I attribute to vaginitis caused by the plugging. She never again has had uterine hæmorrhage. Fifteen months after the operation she had a miscarriage.

The tumour measured 5 inches by 3. It was not very dense in its structure, having numerous lobules. Several shreds of muscular uterine tissue were attached to it. On the surface of the tumour, and between the lobules, large and freely communicating venous vessels were seen. The largest were about one-sixth of an inch in diameter, contained clots, and opened on the presenting part of the tumour. It was easy to make out that somewhat more than a half of the mass had been separated from its uterine nidus in the operation.

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[1] Lisfranc, in his Maladies de l'Utérus, p. 508, relates a fatal accident which occurred in one of his operations, from a mistake, the result of a similar difficulty.

The following case presented such extreme urgency, that I operated without having any reason to believe that enucleation had commenced, and the examination of the tumours after separation showed that they had not been denuded at all. I find it difficult to account for their successful removal, and prefer giving little account of it.[1] It is, however, almost certain that the volsella had got fixed in the tumour, and had at the same time lacerated an opening in the uterine wall for its transmission, and pulled it through.

Case XIV. J. C., æt. 45, unmarried, has for fifteen years been very anæmic, and constantly seeking relief from eminent obstetricians in various places for menorrhagia and dysmenorrhœa. She has been several years under my care for the same maladies, and the advantage gained by medical treatment has been certainly very slight. About twenty months before the operation now to be described, and again two years before that time, I made attempts to relieve her by surgical interference. On the first occasion, I incised the tumour, having previously dilated the cervix to reach it as it lay in the anterior uterine wall. This was followed by diminished monthly losses, and consequent improved health for several months. On the second occasion, twenty months before the operation now to be described, I attempted and failed to do what I subsequently succeeded in effecting.

I was driven to the operation by the most urgent considerations. Her life was a burden, and she desired relief at whatever risk. Twice her life had been for many days despaired of, and she was living almost entirely in bed, when I resolved to try a third time.

The description of her anæmic condition at this time cannot be exaggerated. Her pulse was scarcely to be felt, and was quick. Her tongue and fauces were as pallid as her skin. She had no appetite, and subsisted on the smallest possible amount of liquid food. Anasarca of the whole body was present, and though lying in bed her lower limbs were as swollen by a pretty dense œdema as they are seen to be under any circumstances. She had considerable hydrothorax of both sides, and great fits of breathlessness, with orthopnœa, which looked like impending dissolution.

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[1] Although the operation in J. C.'s case lasted fully an hour, it was quietly conducted, and there was no expression of pain uttered by the patient. There was nothing to justify a description such as Lisfranc gives in the second volume of his Clinique Chirurgicale de l'Hôpital de la Pitié, (page 554). "J'ai coupé dans l'intérieur de l'utérus à petits coups et par morceaux des polypes volumineux à bases extraordinairement larges ; l'opération a été excessivement longue : pour détruire des tumeurs polypeuses, en grande partie ramollies et situées dans la même cavité, j'y ai porté mes doigts, des tenettes, des instruments tranchants ; j'ai tordu et arraché quelques portions de la tumeur ; j'en ai amputé d'autres : j'ai râclé la face interne de l'organe avec une curette : la manœuvre a été d'une durée effrayante ; . . . ."

Assisted by Dr Linton, I proceeded as I shall now describe, premising that, in consequence of the hardness and length of the perineum, every step of the operation was very difficult. In addition, she was so fat that the size and relations of the tumour could not be even guessed at with any exactness. I had previously dilated the undeveloped cervix and cavity of the uterus with tangle tents, but nothing like a tumour or projection, only a slight bulging elevation, could be felt in the anterior wall of the uterus, in which other means of examination showed the lower part of a tumour to be. Into the lower part of this elevation I fixed a volsella, and as it slipped, I seized and pulled by another, or by a hook. The violence used was great, as much as the tissues would bear. As I did not see that any other proceeding was available, we continued laboriously pulling with all the force we could apply. The volsellæ were moved to new and higher holds as a mass descended; and finally, a clean enucleated fibrous tumour came away. It measured 2 inches by 1¼. It was probably only a lobe of a mass larger than itself. I therefore renewed the operation. But the next mass began to tear, and we felt that the root, not thicker than a finger, and having its attachment high in the uterus, would soon be entirely severed. I divided it with scissors, getting away a fibrous mass measuring 1¼ by 1 inch. Very little blood was lost, and the operation was finished. The examination of the tumours discovered no indication of their having been even partially denuded before the operation.

The operation was painful, but she did not seem to suffer after it, and gradually improved in health. In a few weeks she began to get up, and to go about her house. Since this time—fifteen months ago—she has never been confined to bed for a day. The monthly periods are regular and moderate. She has almost always a slight watery discharge, tinged often with blood.

ARTICLE VII.—*Excision of One Lateral Half of the Tongue.* By GEORGE BUCHANAN, A.M., M.D., Surgeon, and Lecturer on Clinical Surgery, Glasgow Royal Infirmary.

THIS second successful case of removal of one entire lateral half of the tongue, by division of the lower jaw at the symphysis, has satisfied me that this is a safe and efficacious operation, affording the operator the opportunity of ascertaining, during its progress, the extent of the disease, and so directing his incisions as to remove all diseased structure. Before having recourse to the knife, I carefully considered how the wire of an ecraseur could be applied so as to bisect the tongue and divide its posterior attachment close to the tonsil, to which point the disease seemed to extend. Failing to satisfy myself that this could be done with sufficient certainty, I adopted the operation to be detailed. The history of the case is shortly as follows :—

Benjamin Brown, aged 51, by occupation a cloth-glazer, was recommended to me by Dr Cullen of Alexandria, in the month of July. At that time he had an ulcer on the right side of the tongue, the base of which was hard and painful. As there were some sharp irregular teeth opposite the sore, I recommended the extraction of these and some soothing treatment for a few weeks. The ulcer, he stated, was of four months' standing. The treatment recommended having failed to produce any effect, he was admitted to the Infirmary on the 5th September 1866. At that date there was a deep ulcer on the right side of the tongue, far back, with thickened, hard, everted edges. There was pain in the tongue, side of head and ear, and other signs of epithelial cancer of the tongue.

11*th Sept.*—To-day I removed the ulcerated part of tongue with the ecraseur, keeping the chain of the instrument seemingly free of the hardness by inserting needles through the tongue into the soft part. No vessel required ligature, the ecraseur dividing the tissues slowly, and the diseased part seemed well removed. However, the crushed appearance of the divided part did not afford an opportunity of judging accurately of the nature of the tissues through which the division was made.

On the 21st September he was dismissed, with the parts apparently cicatrizing.

Readmitted on 11th December. Shortly after leaving the hospital the disease re-appeared in the site of the wound, and rapidly advanced. There is now a large eroded ulcer at the back part of tongue on the right side. The whole base of the tongue seems invaded on that side by a hard tumour, which causes great pain on deglutition, the pain shooting back to the ear, and affecting the whole side of the head. The patient was willing to submit to a more extensive operation for its relief.

On the 15th of December, the operation was performed in the following way :—The patient having been put deeply under the influence of chloroform, the lower lip was divided in the middle line, the incision being carried below the chin to the hyoid bone. The lower jaw was now sawed through at the symphysis, and the two halves held apart, giving free access to the mouth. An incision was made in the mucous membrane inside the right half of the lower jaw, and the sublingual gland raised from the floor of the mouth, the attachment of the genio-hyo-glossus being divided to facilitate the raising of the tongue from the mylo-hyoid. In dissecting up the sublingual gland, a considerable artery was cut and tied. I believe it was a large branch of the facial perforating the floor of the mouth to reach the gland, as I have often seen in dissection. The tongue was now pulled out, and cleft in the mesial line back to its attachment to the epiglottis. The right half could now be pulled well forward so as to allow of my finger being slipped behind it to examine the exact extent of the disease. I found that, by dividing the anterior curtain of the fauces, keeping the knife close to the tonsil, I could remove all trace of diseased tissue. This I accordingly did with a probe-pointed knife, and so removed entirely the right half of the tongue. The lingual artery was seen spouting close to the hyoid bone, and was secured without delay. Although for nearly a minute there was considerable gurgling, from the blood getting behind the epiglottis, at no time was there any danger of suffocation, the glottis seeming to retain its power even under chloroform. When the blood was sponged away, it was seen that no other vessel required ligature, the incision having been made fairly in the middle line.

The halves of the divided jaw were approximated and kept in apposition by a strong silver wire passed through a hole drilled on each side of the section of the bone. The lip was united by silver sutures, an opening being preserved in the floor of the mouth by a bit of drainage tube. This I consider of great importance to preserve an exit for the discharge, which often has a foul odour after operations on the mouth. As an additional precaution against this foulness, I smeared all the cut surfaces with a strong solution of the chloride of zinc, which has a powerful effect in preventing decomposition in wounds.

The patient was ordered an injection of 60 drops of laudanum, and to take nothing into the mouth but small bits of ice to relieve thirst, if it became urgent.

16*th.*—Patient has passed a fair night. Pulse 100. Has swallowed very little. On attempting, he managed to swallow a few teaspoonfuls of sherry, but with considerable difficulty. Having satisfied myself that he could take some sustenance by the mouth if necessary, I ordered him to abstain from that as much as possible, and determined to nourish by the rectum for a few days, so as to give absolute rest to the mouth and jaw. He was therefore ordered to have an injection of four ounces of strong beef-tea, and two ounces of port-wine, thrice daily, the last injection at night to contain sixty drops of laudanum.

17*th.*—Is comfortable this morning, having slept well during the night. The right side of face and head is yellowish in colour, but that evidently depends on the bruising caused by the forcible separation of the jaws. The drainage-tube causing some uneasiness, is removed, the opening being patent.

18*th.* — Doing well. Some tension existing below chin, the drainage opening was enlarged by introducing a dressing-forceps and expanding the blades. He is ordered to wash his mouth frequently with a saturated solution of chlorate of potash, and as much as possible to spit out all the secretion from the wound.

19th.—Deglutition having become more easy, he is ordered to swallow small quantities of chicken-jelly and sherry frequently. The sutures were removed from lip and chin, and the incision found united throughout.

21st.—Continues favourable. The discharge from below chin very free. Fluids introduced into the mouth pass out by the opening, but this can be checked temporarily by pressing the finger against it. He shaved his beard this morning.

23d.—Patient continuing very well, and being able to swallow freely, the nutritious enemata were discontinued.

25th.—Keeping favourable: he is to sit up to-day.

11th January 1867.—This morning the patient walked into the theatre to show the result at the clinical lecture. The wound is cicatrized throughout. The jaw is completely united. The patient is quite free of pain, and can swallow perfectly well. Already he can speak so as to make himself understood.

As in the case I operated on a year ago, the tongue is turned round upon itself, the cut surface being underneath, so that the remaining half occupies the median line. The finger detects no hardness or symptom of remaining disease. I anticipate that this formidable operation will be followed by permanent relief, as in my former case, the present condition of which I noted in the December number of this Journal (1866).

ARTICLE VIII.—*Diphtheria.* By W. WATSON CAMPBELL, M.D., M.R.C.P., etc., Dunse.

I HAVE been led to communicate this paper by the fact that I have had much success in the treatment of diphtheria by a medicine which, if not altogether new in this disease, or, in the experience of others, so successful, may have been rather undervalued, because not so determinedly and persistently used as in the cases which have recovered under it in my practice since May last. I would ask the reader to exercise a little forbearance and patience while I note a few cases in a general way, and their results under former treatment, before proceeding to the others which have come under my care since I began with the permanganate of potash. The difference in the general result of the treatment seems to me so marked that I may, perhaps, be excused for bringing forward a few cases of an ordinary kind first, in order to impress the value of what I have found a remedy in this disease.

On the 17th of August 1862, I was asked to visit a little girl about six years of age. This was my first case of diphtheria. She had been complaining much of sore-throat and difficulty of swallowing for some days. On examining her throat, a thick pellicle of whitish colour was seen, rather loosely attached to the anterior surface of the uvula. The rest of the throat seemed healthy. Her pulse was high and skin hot. The nitrate of silver was freely applied to the part, and the tincture of the muriate of iron and port wine prescribed,—to be taken every four hours. A gargle of the chlorate of potash was also recommended. These, and strong beef-tea and milk, which were ordered, were persistently refused from the first. The area of the pellicle was no greater for a day or two, but I observed that as soon as one was shed another took its place, notwithstanding the use of the nitrate of silver. By the 21st, the disease had extended along the rim of the velum palati on each side, as far as the anterior part of each tonsil. The patient, from the first, became weaker, and died quietly on the 23d.

On the 4th of February 1863, I was requested to visit a family about eleven miles distant. I found two of the children suffering from diphtheria. One member of the family had died of this disease sometime before. The same treatment as in the first case was prescribed. I saw them again on the 7th, and found them improving. They made good recoveries, though both experienced, for some time during convalescence, a feebleness of the legs.

I saw no other case until the night of the 28th August 1864, when I was asked to visit a child, about seven miles off, who was said to be very ill with sore-throat. I found two of the children suffering from diphtheria. The pellicle, in both cases, was firmly attached and yellowish in colour. What were believed to be orthodox remedies were prescribed, but without avail. One of the children died of diphtheritic croup next day. By this time some of the other members of this family were seized, and I was informed that some of their neighbour's children were ailing, and was asked to see them. I found two of them ill. The pellicle was on the back part of the pharynx in one case, and in the other it was on the tonsils and down the sides of the throat. In the former case the exudation extended further than could be seen, and there were, in addition to the ordinary symptoms of diphtheria, those of croup. The nitrate of silver was freely applied. The tincture of the muriate of iron and a gargle of the chlorate of potash were prescribed. Port wine was ordered to be administered every four hours, and strong beef-tea and milk were to be taken as food. I gave the croupy patient an emetic, and left a stimulant expectorant which was to be taken occasionally. I saw them all on the following day. The survivor of the two first affected in the *first* family was worse, and so was the croupy patient in the *second* family. The former died on the 31st, and the latter on the 1st September. A third sufferer in the first family died on the 2d: two in this family recovered. The second patient in the second family recovered. Five children in a third family had diphtheria after the last death (which occurred on the 2d), and only one died on the 15th. Thus, in this place, of five who suffered in the first family only two recovered, of two who suffered in a second family one recovered, and of five in a third family only one died. There were altogether twelve cases of diphtheria,—of the twelve, five died and seven recovered. It was a disgraceful row of houses in which the farm-servants lived,—one of the worst I ever visited. In wet weather, the rain was, in some parts of almost every house, caught in tubs placed on the nasty earthen floors; and during night, in some of these houses, the people could lie in bed and gaze at the stars. Perpetual mud puddles were in front of the houses, which small expense on part of the proprietor could have removed by draining: this by the way. From the end of September for fourteen months no other case of this disease came under my care.

On the 6th November 1865, I was asked to visit a little girl, whose parents resided in the same street with me. She was about five years of age, and proved one of the most tractable patients I ever had. On my first visit she was suffering from symptoms of croup. Her appearance was anxious and feverish, and her pulse was very high. On examining the throat, an extensive and rather patchy pellicle of yellowish colour was seen. The other children were kept away from the patient, and a strong solution of the permanganate of potash was used throughout the house as a disinfectant. Stimulants and expectorants were freely administered and taken, and the throat, as far as possible, brushed with the nitrate of silver repeatedly throughout her illness; but the croup gradually became worse, until tracheotomy was thought of as a last resort. Not wishing to take all the responsibility of the operation, I asked my friend Dr M'Watt to see her with me on the evening of the 9th. He was engaged until rather a late hour that night, and after seeing the patient, we agreed to operate in daylight,—unless the symptoms should become so much more urgent as to render operation necessary during night. Next day—the 10th—the parents objected, and about noon she died from suffocation.

This child's father was in the habit of working with another man who resided about two miles out of town. He continued at his work for the first two or three days of his daughter's illness; and on the 17th, I was asked to visit this other man's wife, who was said to be suffering from sore-throat. On examining her throat, I saw a very small light yellow spot on the middle of the left tonsil. By the 19th, this was very much larger, and it continued to increase until the whole of that side of the pharynx and the greater part of the other tonsil were covered. The difficulty of swallowing was considerable from the first, and the articulation was indistinct, and became more so as the disease progressed. On the 19th, when seen for the second time, the mucous membrane round the exudation was red and irritable, her pulse was high, and prostration was much greater than seemed consistent with the local disease. Local applications appeared rather to irritate than soothe the part or arrest the extension of the disease. Although her prostration was well marked at an early stage of the disease, still, by means of wine and strong beef-tea and milk, her strength was wonderfully well sustained, and, about the beginning of December, an improvement was apparent. This was the most protracted case I had seen, and when an improvement was perceptible, its progress was very slow and interrupted. From the beginning of December the exudation began to lessen its area, and it continued to diminish slowly until it had disappeared completely about the 14th. Food, which, in any form, had, previous to the crisis, been almost forced upon the patient, was asked for about the time the exudation began to fade; and, though a small quantity satisfied the patient's desire, it was retained, until I had some hope that she was to get over the disease without sequelæ. About the middle of the month however, fluids began to be discharged by the nostrils, and when it reached the stomach it was not unfrequently vomited. This vomiting became exceedingly troublesome from its frequency and persistence. Bismuth was administered three times a-day, and food was given in small quantities and given often. The vomiting continued, but was not so frequent. It did not altogether cease before the beginning of January 1866. Before this time the pulse, which was about 78, became irregular,—three or four beats per minute being wanting. The heart sounds were normal, though irregular, but she never experienced any uneasiness in the cardiac region. The urine had a mere trace of albumen—(this condition of the urine, I may here observe, was found in one or two cases). After a little time the legs began to swell and became feeble. This feebleness increased until she lost all power of her legs. Her arms were likewise affected with paralysis to such an extent that she required to be fed. Iron and quinine were given, but these brought back the vomiting. Bass's ale took the place of these medicines as a tonic, and was retained,—I believe the patient was much benefited by it. The tincture of nux vomica and galvanism were used for some time in order to restore the nervous system, and seemed to do good, but not much. At all events, the paralysis did not leave quickly, as it was April before she could perform her household duties with any freedom or ease. Before the end of that month, every sign of the disease had left her. She is now in excellent health. About my next six cases I need say nothing further than that three of the patients died; two of the others recovered rapidly, and the third had paralysis of the legs for some weeks. Five of the six cases occurred in the same building, but in two different families.

It was towards the end of my attendance on these patients that I caught the disease myself. This was about the beginning of May last. One evening about this time I had slight throatache and supposed it to be due to cold; but, on examining my throat by the aid of a mirror, I observed a small white spot on one tonsil. Next morning the pulse was quicker than usual, and on the other tonsil there was another spot. I at once began with the iron and wine, and tried the effect of a gargle prepared with the permanganate of potash. This gargle, though weak and not very often used during the day, seemed to have a good effect, as the spots were rather smaller before bedtime. Next morning they were as large as ever, but under the influence of the gargle, more freely used, they all but disappeared before night. This state of matters continued for about a week,—the exudation disappearing when the gargle was used, and reappearing when it was stopped. At last I used it considerably stronger (gr.x. to ℥xx.) and more frequently for a short time, and I was soon all right.

Not long after this—on the 18th of May—I was asked to visit a young lady, residing at a distance of about eight miles, who was said to suffer from sore-throat. Here I may remark that her brother resided in the same street where the five cases last referred to occurred, and that he occasionally asked after the children, while they were ailing, as he passed along. He visited his home while the children were ill, and about ten days after he had been there, I was requested to attend his sister. I found her suffering from diphtheria. The exudation was very adherent, of a dirty yellow colour, and extensive,—covering nearly both tonsils, the rim of the velum, and the anterior surface of the uvula. She was very feverish and prostrate. She had a shivering three or four days before she was seen, and this was followed by sore-throat, which gradually got worse till I saw her. Deglutition was very painful, and articulation difficult and indistinct. In the treatment of this case I immediately began with a gargle of the permanganate of potash (gr.x. to the ℥ xx.), recommending it to be used very frequently. To insure its application to the whole affected surface, she was told to swallow a little of it now and then, in order that, should there be any trace of the disease further down than could be seen, it might be flushed (if I may be allowed the expression) with it as often as a little of it was swallowed. Iron and port wine were also used. This patient never went back a day from this time, if we except the retardation of cure by sequelæ. Next day the exudation was very much less, and in four or five days it had entirely disappeared; and, though the throat was raw and tender-looking, I was much pleased to find matters going on as they were. From the first application of the gargle her convalescence may be safely dated. From that time the pain began to subside, and deglutition and articulation became more easy. She went to the sea side about three weeks after my first visit, and though she had great weakness in the legs, and suffered from almost complete blindness from amaurosis for some time, she is now quite well.

I have no wish to extend this paper further than the subject requires, but must state that, since this last case came under my care, I have had other ten cases to attend, and that under the use of the permanganate-of-potash gargle, the tincture of the muriate of iron, and port wine, every one of these cases recovered rapidly. I would almost make one exception, and in this case—that of a lad about 18 years of age—there was only a slight weakness in the legs experienced for about fourteen days. Some of these cases I would certainly have despaired of without the aid of the permanganate of potash, so very severe did the attacks seem to be when I first saw them. At the present time I have a very bad case in hand. I was called upon to see him on the 30th October. He had a shivering on the night of the 25th. On making my first call, his pulse was rapid and wiry, his face pale and anxious-looking, his skin clammy and moist, deglutition very painful, and articulation very indistinct. The same treatment was adopted in this as in that of the young lady noticed above. I saw him again on the 2d of November. He was clear and bright-looking, the pain was greatly relieved, and articulation was very much improved. He felt and continues to feel better ever since he used the gargle. I saw him on the 4th again. The exudation, which was of a dirty yellow colour and very adherent, was nearly gone, and the throat was rather raw-looking. Whether the improvement will continue or not remains to be seen, but I have great hope that he will do well.[1]

Perhaps I may be excused for referring to two or three points in my experience of this disease, which are somewhat interesting.

The communication of infection is not necessarily direct. Indeed, in none of the cases I have seen did the disease seem to pass directly from one to another, unless where it spread in the family, as in the first cases I have noticed. On five well-marked occasions, it appeared to have been carried by a third party to a distance varying from one to eight miles. On one occasion it reappeared in the family of the woman attended first in September 1865, after an interval of six or seven months. Only during my attendance on the last two or three cases have I known scarlet-fever to be present in the district.

In some cases of scarlet-fever, I have seen whitish pellicles on the tonsils—not very unlike what I saw in my first case of diphtheria; but, otherwise, there was in every case quite enough—even putting the rash out of the question—to distinguish the scarlatinal from the diphtheritic exudation.

In no case of diphtheria did I ever observe an abundant muco-purulent discharge from the nostrils; and though the tonsils are generally felt outside to be hard and large, yet I have never seen in this disease a single case of cervical cellulitis.

In all the cases of scarlatina where I have seen an exudation resembling that of diphtheria, there was not long afterwards a well-marked and frequently profuse discharge from the nostrils, and, in some of the cases, very extensive cervical cellulitis.

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[1] He made a good recovery, and is now quite well (January 1867).

In some of the cases of diphtheria the exudation appeared to select, as its primary seat, the mucous membrane in the upper part of the larynx; and in some of these, by extension of the exudation, a modification of croup was caused.

In no case of scarlatina, even with intense throat affection, have I ever seen croupy symptoms arise to give evidence of the larynx being affected.

I am not aware that palsy is ever met with after scarlet fever, however severely the throat may have been affected, while palsy very frequently follows diphtheria.

Only in one case out of thirty-five that I have had under my care have I seen dropsy—œdema of the legs only—follow. Dropsy is comparatively frequent after scarlatina.

In such cases of diphtheria as I have tested the urine, the chlorides have never been found absent, and only in two or three cases have I detected albumen. In scarlet fever we expect a deficiency or absence of the chlorides, and are not surprised at the presence of albumen.

Notwithstanding all this, there may have been found, in the experience of others, more conditions common to both diseases than I have met with. I was certainly surprised at the reappearance of the disease in the same family after an interval of six months—a circumstance which has been known to occur in scarlatina.

With regard to the occasional resemblance of diphtheria to croup, I am disposed to think it accidental; and that the pellicle of the former differs from the false membrane of the latter in extent, in the rule of place, and in the latter being the result of true inflammation.

From my experience of this disease, I venture to differ from authorities such as Dr Begbie, sen., and Dr Jenner, with regard to the disease being constitutional. My opinion is that it is not so primarily, but that it becomes so, not so much, perhaps, by the absorption of poisonous matter from the seat of the exudation, as by the effect that the presence of such exudation may have on the nerve centres, through the nerves distributed to the part on which the exudation has taken place. It may be objected that some cases have ended fatally without much local disturbance, and this by rapid prostration. Still the time required to effect this prostration, and the amount of exudation which will cause death in this way, may, as with other poisons, differ much in every case, whether acting indirectly or directly. The best reason, however, for supposing that the disease is local at first, is afforded by the success which has followed the local treatment, and also, by what was very apparent to me, that, on the whole, the more rapidly the local disease was removed, the less likely was the constitution to suffer.

Of twenty-three cases of diphtheria which occurred in my practice before I used the permanganate-of-potash gargle, ten died. Of the thirteen who recovered, four had paralysis to a greater or less extent. On the other hand, of the twelve cases which have occurred since (not including that under treatment) *none* died, and only *two* have had paralysis.

Since I thought of making this communication I have seen a letter from Dr N. Evans, in the "Medical Times and Gazette," of 27th Oct., in which the report of an interesting case is given, which corroborates very strongly my impression that the permanganate of potash may be safely allowed to exercise a remarkably beneficial effect when used perseveringly, and that even in the worst cases a cure may be hoped for, provided the larynx has not been affected.

Part Second.

REVIEWS.

A Treatise on the Principles and Practice of Ophthalmic Medicine and Surgery. By T. WHARTON JONES, F.R.S., Professor of Ophthalmic Medicine and Surgery in University College, London; Ophthalmic Surgeon to the Hospital, etc. Third Edition. Pp. 806. London: John Churchill and Sons: 1865.

THIS is now one of the most voluminous of Churchill's series of Manuals. But from its very growth in bulk it has become a rather unwieldy text-book, and with an increased amount of information, there is, perhaps, an increased difficulty in getting what is desired. There has of late, no doubt, been a great advance in our knowledge of the nature and treatment of many diseases of the eye, more particularly diseases of accommodation and refraction, and operations for cataract, which require to be introduced into any work pretending to be a treatise or text-book on the subject; but still we are of opinion that a good text-book for the student, and even book of reference for the medical practitioner, might readily be written so condensed as to require little more than one-half the space occupied in Mr Jones's treatise. Indeed, one and perhaps the chief reason why this manual extends to such a size is, that the arrangement and classification of diseases is such as to render a large amount of repetition unavoidable, which, especially to a student, is annoying and confusing. Mr Jones classifies diseases chiefly according to their pathological nature, instead of according to their locality or seat; and hence inflammatory affections are separated from the causes to which they may be owing, such as wounds, and from the effects they produce, such as hypertrophy, atrophy, etc. By far the simplest method of studying or describing diseases of the eye, is to take each structure in turn, and consider the various diseases to which it is subject, and this is usually the basis of classification in this country. Another considerable source of repetition, is the unnecessary extent to which diseases have been subdivided according to the circumstances under which they have occurred, or the constitution of the patient. For example, we are favoured with a description of morbillous, scarlatinous, and variolous ophthalmia—as if these were distinct inflammations; while, as every one acquainted with the subject knows, and as Mr Wharton Jones himself declares, the practitioner must "be prepared to find *that* inflammation more or less different in kind in different cases of the same disease, and requiring, therefore, different modes of treatment—modes of treatment, however, not differing from those indicated for the particular species or form of inflammation, except in so far as may be necessitated by the presence of the general disease." In fact, such diseases render the patient more susceptible to different inflammatory affections of the eye; but these affections do not in the slightest differ in symptoms, progress, or treatment, from others which have no such origin. What good, therefore, results from such a sub-division? All that we require to know is, that such and such affections frequently occur after measles, scarlatina, or small-pox. A considerable amount of space is also occupied by a detailed description of diseases which are of excessively rare occurrence, and of operations and methods of treatment which are in disuse.

Ophthalmology is proverbially rich in technical terms. There are few diseases which do not possess three or four different names, and all prominent symptoms are equally well cared for. Nevertheless Mr Jones would increase our already abundant supply, by furnishing us with posterior-internal ophthalmia; 1*st*, with serous effusion (this, from the symptoms detailed, would, by most modern authors, be termed glaucoma); 2*d*, with plastic effusion (choroiditis); 3*d*, affecting chiefly the optic nerve (neuritis optica); 4*th*, "morbid alterations of structure-effects of posterior internal ophthalmia" (under this head are included almost all the diseased conditions with which the ophthalmoscope has made us familiar); 5*th*, scrofulous (sclerotico-choroiditis with staphyloma); 6*th*, syphilitic (syphilitic retinitis); and, 7*th*, arthritic (glaucoma). We do not consider this attempt at classification any improvement.

To proceed, however, from these general observations to the particular contents of the work, the first chapter on ophthalmoscopy gives a very good account of the different methods of inspecting the eye, including the exploration of the anterior segment of the organ by oblique illumination and the catoptric test, the examination of the field of vision, the use of the opthalmoscope, etc. (The few pages on morbid ophthalmoscopic appearances should, to avoid repetition, have been reserved for the chapter on the diseases described.)

The various forms of local remedies, and their method of application next come under consideration, followed by some hints regarding the abstraction of blood, the evulsion of eye-lashes, and the removal of foreign bodies from the oculo-palpebral fold of conjunctiva. Under the head of abstraction of blood, reference might, with advantage, have been made to the use of the artificial leech for this purpose,—a modified form of cupping apparatus which is extensively used by continental oculists, and answers its purpose admirably. We may also mention we were not a little startled at a statement that occurs here, to the effect that "half-a-dozen or a dozen leeches is the *average* number applied around one eye in a grown-up person." As far as our own observation and experience go, we would consider one-half that number a large average at the present time.

These preliminary subjects occupy ninety-six pages, and the rest of the book is devoted to a description of diseases of the eye proper, and their treatment; taking in succession, inflammatory affections, partial and general enlargements, dropsical effusions, extravasations of blood, hydatids, cataract, operations for artificial pupil, etc. Here we would again remark that it would prevent a great deal of confusion and useless repetition, were the general description given of inflammation of each tissue of the eyeball either very much shortened, and made immediately to precede the individual inflammatory affections of that tissue, or altogether dispensed with. Thus, deep-seated inflammations of the eye are described in three different parts of the book. If we look up Retinitis in the index, we are referred to pages 56, 126, and 231, and all through the book the reader is constantly reminded that further information on that subject may be obtained in some other chapter. We cannot follow the author into a consideration of each topic, and will content ourselves with making a few remarks on certain points on which a diversity of opinion may exist, and pointing out some important omissions.

While treating of pustular inflammation of the cornea, the author enters upon a discussion regarding the nature of inflammation, and, from experiments and observations, concludes that it consists in, or depends upon, a constriction of the small arteries, which allows of the blood-corpuscles to accumulate in the capillaries and venous radicles. It is to be observed the author views congestion as the distinctive symptom of inflammation altogether apart from exudation, a view which few pathologists of the present day consider correct. It were out of place to enter upon a discussion of this subject in reviewing a work on special diseases, but we may be permitted to allude to the action of atropine, which he cites in favour of his theory, and which, we are of opinion, rather serves to controvert it. The author has observed that the application of a solution of the sulphate of atropine to the frog's web occasions constriction of the arteries, and gives rise to a congested condition of the capillaries and veins, and he explains on this ground the aggravation of symptoms which results when this substance is applied to a glaucomatous eye in which there is great venous congestion. But it must be remembered that the external venous congestion in glaucoma arises from the mechanical pressure (from increased secretion of vitreous humour) to which the deeper vessels are subjected preventing the return of blood by its natural channels, and thus forcing it to flow by the superficial vessels. In such a condition as this, it is not to be expected that atropine would be capable of exerting any beneficial effect by constriction of the vessels; but, on the contrary, that it would, by its paralytic effect on the power of accommodation, render vision more confused. If, however, we watch the effect of atropine in cases of acute inflammation of the cornea or iris we cannot but be struck by the marked effect it has in reducing the inflammatory symptoms; which, were Mr Jones's theory correct, could only be explained on homœopathic principles.

It was with some difficulty that we persuaded ourselves that Mr Wharton Jones viewed granular lids as a species of purulent ophthalmia,—a doctrine we consider essentially erroneous. We do not for a moment deny that granular conjunctiva may follow purulent ophthalmia; but, on the other hand, we have abundant instances of this disease in persons who have never had purulent ophthalmia, and in whom the granular condition has been slowly developed without exhibiting any of the symptoms of the purulent affection. How rare, too, is it to find a granular state of the conjunctiva, where an eye has been even seriously injured by the severity of an attack of purulent inflammation? We would also ask how it is to be explained that the super-induction of purulent ophthalmia sometimes cures aggravated cases of granular lids; unless, indeed, a doctrine be adopted similar to that upon which the practice of syphilization rests. From the erroneous views thus taken of the nature of granular lids, the chapter on Egyptian ophthalmia gives a description of an inflammation which may attain to three different degrees of severity, and which, while exhibiting characters belonging to both purulent ophthalmia and granular lids, can certainly not be taken as accurately typical of either.

In the treatment of iritis, the author places the greatest confidence in bleeding and mercurialization. Indeed, so far does he rely upon the latter means of treatment, that, even in scrofulous iritis (one of his sub-divisions), he recommends its careful administration till the gums are affected. Now, while we are not of the number of those who would deprecate the use of mercury in all cases of iritis, but, on the contrary, are of opinion that in certain sthenic cases it may be beneficially employed, we should certainly be very much averse to its administration in iritis occurring in scrofulous subjects. We consider Mr Jones ought to have alluded to the fact, that very opposite views are held upon this point, and to have referred to the large number of cases that have been published, in which neither mercury nor bleeding had been employed, and yet in which most favourable results were obtained. Mr Zachariah Laurence, for example, published a very interesting series of successful cases in which opium was almost the only internal remedy used. What of itself might serve somewhat to shake one's belief in the universally beneficial action of mercury, is the fact, that iritis sometimes occurs in a patient already under the influence of that drug. Indeed, from the wonderful amelioration we have observed in incipient cases from the local application of atropine and belladonna, we are inclined to place far more reliance upon these local means than upon any general system of treatment. The employment of iridectomy in cases of recurrent iritis is only cursorily referred to, and its use in preventing a relapse considered problematical,—a case being narrated in which "iridectomy" did not secure the eyes against a first attack of iritis. But in the operation was the portion of iris removed from its *ciliary attachment?* for that is a necessary point in operating for recurrent iritis, while it is not essential in performing excision of a portion of iris for artificial pupil, for which object the operation was performed in this case. The operation of iridectomy is certainly not infallible in preventing a recurrence of the inflammation, but undoubtedly succeeds in a considerable number of cases, and deserves more consideration than is accorded to it in the passage we refer to. We were amused by the restrictions placed on the use of iodide of potassium in the following passage: "Iodide of potassium may be also tried under such circumstances (when mercury does not exert its usual curative effects), provided the pupil is not obstructed with lymph, for in this case the effect of the iodide will be to consolidate and fix the lymph so as to arrest the absorption of it." This he explains at another place to be due to the removal of the mercury from the system by the iodide!

One of the greatest improvements that have been effected in the operative department of ophthalmology during the last few years, is undoubtedly the modification of the operation of abscission of the front of the eyeball, introduced by Sir W. Wilde of Dublin and Mr Critchett of London, which consists in the introduction of needles armed with ligatures round the base of the part to be abscised prior to its removal; so that, immediately thereafter, the edges of the wound may be brought into apposition, the vitreous humour prevented from escaping, and a good stump obtained,—the result contrasting strongly with the old method of operating, in which the vitreous humour was allowed to escape, and the wound to heal slowly by granulation. We regretted to find no reference whatever made to this improved method of operating.

Under cataract, we find a long diagnostical table given, displaying "the principal points of difference between cataract on the one hand, and glaucoma with amaurosis on the other." As these diseases possess very few symptoms in common, such a table is unnecessary; it is perhaps serviceable, however, in giving in a concise form the leading characters of these affections. The removal of hard cataracts through a linear incision in the cornea, (with the assistance of a scoop or modified curette,) in combination with iridectomy as first recommended by Schuft and von Graefe, and subsequently adopted and extensively practised (with somewhat altered instruments) by Bowman, Critchett, and others in London, is evidently not approved of by Mr Jones, as the whole subject is disposed of in eight and a-half lines! When we reflect that published reports of cases show that the results obtained from this operation may stand a favourable comparison with the ordinary method of flap extraction, a little more consideration might surely be given to it. This does not arise from want of space, as we find more than two pages allotted to the operation of reclination by corneal puncturation,—an operation which, to quote the author's own words, "has never come into use"! If we desire information on the removal of cataract by suction—a method of operating recently revived—where shall we find it? We have vainly searched for the least reference to it. Professor Sperino of Turin published, a few years ago, a considerable work on paracentesis of the cornea, which operation (repeatedly performed) he declared to possess, among numerous other remedial properties, the power of staying the progress, and even of curing, certain cases of cataract (especially the senile variety in its incipient stage). He supported his statements by adducing a large number of cases, in some of which great benefit was apparently experienced. Even if cataract could be thus cured, it is very doubtful whether a prolonged course of operative treatment such as this would be often had recourse to, but still it would be right to mention the possibility of curing cataract by such means. We cannot speak from personal experience on the point, but from the position held by Professor Sperino, his statements are worthy of notice. What, perhaps, has been most effectual in increasing the percentage of cures in cataract operations, is the improvement in the after-treatment; more especially, the employment of the elastic-pressure bandage of von Graefe, by which the lips of the wound are kept in accurate apposition, the motions of the eyes restrained, and the evil effects arising from coughing or straining of any kind, very much diminished. Upon this point the Manual is sadly deficient. In the paragraph on cataract glasses we find the statement, that the crystalline body contributes comparatively little to the convergence of the rays of light passing to a focus on the retina. If so, why is it that such strong lenses as from three to four inches focus are necessary for distinct vision after the lens is removed? How, too, can one explain the great alteration in the refractive power of the eye which an increased curvature of the lens occasions in accommodating the eye to see near objects distinctly (equal to an artificial lens of five inches focus)?

That the power of adjusting the eye for distinct vision of near and distant objects depends upon an alteration in the curvature of the lens, Helmholtz's experiments have placed beyond a doubt;

but the exact manner in which this alteration in the curvature of the lens is effected is still a subject of controversy. The view most generally entertained at the present time is, that when the eye is at rest, or the power of accommodation paralyzed, the eye is adjusted for distant objects, and that, when near objects require to be seen, the curvature of the lens is increased by contraction of the ciliary (or tensor choroideæ) muscle; vision being at the same time rendered more distinct by a coincident contraction of the pupil. Mr Jones, however, maintains that the eye at rest is adjusted for the perception of objects at an intermediate distance, and that for near or very distant vision the accommodative power must be called into action; the accommodation for near objects being effected by the circular, that for distant objects by the radiating fibres of the ciliary muscle,—a view held by some continental authorities. This view chiefly rests on the fact that the lens, during adjustment of the eye for distant vision, has been found by Helmholtz to be thinner than it is in the dead eye. But when we consider that the lens, when relieved from the restraint of its capsule, has a tendency to assume the globular form, and that after death there is great relaxation of the tissues, favouring this tendency—this fact loses its significance. Besides, if the eye when at rest were adjusted to some intermediate point, one would expect in cases of paralysis of accommodation (such as occurs after diphtheria, long-continued fever, and other debilitating diseases), to find the patient's sight most distinct at a moderate distance from the eye, which is not the case. Mr Jones considers the radiating fibres of the ciliary muscle to be under the influence of the sympathetic; consequently, in cases where the sympathetic is paralyzed from pressure of tumours in the neck or other causes, we should find the perception of distant objects defective, but improved by the use of concave glasses, which, so far as our observation goes, is not the case. It is well known that the range or extent of accommodation gradually diminishes as age advances, obliging most old people to use spectacles. This arises chiefly from an increased firmness or rigidity of the lens. Now, if accommodation were of a twofold nature, we would expect our perception of distant as well as near objects to become confused as age advances, and that to see remote objects clearly, concave glasses should be worn. Daily experience tells us that this does not occur.

The effect of atropine upon the accommodation which is generally ascribed to paralysis, Mr Jones views as the effect of stimulation through the sympathetic of the radiating fibres of the ciliary muscle, and, in like manner, its action on the pupil as due to stimulation of the radiating, rather than to paralysis of the circular, fibres of the iris. We have not space to discuss this subject here; his views, although apparently supported by some facts which he adduces, are not those held by most oculists or physiologists. We believe the dilatation of the pupil by atropine to be due partly to stimulation of the radiating, but chiefly to paralysis of the circular fibres of the iris. Our author makes no allusion to the local application of the Calabar bean to the eye in cases of paralysis of accommodation, in which, besides affording great temporary improvement of vision, it has also been found of the greatest use in expediting recovery.

The employment of iridectomy in the treatment of glaucoma does not meet with much favour. Now, no one has ever put this operation forward as an "unconditional cure for glaucoma." No doubt, when it was first introduced, it was employed in all cases, however far advanced the disease might be, and of course it failed in a very large number, as they were unsuited for the operation, the optic nerve and retina having become completely paralyzed and altered in structure from the great and long-continued pressure to which they had been subjected. Thus considerable discredit was thrown upon its use; but now that suitable cases alone are submitted to it, its remedial powers are almost universally acknowledged, and if proof of this were wanted, we need only allude to the fact that most of those who opposed the operation when first suggested, are now its strenuous advocates. We must always bear in mind that the disease for which it is employed is one which, if left to itself, must almost infallibly lead eventually to complete loss of sight, so that, even if the results were less beneficial than they really are, it would still be advisable to give the patient the chance of recovering or retaining his sight. In the case cited by Mr Jones as one in which extraction of the lens and iridectomy had been performed, and in which glaucoma was nevertheless subsequently developed, the portion of iris excised was evidently merely a part of the pupillary margin which had fallen before the cataract knife in making the section, and this, as already explained, does not constitute the operation of iridectomy; and we often observe in such cases, when a portion of iris remains afterwards adherent to the corneal cicatrix, that the iris becomes more susceptible of irritation, and this may eventually lead to a glaucomatous inflammation. This also may serve to some extent to account for the want of success which in some cases has attended the performance of so-called iridectomy.

Great temporary benefit has often been derived in cases of glaucoma, from tapping either the aqueous or vitreous humours, and to this we ascribe the good results that have frequently attended Mr Hancock's "division of the ciliary muscle," and Mr Solomon's "intra-ocular myotomy;" operations which, as being extensively performed at some of the ophthalmic hospitals in this country, deserve a passing notice, but are not mentioned by Mr Jones.

We cannot pass over in silence a mistake of a somewhat important kind which has crept into Mr Jones' description of the operation for convergent strabismus,—viz., that the tendon of the internal rectus should be cut at some distance from, instead of close to, its insertion. The operation thus performed would certainly be more potent, but would materially restrict the mobility of the eye. Of the many ingenious operations which of late years have been suggested in the treatment of various forms of strabismus, Mr Jones says little; and in particular the beautiful plan recently brought into use of curing *divergent*, the result of excessive operations for *convergent*, strabismus, by bringing forward the insertion of the internal rectus, thus at once rectifying the strabismus, and increasing the mobility of the eye, has been entirely overlooked.

The author also omits to mention the able researches, physiological and clinical, by which von Graefe has demonstrated the nature of many cases of blepharo-spasmus, and proposed what has proved in his hands a very effective treatment,—cases, namely, which depend upon reflex irritation of some of the branches, more especially of the fifth pair of nerves; division of which served to remove the affection.

Aware of the great benefits Mr Jones has conferred on our profession by his many valuable scientific researches, which have deservedly secured for him a position of high eminence, it is with great reluctance that we have felt ourselves compelled to point out that this manual is in many particulars behind the day, and that in some of its teachings it is altogether erroneous; but there are certain points which we can heartily commend to the attention of the profession, among which we may specify the descriptions of some of the subjective phenomena, the observations on paralytic affections of the muscles of the eye, and diseases of the eylids, and, particularly, his admirable plan of operating for ectropion. Mr Jones certainly deserves well of the profession for the introduction of this operation, and we hope, that erelong, in suitable cases, it will be extensively employed. Should he favour the public with another edition of this work, we think that by a simplified classification, by judicious omissions, and by the introduction of some information which we seek for at present in vain, he might render it, in all respects, worthy of the confidence of the profession.

Chemistry for Students. By Professor WILLIAMSON. Oxford: Clarendon Press: 1866.

Lecture-Notes for Chemical Students. By Professor FRANKLAND. London: Van Voorst: 1866.

Lessons in Elementary Chemistry. By Professor ROSCOE. Macmillan: London and Cambridge: 1866.

Inorganic Chemistry. By the late GEORGE WILSON, M.D. Revised and enlarged by STEVENSON MACADAM, Ph.D. London and Edinburgh: W. & R. Chambers: 1866.

Practical Chemistry. By STEVENSON MACADAM, Ph.D. London and Edinburgh: W. & R. Chambers: 1867.

Elements of Qualitative Chemical Analysis. By W. H. SPENCER, B.A. Macmillan: London and Cambridge: 1866.

Practical Chemistry. By W. ODLING, M.B. London: Longmans: 1865.

Elements of Chemistry. 3d Edit. Part III. By W. A. MILLER, M.D. etc. London: Longmans: 1867.

TEACHERS of Chemistry have long been in want of a short elementary text-book. The valuable octavos of Turner and Miller[1] were too large and expensive for the majority of our students, requiring more money to buy, and more time to study them, than they could well afford. The excellent manual of Fownes was smaller and cheaper, rather by condensation of style than by selection of matter, while the school chemistries belonged, as a rule, to the age before the great chemical revolution, and were nearly useless to the student of the present day.

The first who attempted to supply this demand for a short text-book, was Professor Williamson. His "Chemistry for Students" is a very remarkable little book. One of our greatest theoretical chemists has produced a text-book containing absolutely no theory —nothing but a clear statement of carefully selected facts. This peculiarity will, no doubt, recommend the book to a large number of teachers, who reasonably prefer their own theoretical views, and would gladly avoid the trouble of controverting those which their students find in their text-books. Unfortunately, Professor Williamson's book is disfigured by a number of errors of the press unusual even in a scientific treatise. The list of errata appended to it is large, but might easily be doubled by any one without much trouble.

The next book in our list is the direct antithesis, or rather complement, of Professor Williamson's. The latter contains nothing but fact, while Professor Frankland's "Lecture-Notes" are almost exclusively theoretical. The object of the work renders this necessary, as it is written expressly for the purpose of saving the time which a student generally consumes in copying into his note-book the formulæ and equations on the black board. Every teacher has, no doubt, often regretted, with Professor Frankland, "the great amount of labour which an earnest student expends in noting down the reactions and the names and formulæ of substances which are presented to his notice in the lecture-theatre," and will agree with him that "he is thus greatly interrupted in following the arguments and explanations of the speaker." The book is exactly what it professes to be, and is invaluable to any one who wishes to obtain an elementary, and, at the same time, an accurate view of modern chemical theory. The author uses, to express the constitution of substances, two new modes of notation,—his own symbolic formulæ, and the graphic formulæ of Dr Crum Brown. By means of these, the most complicated chemical compounds and reactions are made intelligible even to a beginner. The book has at first sight a somewhat repulsive appearance, from the mass of formulæ which bristle in every page; but it must be recollected that it is not intended to be an independent treatise, but a supplement to a course of lectures.

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[1] A third edition of Miller's "Organic Chemistry" has just been published, which fully maintains the high reputation which Dr Miller's "Elements" deservedly enjoy as advanced text-books.

Roscoe's "Lessons in Chemistry" is an admirable school-book. Concise without obscurity, elementary without meagreness, it is a book we should strongly recommend to those who wish to know what chemistry is without becoming chemists. Though intended for schools, it contains almost all that a medical student is expected to know of the science; and we have no doubt it will become a very popular text-book.

Dr Macadam has given us a new edition of the "Manual of Inorganic Chemistry" of the late Dr George Wilson. He appears to have contented himself with translating the formulæ and equations of the last edition into the language now generally used by chemists. From the length of time which has elapsed since the work was first published, it is to be regretted that Dr Macadam did not re-write it entirely, as his "Text-book of Practical Chemistry" shows that he was well able to do.

Notwithstanding the lack of systematic text-books on chemistry, we have long had abundance of practical treatises. Among new books of this class, we may mention Spencer's "Elements of Qualitative Chemical Analysis," which is accurate and well-arranged. A large size of page is used, and the author is thus enabled to exhibit at a glance the reactions of each group.

A new edition has been published of Odling's well-known "Practical Chemistry," probably the best book of its kind for a medical student.

Histological Demonstrations: A Guide to the Microscopical Examination of the Animal Tissues in Health and Disease; being the Substance of Lectures delivered by GEORGE HARLEY, M.D., F.R.S., Professor in University College. Edited by GEORGE T. BROWN, M.R.C.V.S., Professor of Veterinary Medicine, and one of the Inspecting Officers in the Cattle Plague Department of the Privy Council. London: Longmans, Green & Co.: 1866.

THERE is a want of a good, short English work on Histology. The best treatises on the subject in our language, either, like Dr Sharpey's, or Todd and Bowman's, form part of larger works on anatomy or physiology, or, like the translation of Kölliker's excellent Manual, are too large in size, and too detailed to suit a beginner. Kölliker's work, the style of which is, unfortunately, somewhat tedious and unattractive, is less a text-book than a work of reference for consultation by the anatomist, or by the advanced student, who is devoting special attention to the study or investigation of minute structure. What is needed, within a brief compass, is a clear outline of the science of histology, with simple and correct descriptions of the principal structures or tissues, prefaced by proper directions for handling the microscope, and preparing the different objects for examination. The histological demonstrations of Professor Harley and Mr Brown appear to be designed to supply this deficiency. The text has been prepared by Mr Brown from Dr Harley's notes, and with his concurrence, and is intended to represent the method of instruction practised, and the principal information conveyed in Dr Harley's lectures and demonstrations in the Physiological Laboratory of University College. The idea of the work originated with Mr Brown, who was so satisfied with the success of Dr Harley's method of teaching as to desire to extend the benefit of it to a wider circle. The little book is full of matter. Within about 260 pages are comprehended an account of the microscope and apparatus, with the mode of using them, and a description of the minute structure of tissues and products, both healthy and morbid, including parasites, while numerous woodcuts, many from Kölliker, and some original, are introduced for the purpose of illustration.

Useful as this little book is in some respects, we regret that we cannot recommend it as a treatise on histology. We are not disposed to find fault with such a mere put-off as the chapter on the internal ear, for example, because, where so many subjects are treated of in a small space, completeness is impossible. It would have been preferable, however, either to have given a fuller description, or to have omitted it altogether. Certainly the delicate and beautiful structures of the cochlea are too difficult for examination by the beginner, although a knowledge of them is useful and interesting, and might stimulate the young inquirer to future research. But such omissions are of minor importance in an elementary work. What is of more consequence, is that the information, so far as it goes, should be useful, correct, and well arranged. Now, the plan of the book is, we think, on the whole, good, and well adapted for a practical guide. The directions for using the microscope, and preparing objects for inspection, are sufficient, with a little assistance, to enable the inquirer to set to work for himself. But it is with the more purely histological part that we find reason to be dissatisfied. The descriptions of many of the tissues are neither so clear nor so accurate as we should have expected, and the principles of histological science, so far as they are given, are very unsatisfactory. In short, though useful as a working manual, the treatise cannot, without certain corrections and improvements, be regarded as an accurate exposition of the present state of histology.

At the very beginning of the histological part, in treating of cells, we are told that "the ultimate fibres of muscles are formed of corpuscles arranged in rows," and that "even the seemingly homogeneous filaments of fibrous tissue are associated with occasional nuclei, which bear testimony to their cellular origin." Now, these statements, though true in a sense, and as representing the peculiar views of certain observers, are quite erroneous as general propositions, and are calculated to confuse and mislead. The account of the origin of cells, in like manner, sets out with the assertion that, "1. Cells may arise from a formative fluid, derived from the blood, called the blastema;" and no reference whatever is made to the great principle, which, right or wrong, is one of the leading ideas of modern histology, that cells arise exclusively from pre-existing cells, "omnis cellula é cellula." Even supposing that Dr Harley does not agree with this maxim, so strenuously maintained by Virchow and his school, yet surely some prominent notice ought to have been taken of it. In the same way the description of fibrous tissue is very defective, and throughout there seems no endeavour to simplify the study of histology, by a judicious classification of tissues. So far as it is an exposition of histological science, the book might have been written twenty years ago.

In the chapter on muscle we find a repetition of the usual confusion, for which we do not specially blame Dr Harley, but which has been already too long retained in our text-books. "Two kinds of muscle," we are informed, "exist in the animal body, differing from each other in structure and function—1. Voluntary muscle, which contracts under the influence of the will; 2. Non-voluntary muscle, which acts independently of the will." The author then proceeds to the "*Examination of Striated or Voluntary Muscle*," which is followed by that of "*Involuntary Muscle, Striated variety*," and finally, the "*Non-striated variety*." How any one could derive clear notions from such distinctions, it is difficult to understand. Voluntary and involuntary muscle are first stated to differ in structure as well as in function; then striated is put as equivalent to voluntary (striated or voluntary muscle), and immediately afterwards, involuntary muscle is introduced by the *striated* variety. It is certainly time that all this confusion should cease. Many years ago, John Reid pointed out that there was no propriety whatever in the distinction of muscular tissue into voluntary and involuntary, and that error would certainly ensue from regarding the striped fibre as the voluntary, and the smooth as the involuntary. It had long been known, for example, that the muscular fibres of the heart in man, and of the upper part of the œsophagus, were striated, yet no one could for a moment suppose that they are voluntary. Yet the true distinction by which all confusion may be avoided is sufficiently simple. No muscle is voluntary or involuntary in virtue of any peculiarity of structure, but solely in consequence of its nervous connexions, which may render it subservient to voluntary, or involuntary, motion, including reflex and other varieties of it. *The difference between the striped and the unstriped fibre is, not that the one is voluntary and the other not, but that the former is designed for rapid, and the latter for slow, action.* This is a fact which their behaviour under stimulation clearly demonstrates. It is easy to understand, on this principle, that as the muscles which obey the will must act rapidly in order to be efficient, so they must be composed of the striped kind of fibre. On the other hand, most of the involuntary muscles, like those of intestines, require to act slowly and continuously, and are consequently made up of the smooth kind of fibre. But in such an involuntary muscular organ as the heart, whose peculiar function implies celerity and precision of contraction and relaxation, the employment of the striped fibre is again required. It is proper then to speak, if we please, of voluntary and involuntary muscles or muscular organs, in respect of their nervous relation; but in classifying muscular tissue, we ought to speak only of the striped and unstriped or smooth, not of voluntary and involuntary. And this accuracy of language, which is required to avoid confusion in human anatomy, is still more necessary in comparative histology. For the medical student it is especially needful. Again and again have we heard students, in disputing with their fellows, maintaining that the fibres of the heart must be non-striated, *because* the heart was an involuntary muscle. In examination-questions by the different boards the same confusion is frequently obvious, and is sometimes made the occasion of so-called catch questions. And we have even witnessed an anatomical lecturer, skilful in dissections, and in naked-eye anatomy, but unpractised with the microscope, assure his audience, *more solito*, that muscular fibre is divided into the voluntary or striped, and the involuntary or smooth, and immediately proceed, somewhat unluckily, to instance the heart as the principal involuntary muscle, and point to a diagram of smooth muscular fibre-cells as the kind of fibre of which the ventricles are composed. But so long as confusion exists, errors must naturally follow. We add only one word more. So far back as 1845, Kölliker demonstrated, as others have often done since, that the smooth muscular fibres are made up of spindle-shaped fibre-cells, yet we find Dr Harley, after repeating the old description of the fibres, making the dubious statement, "that some authorities consider involuntary fibres to be constituted of elongated nucleated cells cemented together."

In conclusion, the book, we repeat, may be useful as a guide to work with the microscope in the investigation of minute structure, but it is not to be relied upon as accurately representing the present state of histology, either in its general principles or its details. Perhaps in a new edition these faults may be corrected. There are also some inaccuracies due to want of care, which should be removed. Thus, at p. 173, we are told that "A corpus luteum is formed after the escape of the Graafian follicle from the ovarium;" and such words are found as "vibrines," "foliæ," "ciliæ," "dermis," "ptyriasis," and "schirrus," which is so spelt wherever it occurs. The woodcut representing the cornea (fig. 135), also gives a very inaccurate idea of the structures composing it.

Histology is in truth only a branch of the great science of development. It can never be really understood until it is studied in that connexion. But, meanwhile, even in the present modes of teaching it, it serves as the foundation for a great and important part both of physiology and pathology, and it is essential that its principles should be accurately explained.

Orthopraxy: The Mechanical Treatment of Deformities, Debilities, and Deficiencies of the Human Frame: a Manual. By HENRY HEATHER BIGG, Assoc. Inst. C.E., Anatomical Mechanist to the Queen and Prince of Wales, the Royal Hospitals of Chelsea and Greenwich, the Board of Ordnance, the Admiralty, the East India Department, St George's, St Thomas', Guy's, Middlesex, King's College, Royal Free Hospital, etc. London: John Churchill and Sons: 1865.

THIS work, we are told in the preface, is founded upon four others which, at different dates during the last ten years, the author has given to the public. Two of these describe artificial limbs, another is on the subject of localized movements, or muscular exercise in the treatment of deformities, while the fourth, which has already been favourably noticed in this Journal, is devoted to the mechanical appliances necessary for the treatment of deformities.

These works have not been merely bound up together, but so modified and recast, as really to result in a new and very complete work.

An introduction of considerable length, begins with a rather clever special pleading in behalf of "mechanical therapeutics" as a new branch of the healing art; an unsuccessful attempt to prove that "mechanical therapeutics" should be considered a branch of medical *science;* and some sensible remarks on the necessity of its being practised as a separate mechanical *craft.* The name Orthopraxy is then introduced and defended, and a rapid and amusing summary given of the history of orthopraxy from the days of Hippocrates down to the volunteer movement of our own day.

The remainder of the work is divided into four chapters, and each of these again into three sections,—the latter being devoted to the deformities, debilities, and deficiencies of the four great regions of the body: head, upper extremities, trunk, and lower extremities, each of which has a separate chapter.

The work is very full and elaborate, illustrated with numerous wood-engravings, and written in a pleasant amusing style. In the mechanical ingenuity and completeness of the very numerous instruments and appliances, there is scarcely anything left to be desired; as surgeons, we might find more to criticise in the cases to which they are applied.

In the treatment of spinal disease, especially, we believe that the use of mechanism is, in many cases, much to be deprecated; in all, it is to be used with extreme caution.

As a surgical mechanist, however, we must allow that it is Mr Bigg's duty to be able to devise and supply mechanical aid for every case in which it is ordered, and, this we think, his work proves he is thoroughly able to do.

The section on artificial arms is one of the most interesting in the book, the number and variety of the instruments which can be fitted on to the stump being very great; from the useful fork, and the more dangerous pen, to the nail-brush or file, the reins, the pacific rake and hoe, the more doubtful pruning-knife, and the unmistakable dagger or gun. A most ingenious artificial hand is described by which a celebrated French operatic singer, who had lost his right arm above the elbow, was enabled again to use it for the purpose of histrionic declamation, and with it to draw his sword from its scabbard.

"A hand thus constructed," says our author, "when the fingers are placed in a natural position, leaves nothing to be desired in the way of symmetry. Often, indeed, the artificial hand is more symmetrical in aspect than the natural, and, in a spirit of coquetry, the gloved substitute is frequently displayed in preference to the real hand." A most fascinating account of the artificial leg, invented by Dr Bly, of Rochester, U.S., shows what may be done by the surgical mechanist for losses of the lower limb; the free movements of the knee and ankle, combined with strength, being almost as wonderful as the powers given by springs and catgut to the fingers of the artificial hand.

The work is got up with very great neatness and taste.

The Toxicologist's Guide: a new Manual on Poisons, giving the best Methods of Manipulation to be pursued for their Detection (post-mortem or otherwise). By JOHN HORSLEY, F.C.S. London: Longmans and Co.: 1866. Pp. 70.

THIS little manual gives the tests of all the ordinary poisons, with the exception of the mineral acids. Mr Horsley is an experienced chemist, and has evidently studied with care the reactions of the alkaloids. These are given in detail, and the colorations spoken to are rendered more intelligible by means of a plate representing seventeen of the coloured reactions. On comparing two copies of the Guide, however, differences in the shade of colour are observed in similar plates. This may depend on their being hand-coloured by persons not aware of the importance of preserving the exact tint. In a subsequent edition this should be looked to. For where substances are spoken to as being present from certain changes of colour produced by re-agents, it is especially necessary that there should be no dubiety as to the exact shade of colour. This illustrates, however, the danger of trusting implicitly to colour tests alone in determining the presence of poisons. No doubt, Mr Horsley tells us, p. 68, that "the practised eye is a much better judge of quantity, by the intensity of shade produced, than the most accurate balance is of weight," and gives us at the same time directions for the detection of 100,000th of a grain of strychnia. Such a statement, and also the detailed reactions of rare alkaloids, might lead some to suppose that the detection of these substances was an easy matter. Our author warns such, however, that "as regards the 'vegetable poisons,' it is very *questionable* whether, having been absorbed and passed through the circulation, they can be found at all;" and with reference to infinitesimal quantities, he is careful to add, "of course this is understood as applying to the *pure* poison (free from organic matter)." That is the difficulty—the toxicologist is called upon to conduct a search for alkaloidal principles in the midst of organic matter undergoing putrefaction. It is easy for the chemist, with the substance separated and specially prepared, to devise tests for it, and by dilution, to estimate the quantity present by the intensity of the colour produced by the various tests. But all this is of no practical importance, unless we have processes which enable us to separate the poison from organic matter in such a state of purity as to allow of the satisfactory application of the tests. In giving these reactions in detail, Mr Horsley's manual conducts the analysis of poisons on a definite plan. The process for the detection of unabsorbed poison is first given, and this is followed by that for poisons which have been absorbed and contaminated with organic matter. The various steps of the analysis, in both cases, are carefully described; and we agree with the author in thinking, that advantage is gained by the positive statements of a concise manual, which have all been tested by an experienced chemist, as "reference to larger works in cases of emergency is often attended with inconvenience."

Cancer: A New Method of Treatment. By W. H. Broadbent, M.D., London; Assistant-Physician to St Mary's Hospital, and Lecturer on Physiology at the St Mary's Hospital Medical School. Pp. 28. London: John Churchill and Sons. 1866.

After the thorough ventilation which the subject of the treatment of cancer by injections has lately undergone in the correspondents' corner of the weekly Medical Journals, we need at present hardly do more than merely express our opinion that Dr Broadbent has fairly made out his claim to the credit of introducing a method of treatment which aims at the solution and absorption, not the destruction, of tumours by sloughing.

Having adopted the idea of injections of the cancerous tumour by the hypodermic syringe, in the hope of retarding or arresting its growth, Dr Broadbent selected acetic acid for the following reasons:—

> "1. This acid does not coagulate albumen, and might therefore be expected to diffuse itself through the tumour. The effect would thus not be limited to and concentrated in the point injected.
>
> "2. If it entered the circulation it could do no harm in any way, either by acting as a poison or by inducing embolism.
>
> "3. Acetic acid rapidly dissolves the walls and modifies the nuclei of cells on the microscopic slide, and might be expected to do this when the cells were *in situ*.
>
> "4. It had been applied with advantage to open cancer and to cancerous ulcerations.
>
> "The experiment was made, and it was found that acetic acid, though in healthy tissues it causes very severe smarting and burning, unless very strong, gives little pain when thrown into malignant structure. On the other hand, it acts energetically on cancer, but has comparatively little effect on normal structures."

Dr Broadbent's cases are four in number, besides a fifth reported by Mr A. Cooper. For details we must refer to the pamphlet; but the cases were:—

1. Scirrhus of breast, recurrent after two removals. After various applications of acetic acid by injection, and carbolic acid and glycerine to the surface, the ulceration is reported, after five months' treatment, to be *nearly healed.*

2. Secondary cancer of glands in axilla, with enlargement of the arm. Contraction of the diseased glands, and diminution of the swelling in the arm.

3. Cancer of the rectum, with obstruction. Pain palliated, and obstruction diminished, by weekly injections of acetic acid.

4. Cancer of the tongue, with enlarged glands. By injections the size of the tumour was diminished, and the ulceration became less fœtid.

The fifth case—reported by "Mr Alfred Cooper, of St Mark's Hospital for Diseases of the Rectum"—is so remarkable as to deserve quotation, but so astounding in its results as simply to be incredible. Without venturing to throw a single doubt on Mr

Cooper's veracity, we prefer the possibility of a mistake in diagnosis, even by a surgeon to a special hospital, to such an absolute overturn of all our surgical principles as would be required in accepting this most unique case. We simply quote it *in extenso*, without further remark:—

> "The disease was of long standing; the patient's sufferings were extreme, and he was in the last stage of exhaustion. There was evidence also of extension of the disease to the liver; no hope could be entertained of saving life, but it seemed possible that some relief might be given, and at the worst an opportunity would be afforded, by post-mortem examination, of ascertaining the effects of the injection.
>
> "I saw the case with Mr Cooper on July 6th. On examination the finger came upon a mass of disease apparently filling the entire pelvis, and the canal by which fæcal matter passed could not be made out. About eighty minims of acid were injected at twice. The manipulations and the entry of the acid gave pain, and he was weak and ill afterwards. A long drive in a cab contributed to this, as he was exhausted before any examination was made. The injection was repeated on the 13th. He experienced no marked relief till, on Aug. 28th, he passed a solid substance, described by himself as 'being large enough to cover a crown-piece, and as tough as chamois leather,' after which he suffered less. His strength, however, was exhausted, and he died on September 7th.
>
> "On post-mortem examination, the obstruction in the rectum was found to have entirely disappeared, and no trace of the tumour was to be found. The liver and other abdominal organs were extensively affected with cancer."

Dr Broadbent recommends that the injection should be gradual, that the proportion of the mixture of acid with water should vary with the case, but should never be so strong as to cause pain. From what experience we have yet had of this method of treatment, we believe it worthy of the fullest trial, and indulge the hope that in some cases it may cure; in others, palliate; in others, again, (and perhaps these may be the most frequent and the most useful), prepare the way for other remedies by diminishing the size and subduing the tendency to increase of malignant tumours.

The Use of the Laryngoscope in Diseases of the Throat, with an Appendix on Rhinoscopy. By MORELL MACKENZIE, M.D. Second Edition. London: Hardwicke: 1866.

THE fact that, within a very short time, a second edition of this work has been called for, justifies the favourable comments which were passed in these pages upon the first edition. It is sufficient now to add that Dr Mackenzie has, by careful revision, amplification, and the addition of woodcuts, made his book more than ever deserving of the attention of those who desire a full, concise, and simple guide to the art of exploring, and of applying remedies to the posterior nares and larynx.

Part Third.

MEETINGS OF SOCIETIES.

PROCEEDINGS OF THE EDINBURGH OBSTETRICAL SOCIETY.

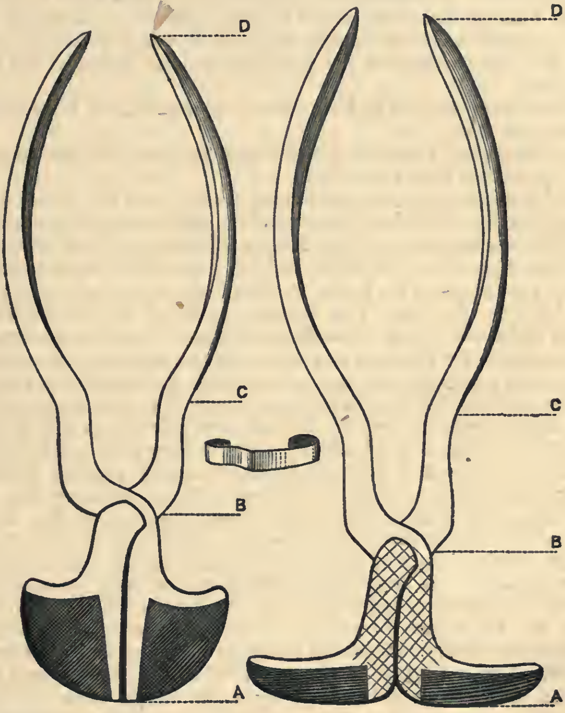
SESSION XXVI.—MEETING II.

28*th November* 1866.—Dr Charles Bell, *Vice-President*, in the Chair.

James Watson, L.R.C.P., Aberdeen, was admitted as a Corresponding Fellow of the Society.

REMARKS ON SOME NEW FORCEPS. BY DR ANDREW INGLIS.

Fig. 1. Fig. 2.



Measurements of No. 1.	
From A to D, ...	10 inches.
„ A to C, ...	6 „
„ A to B, ...	3·5 „
Breadth of handle, . . .	2·5 „
Weight, 13 oz.	

Measurements of No. 2.	
From A to D, ...	10·3 inches.
„ A to C, ...	4·4 „
„ A to B, ...	2·2 „
Breadth of handle, . . .	4·5 „
Weight, 14 oz.	

The blades of both are made very like those of Sir J. Simpson's, but in the short pair the distance from B to C is less, and in the long pair it is greater. There is some objection to having two different instruments, more particularly in country practice; but in bringing forward this novelty, it is not wished to assert that there is any particular advantage in these two special forms of blades, but only to recommend that form of handle which, of course, may be fitted to any kind of blade.

Dr Inglis showed several new patterns of midwifery forceps. One by Mr Archibald Young was somewhat like that shown some time ago by Dr Cappie.

The joint which was in the middle of the instrument consisted of a rod and socket like that of a bayonet, the projection, however, from the side of the rod was not fixed as in the bayonet, but was projected outwards by a spring, so as to catch when pushed in without requiring to be turned round. The second pattern was his own invention. The handles were short, and consisted only of two large lateral horns for traction, like those on the handles of Dr Radford's and Sir J. Y. Simpson's forceps. The reasons for this change he asserted were, that good forceps blades well applied ought not to slip even when very little compression is used, and that if traction be the principal use of midwifery forceps, the handle ought to lie across the line of traction (like the handle of a corkscrew) and not in that line. He has used them in seven cases, several of which were very severe, and in all they seemed to answer admirably.

Dr Charles Bell objected to the forceps shown by Dr Inglis, as the handles were so short and inconveniently formed that they afforded insufficient power of compression, and of altering the position of the head when that was necessary, and in consequence there would be great difficulty in keeping the blades in apposition with the fœtal head; although it was possible to use perfect traction. The construction of the joint was such as to render the instrument very apt to slip.

Dr A. Macdonald said he had seen the forceps used, and his experience was that they did not slip.

Dr Cappie exhibited a new kind of forceps, between long and cross handles, which were separated from the blade.

Dr Keiller considered the exhibition of forceps and the observations made on their comparative advantages worthy of the serious attention of the meeting, and stated his own views regarding the practical value to be attached to the proposed improvements of Dr Inglis, and the ingeniously contrived instrument exhibited by Dr Cappie. Dr Inglis' modification of the handle of the forceps seemed to be very like that of an instrument which he (Dr Keiller) now exhibited, the difference being exceedingly slight. The two instruments now brought forward by Dr Inglis as novelties, did not appear to him (Dr Keiller) as exhibiting any peculiarity beyond a very slight deviation from that which he had had for years past in his possession, and which he had frequently tested in practice.—The instrument he (Dr Keiller) now referred to and exhibited was (as the members present could readily judge), not at all unlike those of Dr Inglis, the improvement, if any, consisting in merely lopping off about two-thirds of the ordinary handle of the ordinary forceps, with the view of getting rid of the supposed disadvantage of the usual lengthy handle, and thereby securing a more portable size of instrument. Dr Keiller could not agree with Dr Inglis or others, who had arrived at the conclusion that thus shortening the forceps handle, so as to convert it into a mere stump, was a useful modification. On the contrary, apart from the undoubted advantage of increased portability, he (Dr Keiller) contended that the process of docking, now so strongly recommended, would not be found so advantageous as it may at first sight appear. Want of length in the handle of any kind of forceps necessarily implies want of power and leverage, which, however, were not required in simple short forceps cases where little extractive force was necessary. But the chief objection to be urged against the short-handled form of instrument now exhibited, was the difficulty of keeping them *in situ* when introduced. Dr Keiller could not imagine the forceps of Dr Inglis ever being equal to Simpson's. He (Dr Keiller) objected to short handles, as the first blade was very apt to turn round during the application of the second blade; and he found that they wanted compressive power. With long handles you have much greater command. Dr Keiller also objected to cross handles, as they were apt to hook on the bed-clothes, unless the patient was laid over the edge of the bed.

Dr Bryce disapproved of Dr Inglis' forceps in any case. He frequently found one blade of ordinary forceps act as a vectis, and the child was born before the second blade was applied.

Dr Keiller said that it was of the greatest importance that we should have an instrument fitted to overcome all cases, and such an instrument would be found in that of Sir James Simpson, which he (Dr K.) preferred and in all cases used.

Dr Main agreed with Dr K., in the advantage, especially for country practice, of having one pair of good efficient forceps, such as Simpson's long forceps.

Some conversation followed on the proper position of the patient, Dr Keiller approving of the patient being brought to the edge of the bed, so as to bring the axis of the pelvis into the proper direction for the application and use of forceps.

Dr Inglis said he preferred operating with the patient on the back, as he thought the position most suitable (in short forceps cases) for all the different steps of the operation, and he quoted a case where the child's head had been compressed from side to side, reducing the breadth to two and a-half inches, as an instance of how much power might be exerted by adopting the supine position. He first placed the patient on her back and pulled downwards, and when the head descended and the resistance at the outlet was found to be very great, he turned the patient on her right side, facing himself, and pulled towards him. The forceps were on for two hours. Both mother and child recovered without a bad symptom, and the head regained its shape in a very short time. This good result he believed to be owing to the traction not having been made continuously, but only during the pains. Another patient, Dr Inglis said, had had craniotomy performed in her two first labours, and at her third, he had pulled in a similar manner for nearly two hours and a-half, and succeeded without injury to the mother, who made a rapid recovery. After craniotomy, she was always confined to bed eight *weeks;* after the forceps, she was only eight *days.*

Dr Cochrane objected to the extreme length of time in which the forceps were applied.

Dr Cappie had frequently used the forceps for one and two hours.

Dr Keiller thought ten or fifteen minutes amply sufficient for ordinary forceps cases; Dr K. had been called to a case two days ago, where he used the forceps for a longer period, but it was an unusually difficult case. The periods mentioned by Drs Inglis and Cappie seemed extraordinary, for the object of forceps was to save time and life, which their application and use for a couple or more hours were not likely to promote. If the case was a proper one for forceps, there was seldom any occasion for delaying the necessary degree of compression and extraction. The condition of the child's heart, however, as ascertained by the stethoscope, and the state of the mother as influenced by the length and character of the labour, must ever be our guides in practice in regard to the having recourse to operative procedure, and, therefore, mere *time* is not to be considered in comparison to the actual condition and circumstances of the case in hand.

Dr Charles Bell thought ten minutes or a quarter of an hour quite sufficient time for the application of the forceps, and he never remembered having them longer in the uterus than twenty minutes.

Dr Inglis objected strongly to using traction in the absence of pains. He considered that it was a rule universally accepted and applicable to all labours, never to diminish the contents of the uterus in the absence of pains. The violation of this rule had, in his experience, almost invariably been followed by bleeding. He quite admitted that the fœtal pulse was a most important guide in the conduct of a forceps case. If a pain, even of ordinary intensity, could by lasting too long have a fatal effect on the child, how much more likely was the continuous use of the forceps to produce such a result? For this reason, he always slackened his hold of the head between the pains, and only used traction during their continuance.

Dr Main thought half-an-hour a comparatively short time in severe cases, but then he only used traction during the pains, and not continuously.

DUNDEE MEDICAL SOCIETY.

At the Monthly Meeting of this Society, held on Wednesday, 2d January, Dr F. W. Miller read a report of a case of suppression of urine.—(This will appear in an early number.)

Dr W. L. Gibson showed some specimens of hippuric acid which had occurred as a very abundant and nearly pure natural deposit in the urine of a somewhat delicate boy, about three years of age. The deposit continued to be present for about twenty-four hours, when it was replaced for a similar period by an abundant sediment of pure uric acid, which in its turn ceased as abruptly as the former.

Dr Gibson desired to know whether any of the members had met in their practice with an instance of cannabis indica in the dose of a grain of the extract, producing poisonous symptoms, with delirium of a peculiar character, presenting for the time very much the appearance of mania of a melancholic type: an effect exactly the contrary to that exhilaration which is generally supposed to be produced by this drug. This had occurred five or six times in his experience, and in one patient twice, at an interval of four years, the medicine having been taken the second time through a mistake.

Part Fourth.

PERISCOPE.

MEDICINE.

CAUSE AND NATURE OF TUBERCULOSIS, AND ITS TRANSMISSION BY INOCULATION.

BY DR VILLEMIN, PROFESSEUR AGRÉGÉ AU VAL-DE-GRACE.

More than a year has elapsed since M. Villemin presented to the Academy of Medicine the first of those interesting memoirs relating to the inoculability of tuberculosis, in which he endeavoured to prove, by experimental researches, that pulmonary phthisis, as well as other tuberculous diseases, is a specific affection, that its cause resides in an inoculable agent, and that tuberculosis belongs to the class of virulent diseases and ought to be placed in nosological relation with syphilis, or, perhaps, still nearer to farcy or glanders. We purpose to give a short account of M. Villemin's researches and results.

Setting out with the oft-told tale of the immense amount of labour that has been bestowed upon investigations into the nature of phthisis, which, in spite of all, still remains the scourge of humanity; of the numberless hypotheses which encumber the pathway of research, and of the discouragements which hinder even the most ardent student—he first pauses at the still prime questions, What is the nature of phthisis and tuberculous diseases generally? and what is their intimate cause? The memoirs are his answer to these questions.

Already every stone seemed to have been turned in the search for an answer to these questions; there is scarcely a single circumstance in the history of mankind that has not at some time or another been charged with the origin of phthisis. The most generally accepted opinion is that it is cradled on depressing influences of every kind; but, asks M. Villemin, are there, on the one hand, many diseases which escape such a general law as this, and, on the other, are there not many exceptions in favour of phthisis? Does every deterioration of the organism end in phthisis? Is every robust constitution spared by it? Tom Sayers, the famous English pugilist, died of consumption, and I could name a hundred cacochymics who have died of old age! There is no morbid predisposition which does not require a determining cause for the development of disease, nor, on the other hand, can the hardest constitution resist for any length of time a concentrated etiological agent. In nature there is nothing but action and reaction, spontaneity exists no more in biological phenomena than in physico-chemical facts.

M. Villemin next reviewed the various causes to which tuberculosis has been attributed, endeavouring to attach a just value to the questions of temperament and inheritance, which are so intimately associated with the subject; he discarded as much as possible the word diathesis, as being an indefinite term, becoming mischievous when it occupies the mind in place of facts; he endeavoured to animate the study of morbid processes by pathologico-physiological ideas; he investigated the litter of scientific *equivoques* and *non-sequiturs* collected under the title of scrofulo-tuberculous affections; he watched tuberculosis in its solitary paths as well as in its accustomed haunts; he compared it both anatomically and clinically with other morbid entities; and finally, combining all these problems with the principles of the individuality of the organic elements, he arrived by induction at the following conclusions, opposed though they be to many popular notions, namely, that,—

Tuberculosis is the effect of a specific causal agent,—in one word, of a virus.

This agent, like its congeners, is discoverable in the morbid products, which are the result of its direct action upon the normal elements of the affected tissues.

Introduced into an organism capable of being affected by it, this agent is reproduced, and, at the same time, reproduces the disease of which it is the essential principle and the determining cause.

M. Villemin believes that his experimental researches have substantiated these inductive hypotheses.

1*st Experiment*, 6*th March* 1865.—Two healthy looking rabbits, about three weeks old, living with their mother (and still sucking) in a hutch raised from the ground, and suitably protected, were selected for the experiment. In one of them a small subcutaneous wound was made behind one of its ears, and into it was inserted two small fragments of tubercle, and a small quantity of puriform liquid, taken from the lung and intestine of a phthisical patient who had died thirty-three hours previously. On the 20th March and 4th April the inoculation of tubercle was repeated. These inoculations were followed by local conditions to be described hereafter. On the 20th June, that is, after a lapse of three months and fourteen days, no appreciable change had occurred in the health of the little animal; it had grown a good deal. It was killed, and the following post-mortem appearances were recorded:—A tablespoonful of serum in the peritoneal cavity; miliary tubercle along the larger curvature of the stomach, in two parallel lines on each side of the mesial line. The granulations were grey, very minute, oblong; many of them presented a small yellow opaque point in the centre. In the intestine, at a distance of about two or three centimetres from the stomach, there was a considerable quantity of tubercle about the size of hemp-seed. Smaller and less prominent tubercle was scattered here and there in the small intestine. There was tubercle in both substances of the kidneys. The lungs were full of large masses of tubercle, evidently formed by the agglomeration of many granulations; these masses had the dimensions of a large pea; their cut surfaces presented a grey transparent appearance with several minute yellowish-white points. Microscopic examination confirmed the tuberculous character of all these products. The other rabbit, which had lived under precisely similar conditions, but without the inoculations, was then killed, and was found to be perfectly free from tubercle.

2*d Experiment*.—On the 15th of July, three fine, healthy looking rabbits, living in the open air, in an enclosure in which there was a covered retreat, and enjoying ample and varied nourishment (bread, bran, and forage) were inoculated. On the 22d of the same month, the operation was repeated on all the three, and at the same time, a fourth rabbit, belonging to the same litter, and living with them, was inoculated for the first time. The four rabbits were killed in succession on the 15th, 16th, 18th, and 19th of September, when the following post-mortem appearances were recorded:—

No. 1. Pulmonary tubercle abundant, transparent grey, projecting from the surface of the lungs, arranged in plates the size of a lentil, with unequal surface, shagreened, formed of agglomerated granules, and presenting yellowish points on the cut surface. Miliary granulations were also observed. The lung was rose-coloured, with no trace of inflammation.

No. 2. Pulmonary tubercle nearly resembling that of No. 1; one mass is already in great part opaque yellow; a dessertspoonful of serum in the pleural cavity.

No. 3. Pulmonary tubercle as in the preceding cases; yellowish-white tubercle in the vermiform appendix.

No. 4. (This rabbit was only once inoculated, on 22d July). Pulmonary tubercle appearing, especially in the left lung; six or eight nodules of the size of a pea projecting from the surface of the lung, and formed by the agglomeration of many granules. A considerable number of small granulations were also found, which were surrounded by a light reddish congestive areola. Tubercle in the peritoneal covering of the liver. Three masses on the upper part of the small intestine.

Whilst these rabbits were undergoing experiment, others which had lived with them, but without being operated upon, were killed for physiological purposes, and presented no appearance of tuberculization.

Having proceeded thus far, M. Villemin felt it to be his duty to communicate his experiments and their results to the Academy of Medicine, making the statements, with reference to tuberculosis, which have been already mentioned at the head of this paper.

3d Experiment.—On the 2d October, three pairs of rabbits, about three months old, were procured, the two of each pair being mothers and of the same litter. One rabbit of each pair was inoculated, the others were untouched, and all were put together into a single habitation having three compartments. The inoculation was repeated on the 24th October, and at the same time, a fourth, full-grown and extremely vigorous rabbit was inoculated.

Pair No. 1.—On the 23d of November, the inoculated rabbit was discovered dead, and on post-mortem examination presented the following appearances:—Two red patches occupied the fronts and posterior borders of the lungs; these patches were constituted of the slightly thickened pleura, and of a layer of congested subjacent pulmonary parenchyma. In the middle of these patches were observed very small greyish granulations, situated chiefly beneath the pleura. The kidneys were remarkable for the great quantity of cysts, developed in the cortical substance, filled with a transparent fluid. Many of these cysts projected beneath the capsules of the kidneys. The brother rabbit was immediately killed and presented no organic lesion.

Pair No. 2.—(29th November). The inoculated rabbit, put to death, presented a very large quantity of miliary granulations, situated chiefly beneath the pleura, which they raised, and equally distributed in both lungs. Similar granulations, but relatively less numerous, deeply seated in the parenchyma. Two aggregations of granulations formed small masses of irregular contour, projecting, and of the size of a small pea. The brother rabbit was entirely exempt from tubercle.

Pair No. 3.—(29th November). The inoculated rabbit presented in both lungs red marblings, clearly defined from the healthy pulmonary tissue, in the midst of which were observed minute grey nascent granulations, to the extent of two or three in each spot, situated beneath the pleura. Tuberculosis had evidently only just begun, and it is probable that a few days sooner no tubercle would have been found. The other rabbit was free from pulmonary or other lesion.

No. 4.—The adult rabbit, which was inoculated at the same time, and had been under the same circumstances as the others, presented the following post-mortem appearances on the 27th November:—The entire surface of the lungs was riddled with sub-pleural granulations; the smallest of these were surrounded by a congestive areola; those which had attained a certain size no longer presented this peculiarity. Forcibly projecting on the surface, moreover, were found two or three tubercles of the size of a small pea. The parenchyma was also strewed with granulations. The surface of the spleen was equally covered, and there were also observed three or four spread and flattened tubercles. The histological examination of all the tuberculous products thus obtained, substantiate the characters previously accorded to tubercle by M. Villemin, in his memoir, *Du tubercle au point de vue de son siège, etc.* (Paris, J.-B. Baillière, 1862). Whilst these inoculations were progressing, M. Villemin inoculated another rabbit with various substances, such as choleraic discharges, pus from a phlegmonous abscess, pus from anthrax; this rabbit was killed on the 30th November and presented no anatomo-pathological peculiarity.

Such were the experiments by which M. Villemin sought to establish his hypothesis; and he deemed them sufficient, but at the same time was prepared to receive contradictory evidence; and in order that other experiments might be conducted in a similar manner, he next proceeded to explain some circumstances of the operations. In spite of all precautions, he admits that every experiment might not be successful, just as we have failures frequently enough with vaccination. At that time, however, all his own experiments had succeeded, for in order to make them so he had been careful to repeat the experiment in all cases except one (No. 4 of the second series), which, nevertheless, was rendered equally tuberculous. The rabbits, he adds, were rendered tuberculous, not phthisical; for, being anxious to determine the general principle, he had not allowed sufficient time to elapse for the completion of the disease.

Not knowing at what stage of its development tubercle was best adapted for inoculation, M. Villemin always collected it from two granulations, the one grey, the other at the commencement of softening. He selected it as much as possible elsewhere than in the lungs, in order that he might avoid those secondary inflammatory products which are more common in these than in other organs. The subjects from which the matter was taken had been dead not longer than from twenty-four to thirty-six hours. With a narrow-bladed bistoury he made a slight puncture near the base of the ear, and into the wound he insinuated a small fragment of tubercular substance, previously broken up by the point of the instrument. The local phenomena which resulted from this slight operation were the following:—During the first two or three days nothing appeared: the inoculation puncture was closed by a slight incrustation; but generally about the third day a little redness, heat and swelling were perceptible. At the end of three or four days, this redness and swelling disappeared; but there remained beneath the puncture a slight nodosity movable with the superjacent skin. This nodosity was sometimes found without apparent previous redness or swelling; it then increased, and, after a longer or shorter time, an opening appeared upon the skin by which a very thick white purulent matter escaped. The small aperture then closed, and the nodosity disappeared; only, however, to reappear after a little while; again to empty itself; and so on. The first of the rabbits had a small ulcer situated above the inoculation wound, which was probably due to the introduction of putrid sanious fluid taken from a pulmonary cavity. This phenomenon was not repeated in any of the other inoculated animals.

One of the morbid processes which is not to be mistaken for tubercle in the autopsies of the rabbits, consists in small cysts met with in the livers of nearly all the cases: cysts which frequently contain oviform bodies analogous to the tænia eggs, and which, in degenerating, assume the appearance and consistence of small yellowish tubercles.

A year later, M. Villemin again publishes the views which a larger experience has enabled him to extend and confirm. After admonishing the reader against the danger of mistaking other products for the only true tubercular degenerations, and mentioning a case in which he had himself been nearly deceived by certain appearances in the organ of a non-inoculated rabbit, he refers to the limited range of other specific diseases in the animal kingdom, and proceeds with the following statement,—that *beyond the human race, which has, unfortunately, a special aptitude for phthisis, there is but the ape, the cow, and the rabbit, and perhaps a few analogous rodents which can truly be recognised as being susceptible of tuberculosis.* With respect to other animals there is nothing but incertitude, confusion, and refutable allegations. He then proceeds to give a critical summary of the facts which have been adduced in support of the notion that tuberculosis appertains to the principal animal species. He treats the question of phthisis and the diseases which resemble it in the lower animals in the following order:—

Quadrumana.—The quadrumana appear to follow immediately after the human family, if, indeed, they do not precede it, with reference to phthisical proclivity. The observations of M. Raynaud place beyond doubt the existence of phthisis in those animals, which succumb so frequently to the ravages of tuberculosis when they live amongst us. Far from attributing the development of phthisis in monkeys to purely physical causes resulting from change of climate, however, M. Villemin believes that it is due rather to contamination transmitted through an atmosphere vitiated by the human crowd.

Ruminants.—Amongst the larger ruminants, the bovine species is remarkably prone to tuberculization. Under this head M. Villemin is careful to describe the means by which the true tubercular pathology may be distinguished from other conditions which produce similar consequences. Whilst admitting that the bovine species affords abundant examples of tuberculization, M. Villemin maintains that sheep and goats are not susceptible of it. Numerous researches, especially amongst sheep, have led him to the conviction that that animal at least is not liable to the disease; and he explains that contrary statements are to be accounted for by mistakes in recognising post-mortem appearances. Here, again, M. Villemin pauses to describe the comparative pathological conditions of tuberculosis and other diseases which produce similar appearances. He dwells particularly on the effects of parasites, and on dilatation of the bronchi.

Solidungula.—M. Villemin finds it difficult to procure trustworthy data as to the existence of tuberculosis amongst the solidungula. Veterinary surgeons believe that it does not exist in that order. The resemblance which exists between the morbid anatomy of tuberculosis and that of glanders, first pointed out by M. Rayer, led to a confusion of statements, which still continues, and the question as to whether the solidungula ever present tuberculous changes independently of glanders remains to be solved. Here, again, M. Villemin warns the observer against the effects of the echinococcus, so common amongst the solidungula, which gives rise to anatomical changes identical with those previously described as appertaining to the ruminants, the post-mortem appearances of which may be mistaken for tubercle. He concludes by stating that at present all things point to the belief that, except the lesions produced by glanders, so analogous to those of tuberculosis, the morbid products, met with in this order, which present tuberculous characters, belong to the class of parasitic diseases.

Pachydermata.—Assuredly, says M. Villemin, if there be one animal more exposed than others to all the conditions which invite the development of tuberculosis, it is the domestic pig. Shut up for the greater part of his life in a dark, contracted, filthy, and ill-ventilated dwelling, it is astonishing that phthisis is not common to his kind. The appearance of tubercle in the pig is evidently of parasitic origin, whilst most observations have established the co-existence of measles. The same caution is given here, as in the former instances, to avoid mistakes in the recognition of post-mortem appearances.

Carnivora.—After weighing the evidence of some rare cases of tuberculosis recorded as having occurred in the carnivora (tiger, lion, fox, dog, and cat), M. Villemin concludes:—With the data which we possess, it is impossible to determine whether or not tuberculosis can be developed in the carnivora.

Rodents.—The assertions respecting the question of phthisis in rodents are extremely contradictory. On the one hand, it is affirmed that this disease is extremely frequent in this class of animals, particularly in the domestic rabbit; whilst, on the other, it is said to be very rare. According to M. Villemin's researches, both by post-mortem examinations and by questioning physiologists who have experimented a good deal upon rabbits, it is probable that tuberculosis is rare in this class of animals. It is probable, however, that it may be developed naturally in the rabbit, the guinea-pig, and the squirrel; whilst, according to M. Rayer, it is not met with in the rat (*i.e.*, the surmulot, or brown rat, *Mus decumanus*).

Histologically considered, the tubercle produced in the rabbit artificially, that is by inoculation, resembles completely, or with very trifling differences, the tubercle met with in man. The outset and progress of the process are the same in both, and may be briefly summed up as follows:—Tumefaction and proliferation of the cells of the connective tissue, ending in the formation of a little grey, transparent, hard nodule, constituted of small elementary particles analogous to those of lymph. These rapidly undergo retrograde metamorphosis; softening of these little centres, which are frequently aggregated in large numbers, gives rise to cavities and calcareous incrustations.

Birds.—After considering the subject at some length, M. Villemin concludes that there is nothing more problematical than the existence of tuberculosis in birds, and he believes that the positive statements which have been made respecting it have arisen from a misapprehension of certain morbid products, which, with some of the characters of it, were nevertheless not tubercle.

Reptiles, Fishes, Molluscs, Insects.—M. Villemin suggests the same reply to those who state that they have met with tubercle in any of these classes of animals, namely, that many substances which look like it are not necessarily tubercle; and he quotes a case observed by M. Chaussat, who, whilst examining some snails, discovered substances upon the surface of their lungs, one of which, on a close microscopic examination, proved not to be tubercle, but an animal moving within a cyst.

M. Villemin concludes his review of the question of tuberculosis in the lower animals with the following summary. He maintains that this review establishes his doctrine of the previous year, namely, that tuberculosis is a specific affection, closely analogous to glanders. Like other specific diseases, phthisis meets with but a limited number of animal species capable of reproducing its principle and its manifestations. Beyond the human family, the quadrumana, the cow, and certain of the rodentia, such as the rabbit, the existence of this disease remains more than doubtful. Parasitic diseases are the most prolific cause of the misconceptions which have led to a belief in the universality and generality of tuberculosis,—a misunderstanding which such investigations as those of M. Villemin are calculated to dissipate. This error has been propagated all the more insidiously that parasites are usually produced in living animals in the midst of those conditions which are reputed to be favourable to the development of phthisis. Crowded habitations in low and damp situations, confinement, and the like, are, indeed, the circumstances which favour the multiplication of entozoa. Moreover, those parasites which infest the respiratory organs give rise to functional derangements closely analogous to those produced by tuberculosis. Cough, dyspnœa, emaciation, which are also the consequences of pulmonary parasitic diseases, are so indicative of phthisis that in most cases one is content to interpret them as diagnostic of that disease, and hence the diffusion of error.

From this we can also understand how certain observers have been led to regard parasites as the starting point of tubercular lesions, and also the confusion which has hitherto and still reigns between tuberculosis and many parasitic diseases.

It is to be remembered that, in the study of the entozoic lesions which simulate tuberculosis, microscopic examination may, in many cases, be fruitless. When the parasites have disappeared and, the process having reached the last stage of retrograde metamorphosis, no trace of organization is left, the microscope can throw but little light upon the nature of the lesion. And even at an earlier period the microscope does not always clear up every doubt, for small parasitic tumours often present a close resemblance to true tubercle.

Inoculation, M. Villemin is convinced, will throw light upon many points relative to phthisis. It should be tried on as many animals as possible; but *it can only be expected to succeed in those which are capable of offering a natural manifestation of the disease*,—a law which pervades all specific and virulent diseases.

In this memoir, says M. Villemin in conclusion, we have but planted certain landmarks, destined to guide the researches which are still to be made on this important subject. We propose to complete, by an accumulation of facts, the imperfect outline which we have now sketched.

On the 30th October 1866, a second memoir was presented to the Academy by M. Villemin, on the cause and nature of tuberculosis. Objections, he says, have been raised against his experiments, on the ground that the rabbit is naturally so frequently tuberculous as to constitute but an imperfect proof of his doctrine; it is upon animals alone which are seldom or never phthisical that a satisfactory demonstration of the inoculability of the disease can be made. To these objections M. Villemin replies, that he does not admit the frequency of phthisis in rabbits; that it would have been simply miraculous if, in all his cases, the rabbits which he selected were precisely in the condition naturally which he had anticipated them to be in artificially, and that it is essential to operate upon animals which are naturally susceptible of the disease, otherwise the inoculation would not succeed. Again, he repeats the causes of the popular belief of the universality of phthisis, chiefly arising from mistakes in the recognition of morbid products.

M. Villemin next proceeds to explain his further researches and experiments, whereby he has been enabled to elucidate other points in this important subject. He regrets that he has not had the opportunity of experimenting upon monkeys and cows, whose proneness to tuberculosis points them out as proper subjects for experiment. His memoir is divided as follows:—

1. *Inoculation of the Rabbit from Man.*—Three series of experiments were conducted: On the 17th December, five healthy rabbits were inoculated; on the 15th of January, five more; and on the 16th March two rabbits were inoculated at the *Jardin d'Acclimatation*, the rabbits being provided by M. Geoffrey Saint-Hilaire, director of the garden. We cannot follow M. Villemin through the details of the post-mortem examinations in these and his other experiments; it must suffice to give his summaries. From the above three, and also from his previous, series of experiments, he concludes that the inoculation of tubercle from man to the rabbit is amply and incontestably established. Altogether, M. Villemin has a total of twenty-two inoculated rabbits, of which only two escaped the consequences, and these exceptions he attributes severally to the use of improper matter in the one case, and to the method of the operation in the other.

2. *Inoculation of the Rabbit from the Cow.*—On the 14th February two rabbits were inoculated with tubercular matter derived from a cow that had been sent to the Veterinary College at Alfort. Three days later one of the rabbits was killed by a dog, whilst the other remained in a remarkably vigorous condition. The local effects of the inoculation were slow in appearing, but were ultimately well marked. The animal rapidly pined away; it was seized with diarrhœa about the end of April, and, on the 30th, the day on which it was killed, it was in a state of profound marasmus. Then follow the post-mortem appearances. This case, says M. Villemin, appears to me to be of considerable importance, for it solves a question of some interest, namely, that the calcareous phthisis of the cow is identical with tuberculosis in man. It is remarkable that none of the rabbits inoculated with human tubercle afforded such rapidly and completely general tuberculosis as that inoculated with tubercle from the cow. Is this a chance case, or is such a result to be considered as the consequence of a greater affinity between the organism of the cow and that of the rabbit, than

between the organism of the rabbit and that of man? M. Villemin did not know how long the cow had been dead from which the matter had been taken.

3. *Inoculation of Rabbit from Rabbit.*—One of the objections urged against M. Villemin's views was, that, by using matter taken from the body of man from twenty-four to thirty-six hours after death, he was inoculating with a cadaveric substance, which, in that sense, might be the cause of the subsequent changes. To meet this objection he proceeded thus: On the 30th April, he inoculated two young well-formed rabbits with tubercle taken from the lungs and kidneys of another rabbit which was killed at the moment, indeed its heart was still beating when the matter was taken from it. On the 12th July, another rabbit was inoculated in a similar manner. The post-mortem appearances are recorded, and the commentary on the cases is as follows. The animals presented a somewhat rapid and considerable emaciation. About two days after the operation, emaciation was already observable. In the first two the tubercle developed at the point of inoculation had become very voluminous, somewhat ulcerated, and covered with a scab. No. 2. of this series was the most intense and most rapid case which M. Villemin had known; so rapid was the progress of the disease that, at the end of two months, the animal fell a victim to asphyxia. As a rule, inoculation of rabbit from rabbit is much more rapid in its effects than inoculation of rabbit from man; and it is a question whether this depends upon the freshness of the matter employed, or that the virus varies in intensity in proportion to the greater or less analogy existing between the organisms of the giver and the receiver of it. M. Villemin next points out some minor differences between the progress and symptoms of tubercle in man and the other animals.

4. *Inoculation of the Guinea-Pig from Man.*—From experiments on the guinea-pig, M. Villemin believes that that animal is as susceptible of the tubercular virus as the rabbit, and that the former may be employed with the same advantage as the latter in the investigation of the question.

5. *Inoculation of the Dog from Man.*—Before describing his experiments, M. Villemin asks, are dogs subject to phthisis? and he replies, that so far as his experience goes, or so far as he can learn from others, they are not; inoculation, he says, will be the *experimentum crucis.* Two little dogs, still sucking, were inoculated in the belly, one was killed at the end of three months, the other at the end of four; they presented no organic lesion. A third dog, an adult of medium size, fell ill a month after inoculation, he lost his appetite, grew unhappy and thin; after several ups and downs, he at length became remarkably emaciated, diarrhœa set in, and he was then killed, at the end of three months from the date of operation. The post-mortem appearances are described. Lastly, a fourth dog, an adult, above the middle size, and very vigorous, was inoculated on three occasions, the last being one month before his death. He was killed five months after the first operation. Post-mortem appearances described. It is difficult to draw a just conclusion from these experiments, says M. Villemin; are we to ascribe the lesions in the third dog to the inoculation? I think not. Those of the fourth also require a certain reservation,—an accident having hindered me from making a satisfactory histological examination, although it was sufficient to convince me that they were not of entozoic origin. Other experiments must be made.

6. *Inoculation of the Cat from Man.*—After experimenting upon three cats, M. Villemin remarks: Tuberculosis, then, seems to have the power of attacking the carnivora, but it is less easily induced in them than in rodents, and the dog and cat may be considered to be relatively antagonistic to this disease. According to M. Villemin's experience, it is necessary to repeat the inoculation several times, in the cases of dogs and cats, until the local symptoms are well marked, in order to obtain indubitable results, and from this he infers the rarity, if not the entire absence, of idiopathic phthisis in these animals. He explains that the dogs and cats which he operated upon, were kept in a state of exceptional comfort, having meat at discretion, and uncontrolled liberty.

7. *Inoculation of Sheep from Man.*—Again, M. Villemin proposes to decide by inoculation the question, whether or not the sheep is susceptible of tuberculosis; himself believing that it is not. After recording the operations on

several sheep, and their post-mortem appearances, he says: "These various changes, situated in many organs, would certainly have passed in the sight of many persons for lesions of a tubercular character, especially the caseous substance of the liver and ganglia. I do not know, indeed, whence comes this ganglionic change, but every chronic inflammation, as well as tuberculization, leads to caseous degeneration of these organs. The hepatic nodules had no longer any appreciable structure. As to the pulmonary lesions, they were readily explicable by the presence of strongyli. The small granulations were but hypertrophied bronchial glands. Nevertheless, there was room for doubt; and it might reasonably be asked whether tuberculosis, in exception to the law of M. Louis, was not the direct cause of the change in the ganglia. Inoculation comes to our aid. Two rabbits were inoculated with matter taken from the diseased lymphatic glands, from the deposits in the liver, and even from the granulations of the lung. At the end of two months, these rabbits presented no tubercle. According to these experiments, there is every probability that the sheep is antagonistic to tuberculosis.

8. *Inoculation of Birds from Man.*—A cock and a pigeon presented no appearance of tuberculosis several months after inoculation.

9. *Injection of Tubercular Matter into the Trachea.*—M. Villemin thought it would be interesting to determine whether tuberculosis could be induced by absorption from the respiratory organs. Into an opening made in the tracheas of two rabbits he injected, by means of a syringe, a solution of softened tubercle in water. The results of this experiment were unsatisfactory; and, moreover were vitiated by the wound, which, if the matter came in contact with it, would simply constitute an inoculation.

10. *Inoculation of the Matter taken from the Tubercle developed at the site of an Inoculation.*—One of the most important features, says M. Villemin, that has offered itself to my mind, has been whether, after the manner of other well-known inoculable diseases, the morbid principle of tuberculosis reproduces itself at the seat of inoculation. This fact, so important in general pathology, appears to me to have been completely solved in the following manner. After explaining the experiments on two rabbits, and their post-mortem appearances, he adds: These experiments show, once more, the great analogy between tuberculosis and the best known virulent diseases. The evolution of tubercle at the point of inoculation, the time which elapses between the moment of this inoculation and the appearance of tubercle in the different organs, as well as the multiplication and progressive intensity of the morbid product, induce the belief that the initial tubercle, developed at the point of insertion of the virus, becomes the source of the generalization of the disease, in the same manner as, in syphilis, the chancre constitutes the primary local lesion from which spreads the general affection of the organisms, and the manifestation of the disease in many and distant organs.

This confirms, moreover, says M. Villemin, an idea which has doubtless occurred to many practitioners as well as to my own mind, namely, that the discrete tubercle which appears at the apices of the lungs at the outset of phthisis in man is the gateway of this disease, from which the general effects are not slow to spread. However slight the tubercle may be, however benign the symptoms which accompany it, the danger is always immense. Sooner or later, new manifestations arise, more or less intense and frequent, in other parts of the lungs, and in other organs. Were there but a single tubercle in the first instance, it is a source of infection to the economy, which seldom fails to manifest itself: there lies the true gravity of phthisis.

11. *Period of Incubation of Phthisis.*—Tubercle shows itself in internal organs at a certain distance of time after the inoculation,—a period which is probably susceptible of variation according to circumstances. M. Villemin endeavoured to determine the period about which tubercle appears in the lungs and other viscera; and, according to his experiments, pulmonary tubercle began to appear from the tenth to the twentieth day; and in those cases in which it became general, it appeared in the kidneys and spleen before the twenty-eighth day after the inoculation.

12. *Influence of Tuberculosis upon Parturition and Offspring.*—On this point, which is the last in his memoir, M. Villemin has the following:—The question of inheritance occupies a prominent place in the history of phthisis,—a problem the solution of which is exceedingly complex. Let me draw attention to a source of error which I think should be kept in mind: if tuberculosis is really transmissible in the manner of virulent diseases, it is very possible that the cases of phthisis met with in members of the same family are rather the result of cohabitation than of any other cause. In other respects, there are few physicians who believe in the hereditary transmission of a tubercular germ; most of them recognise only a transmitted susceptibility to contract the disease.

According to M. Villemin's experiments, abortion, or at least the premature death of the offspring, was the common result of tuberculosis produced by inoculation in rabbits and guinea-pigs. The early death of the young, he thinks, is due to the insufficient secretion of milk in the mothers, and to their neglect of their progeny. In the young rabbits which died either on the day of their birth, or within a day or two afterwards, the stomach, except in one case, was found empty,—a probable indication that death had occurred from inanition. *None of them presented tubercle.* Two only reached the age of about five months, and these were always thin and stunted, and never attained more than half the size of others of their own age. They presented an excessive abdominal development, with a dull and ugly coat, and died of a disease foreign to tuberculosis; it was a sort of rachidian meningitis, which at the same time caused the death of many other young rabbits of a healthy litter with which they lived. Poisoning was suspected. *These two rabbits, offspring of phthisical parents, presented no trace of tuberculization.*—*Gazette Hebdomadaire de Médecine et de Chirurgie*, 15th Dec. 1865, 19th and 26th Oct., 9th and 30th Nov., and 7th Dec. 1866.

M. LEBERT ON THE INOCULATION OF TUBERCLE.

A MEMOIR by M. Lebert (*de Breslau*), was read at the séance of 30th October 1866, of the *Académie de Médecine*, touching the inoculation of tubercle. M. Lebert bears out M. Villemin's views. He operated upon rabbits and guinea-pigs by injecting under the skin of the neck, either from half a gramme to a gramme of yellow or grey tubercle, diluted and triturated with distilled water, or the liquid itself obtained from a cavity. Afterwards, tubercle was found in the lungs, liver, spleen, pleura, beneath the pericardium, and throughout the lymphatics of the animals operated upon. Microscopic examination proved the identity of the tubercle with that found in man. M. Lebert infers from his researches that there is a special virus for tubercle, as there is for small-pox, syphilis, and glanders.—*Archives Générales de Médecine.*

SURGERY.

ANEURISM AT BEND OF ARM.

AT the meeting of the Société Impériale de Chirurgie of 21st November, some interesting cases were read and discussed. M. Tillaux reported a case of aneurism at the bend of the arm, the result of a wound during venesection by a *sister of charity.* Two months after the accident the tumour was as large as the head of a newborn child, and in a state of inflammation. After some waiting and various doubts as to the exact nature of the case (between an arterio-venous, and a traumatic aneurism), M. Tillaux tied the humeral close to its origin. Severe secondary hæmorrhage occurred on the seventh day, when the surgeon had to cut into the sac and tie the artery both above and below the wounded point. In the discussion which followed, MM. Dolbeau and Després expressed their opinion that the diagnosis should not have been doubtful, and MM. Dolbeau and Larrey very properly insisted that the treatment should from the first have been incision of the tumour and ligation of the severed ends of the vessel.—*Gazette des Hôpitaux*, 1st *Dec.* 1866.

SUB-PERIOSTEAL RESECTIONS.

M. **Ollier** gave an account of some of his sub-periosteal resections of bones. He showed a plaster cast of the foot of a girl of seventeen, from which he had removed five-sixths of the calcaneum by sub-periosteal resection. He had left the tendo Achillis in contact with the periosteum instead of cutting it, with the result that though the antero-posterior length of the foot is diminished, the heel is lifted from the ground and the toes are pointed in walking as if the bone had not been removed at all.

He then detailed six cases in which he had removed, in like manner, the whole of the first metatarsal bone. In this operation the risk seems to be that the great toe is raised on to the dorsum of the foot by its extensor tendon, unless prevented by its section, or by the careful application of India-rubber bandages.

A case of sub-periosteal resection of the shoulder-joint was then described, but the advantages of his modification of this operation are not evident. M. Demarquay then showed to the Society a girl, æt. 20, whom he had cured of a false joint in the humerus of fifteen months' standing, by resection of the ends of the bone by a chain-saw, boring holes in them and fastening them together by wire. Treatment seemed to have required about five months. The result was good.—*Gazette des Hôpitaux*, 1*st Dec.* 1866.

Part Fifth.

MEDICAL NEWS.

CONVERSAZIONE AT THE ROYAL COLLEGE OF SURGEONS OF EDINBURGH.

The first of their conversaziones for this season was held by the Royal College of Surgeons in their hall, on Friday evening, 18th January, at which a large company, consisting chiefly of gentlemen of the legal and medical professions, attended. Dr Dunsmure, President of the College, occupied the chair, and, in the course of his introductory remarks, intimated that the Fellows proposed to continue the conversaziones, which had been so successful last winter.

Dr Skae then delivered an able and interesting lecture on "The Legal Relations of Insanity," in the course of which he proceeded to inquire into the causes of those discrepancies between medical witnesses in our courts of law in regard to insanity, which have of late years often tended to bring medical testimony into disrepute, both with the legal profession and the public. At the conclusion of the lecture, Dr Smith, President of the Royal College of Physicians, moved a vote of thanks to Dr Skae for his address, and expressed a general concurrence with his arguments. Mr Archibald Broun, Senior Depute-Advocate for Scotland, in seconding the vote of thanks, took occasion, while speaking highly of the able and interesting character of the lecture, to express his dissent from some of the views expressed by Dr Skae. He did not think there was so much confusion between the legal and medical interpretation of terms relating to insanity as Dr Skae seemed to suppose, and defended the existing practice of the law courts in regard to questions relating to insanity, both in respect of its civil and criminal relations.

The President having conveyed the thanks of the meeting to Dr Skae, the company adjourned to the tea-room. [Dr Skae's Address will appear in our next Number.]

ROYAL INFIRMARY.

Dr Argyll Robertson has been appointed Assistant-Surgeon to the Ophthalmic Wards of the Royal Infirmary of Edinburgh.

ELECTION OF PROFESSOR OF BOTANY IN UNIVERSITY OF DUBLIN.

We have pleasure in announcing that Dr Alexander Dickson, a distinguished alumnus and graduate in medicine of the University of Edinburgh, whose contributions to botanical science are highly esteemed among the botanists of Europe, was, on the 22d December last, appointed Professor of Botany in the University of Dublin by the unanimous vote of the Provost and Senior Fellows of Trinity College.

DEATH OF DR BRINTON.

We regret to record the loss which the profession has sustained by the death of Dr William Brinton, F.R.S., which took place at his residence in London on the 19th January. Dr Brinton's researches on the physiology and diseases of the organs of digestion had gained for him a distinguished place among British physicians. The cause of death is stated to have been disease of the kidneys.

VARIETIES.

CURIOUS CASE OF HERMAPHRODITISM IN 1654.

The Scherreff court of the Sreffe-domes of Elgin and Nairne, holdin within the Tolbuith of Elgin, The third day of Novr. 1654 zeirs, Be Mr Johne Hay, Sreff-deputt,—

Anent the Sumons raisit and execute before the said Sreff and his Deputtis, at the Instance of William Miller, in flemintoune, Againis Isobell M'Kewin, in Raitt, mentioning That quhairupon the —— day of Marche 1653 zeirs, the said William wes Legallie and orderlie maried to the said Isobell M'Kewin be Mr David Dunbar minister at Nairne, within the Kirk thairof, be the quhilk legal marriage the said Wm. did hope for and expect to have Injoyed and fulfilled to him the benefeitt and dewtie of the mariage-bed. But the said Isobell being borne ane hermaphrodite, haiffing members of both sexes, zit not able in any of thame, and speciallie in the females pairt, Thairby the said Wm. is altogether frustrat and dissapoynted of his hopes: And anent the charge given to the said Isobell, at the Instance of the said compleiner, qrby she was lawfullie summont to have comperit before the said Sreffe and his deputtis, To have heard and sein the premisses verified and prowin, and sua the said mariage to be Legallie dissolvit and declairit null, to the effect the said complenar may have freidome and Libertie to be married to ane uther woman, as it shall pleis god to offer the occasione, as the said Sumons, dewlie execute and Indorsit, beirs. The said complenar compeirand personallie, and with him Mr James Annand, his pror.; and the said Isobell M'Kewin, lawfullie summont as said is, and oft and diverse tymes callit but not compeirand; the said Sreffe-deputt, haiffing deliberatlie considerit the said sumones, did find the samyne relevant, and did admit it to the complenar his probatione, Lyk as the complenar did sufficientlie and clearlie verifie and approve the whole poynts thairof, be productione of ane testimonial, under the hands of the said Mr David Dunbar, minr at Nairne, and diverse uyrs of the parochein thairof, witnessing that the said persones were Legallie and orderlie married; As also that the said Isobell M'Kewin, haiffing appeared befor ym did confess that god had not (in his providence) sooted her for the use of the Mariage-bed, and yrfor did consent that the said William Miller should seik to be lousit from her and married to ane uther; and be productione of Two uther testimonies; ane subst. be Johne Innes, designit thairby phisitian, and the uther under the hand of Johne Neill, designing himself professor of Phisick, both declairing that the said Isobell is ane creature callit ane hermaphrodite, and unfitt for the mariage-bed; Lykways did verifie the samyne, be the depositione of certaine famous witnesses sworne and admittit, quha did upon aithe depone and deliver that they were within the said Johne Innes his chalmer, the tyme he did tack inspectione of the said Isobell, and that they did sie her have the forme of the Secrett members of both sexes, but naither of ym compleit for the use of the Mariage-bed; and yt hir papes wer verilie lyk the papes of ane Man.—And thairfor the said Sreffe-deputt, finding the said Sumones cleirlie verified and prowin in manner forsaid, did dissolve the said mariage, maid betwix the said Wm. Miller and Isobell M'Kewin, and did decerne and declair the samyne to be null and void, and that the said complenar is frie to be married to ane uyr woman qn god sall offer the occasione; for the causses and reasones above written, fullie and sufficientlie verified as said is; qrupon the said compleinar, and with him his said pror., requirit and tuik act.—*Extracted from the Sheriff-Court Book of Elgin and Nairne, of date 3d November* 1654.

INOCULATION INTRODUCED INTO FRANCE BY VOLTAIRE.

EVERYBODY knows by whom and in what year inoculation as a preventive of small-pox was introduced into England. How many know by whom and in what year the same novelty in medicine was made known to France? For the first time, we believe, Mr Espinasse, in his brilliant "Life and Times of Voltaire" (of which the first volume has just appeared), informs us that inoculation was not known in France till long after 1718, when Lady Mary Wortley Montague had introduced it into England; and that it was no less a man than Voltaire himself, who, writing from England during his celebrated sojourn, communicated to his countrymen the new discovery which was afterwards to culminate in the grand innovation of Jenner. Strange, that the two greatest countries of Europe should owe their first acquaintance with one of the most pregnant contributions to the prevention of disease, not to the professed votaries of the healing art, but to two of their most brilliant lights of literature.—*Medical Annotations in Lancet.*

PUBLICATIONS RECEIVED.

Atlas of Portraits of Diseases of the Skin, issued by the New Sydenham Society. Sixth Fasciculus.

Braithwaite's Retrospect of Medicine. July to December 1866.

Chambers,—The Indigestions or Diseases of the Digestive Organs functionally treated. By Thomas King Chambers. London, 1867.

Flint,—Principles and Practice of Medicine. By Austin Flint, M.D. Second Edition. Philadelphia, 1867.

Fagge,—Hebra on Diseases of the Skin. Vol. I. Translated and Edited by C. Hilton Fagge, M.D., etc. New Sydenham Society. London, 1866.

Girdwood,—Venous Saline Injections in Asiatic Cholera. By Gilb. F. Girdwood, M.D., etc. London, 1866.

Half-Yearly Abstract of the Medical Sciences. July to Dec. 1866. London.

Hawksley,—Matter and Earth. By Thos. Hawksley, M.D., etc. London, 1867.

Lee,—Homœopathy and Hydropathy. By Edwin Lee, M.D. London, 1866.

Liebig,—The Development of Science among Nations. By Baron Justus Liebig, F.R.S., etc. Edinburgh, 1867.

Transactions of the Pathological Society of London. Vol. XVII.

Thomson,—Prostata-Krankheiten. Autorisirte deutsche Ausgabe. Erlangen, 1867.

PERIODICALS RECEIVED.

Berliner klinische Wochenschrift,—No. 53, 1866; and Nos. 1 and 2, 1867.

Births, Deaths, and Marriages, Monthly Return of, for Dec. 1866.

British and Foreign Medico-Chirurgical Review,—Jan. 1867.

British Medical Journal,—Jan. 5, 12, 19, 26.

Gazette des Hôpitaux,—Nos. 150, 151, 1866; and 1 to 9, 1867.

Gazette Hebdomadaire de Médecine, etc.,—No. 52, 1866; and Nos. 1, 2, and 3, 1867.

Gazette Médicale de Paris,—No. 52, 1866; and Nos. 1, 2, and 3, 1867.

Glasgow Medical Journal,—Jan. 1867.

Journal of Mental Science,—Jan. 1867.

Medical Press and Circular,—Jan. 2, 9, 16, 23.

Medical Times and Gazette,—Jan. 5, 12, 19, 26.

Medizinische Jahrbücher,—No. 6. Vienna, 1866.

Révue de Thérapeutique Médico-Chirurgicale,—Jan. 1 and 15, 1867.

Virchow's Archiv,—Nov. Berlin, 1866.

Wochenblatt der Zeitschrift der Aerzte,—Nos. 44 to 49. Vienna, 1866.

Addendum to Dr Balfour's Paper on Cholera at Slateford.

DR CRUM BROWN has found, since the analysis given at p. 691, that the determination of the nitrates gives the following results: Calculated as nitrate of lime, Mr Veitch's well water contained 20·64 grains per gallon, and the Cross Keys well water 12·35 grains per gallon. The quantity of nitrates in these waters is very remarkable.

Part First.

ORIGINAL COMMUNICATIONS.

Article I.—*Introductory Address delivered at Surgeons' Hall, Edinburgh, at the Opening of the Medical Session, October* 1866. By Patrick Heron Watson, M.D., F.R.S.E., F.R.C.S.E., Lecturer on Surgery, Surgeon to Edinburgh Royal Infirmary and Chalmers' Hospital.

Gentlemen,—I wish the duty which falls to my lot to-day had been confided to other hands, and that to some one of your senior teachers had been intrusted the words of welcome, of advice, of warning, and of encouragement which my remarks are intended to convey to you at this the commencement of another winter's campaign.

It is no want of will on my part which makes me pause on this the very threshold; but it seems to me, as the occasion is one well fitted to arrest the attention, to mould the character, to develop healthy and high-spirited aspirations, it had been better that a wiser head and some more eloquent tongue had been selected for the performance of so momentous a duty.

But if the advantages which grey hairs confer are not mine, if the time since I sat as a student on the benches which you now occupy, and listened to more than one Address introductory to the business of a Winter Session is but a very little time ago, such a circumstance should only bring me nearer to you in my knowledge of your ways and your wants, should only make me more at one with you in heart and feeling, and should surely serve to establish between us such a sympathy of sentiment as may help me to speak more heartily to you, and enable you to listen more patiently to me, as little else than one of yourselves.

First of all, then, my duty is to welcome you all, both juniors and seniors, to your winter's studies; and most sincerely, in the name of my colleagues and myself, do I wish you God-speed in entering upon the duties of this session.

Those of you who have met with us here before will believe, I feel sure, that our welcome is no empty form of words. As our highest ambition is to prove successful teachers in our various departments, so it is our fondest wish that each and all of you may work well, and gain not only class honours and prizes—to these you cannot all attain—but that highest and best reward of all good work, a conscious sense of having done your duty.

> " 'T is not in mortals to command success;
> But you may do more: you may deserve it."

To those of you who come among us for the first time, and scarcely yet know what lies before you, I should wish at once to dissipate any feeling of strangeness which may steal over you—any sense of home sickness which may perchance well nigh make your hearts fail you,—by not only welcoming you among us, and sincerely congratulating you upon the choice you have made of a profession, but also by assuring you, that whatever doubts or fears or difficulties may assail you, or seem to impede your progress at the very commencement, we, your teachers, are most anxious to make your acquaintance, and to direct and advise you, if you wish it; so, pray, be well assured if you do not avail yourselves of our proffered assistance whenever you may need it, the fault lies with you in not confiding in the sincerity of our friendship for you. We have no wish to hold off from you, or to make it seem that there is any greater barrier fixed between us than the inevitable one which a few short years places between men and youths, between teachers and pupils. If difficulties occur in your studies, in the arrangement of your course, in any of the thousand-and-one things which start up as obstacles in the way of smooth onward progress, you may be well assured, if your teachers can be of use to you, that your position as a student is the best introduction to them, and that it will always make you welcome to their best consideration.

While to all of you, willing, working students, we most heartily hold out the hand of friendly welcome, I am sure I only speak the wishes of my colleagues when I say to all mere idlers, or worse than idlers, that we do not wish them amongst us. If they come here without contrition for the past waste and loss of time, without any desire to be something better than before—without the determination to acquire good working habits, and to make up for what in time, in means, in opportunities, they have already squandered, their presence here will not only be prejudicial to themselves, not only a practical deception to their friends, but, worse still, a bad example to their fellow-students, and, as I very well know, calculated to distract many who might otherwise have been diligent in their studies, correct in their behaviour, and (were it not for the habits they have contracted from the example and precept of others) ornaments to a profession, in which any moral deficiency is sure, in the long-run, to meet with its just punishment.

This note of warning reminds me that the present may be no inopportune occasion to admonish the young and inexperienced who come among us for the first time, that they should be careful and reticent in the formation of acquaintances, and, still more so, of friendships.

What I mean when I recommend care and due circumspection in the choice of intimates and friends is, that you should study to form the intimacy and friendship of those, and those only, whose sentiments and characters are pure and unblemished, whose walk and conversation are devoid of offence, whose advice and counsel you can cherish, upon whom personally you can depend, who will encourage in you only what is good and praiseworthy, and who will not hesitate to set their faces against all that is wrong in thought, word, or deed.

Such men there are in every year of student life—perhaps they are not to be found amongst those who appear at first sight the most attractive of your fellow-students. To some their very virtue may seem repulsive, their out-spoken reproof of what is wicked may perhaps seem rude, their chaste conversation may seem prudish; yet such men are deserving of being marked, and their example is worthy of studious imitation and diligent following. These men are safe, sure friends. They "stand like solitary towers in the city of God, and secret passages running deep beneath external nature, they give their thoughts intercourse with higher intelligencies, which strengthens and consoles them, and of which the labourers on the surface do not even dream."[1]

It is a strange perversity of our nature to see in those persons and things verging upon the brink of what is sinful something attractive, for vice has ofttimes a "goodly outside;" and while

"Vice is a monster of so frightful mien
As, to be hated, needs but to be seen:
Yet seen too oft, familiar with her face,
We first endure, then pity, then embrace."

It is therefore all the more essential that those who would live a godly, sober, and righteous life, should lay their account with having to face the multitude of admiring followers who crowd the train of vicious men. They should set before them as their motto the aphorism of Hippocrates, well-known in classic verse—

"Principiis obsta, sero medicina paratur
Cum mala per longas invaluere moras."

Let them learn to turn away their sight and eyes from viewing vanity, even now, to-day, when they may be said to embark upon an untried course. Let them recollect—

"Lives of great men all remind us
We can make our lives sublime,
And, departing, leave behind us
Footprints on the sands of time:
Footprints, that perhaps another
Sailing o'er life's solemn main,
A forlorn and shipwreck'd brother,
Seeing, shall take heart again."

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[1] Longfellow.

I cannot tell what has been the motive which has led to your choosing Medicine as your subject of present study, and the medical art as the profession to which you virtually declare it is your intention to devote your lives; but of this I am confident, that your friends, parents, or guardians, with whose sanction you have come here, and who are, I doubt not, this day full of proud hopes, as well as anxious fears and fond prayers, that you may be kept from evil, have sent you to this school in the confident trust, that in acquiring a knowledge of your profession you will indulge neither idleness nor debauch, but learn to bear yourselves as men loving good, and eschewing what is bad.

This period of your life, this the outset of your professional career, is to you fraught with a large venture. You are sent forth from the parental home, where all the warm affection of loving hearts has cared for your comforts, has shielded you from the cold chilling atmosphere of this selfish, calculating world, has fettered you in the silken bonds of home discipline, whereby self-restraint in the avoidance of evil has scarcely ever needed your serious contemplation. And is all this rich argosy of hope, embarked in strong reliance on your honesty and trustworthiness, to run the risk of wreck and everlasting ruin? is it to go down, perchance for ever, among the darkness of deep waters without one word of solemn warning, without a beacon-light to guide you, without a helping hand held out to assist and direct you?

Shall we, year after year, have the painful experience of youthful depravity, which comes to the knowledge of each one of us, your teachers,—shall we see bright hopes overcast, shall we see the highest expectations dashed to the ground, shall we see young men led away into every manner of vice, and say no word of warning to those who come here to be under our training, and who at the commencement have ventured on the slippery path of ruin, thinking no evil, and fearing no fall?

No doubt, strictly speaking, your teachers are not responsible for anything in your moral conduct or habits. If you attend your lectures regularly, if your presence in the class-room is marked by punctuality and attention, and your visits to the hospital by diligence and interest, we have no more power to make you learn what we teach you, than to compel you to conduct yourselves with propriety when you leave these halls. Ours is no school with collegiate residence, and discipline enforced by tutorial supervision. When you come among us you are presumed to be already men, knowing how to conduct yourselves, and how to study for yourselves.

But, as a painful experience has taught me, in this as in other matters, that things are not always as they should be, I have ventured to raise a warning voice at the commencement of another year of study, in the hope that it may be the means of opening the eyes of some of those who come amongst us for the first time to the fact, that in friendships carelessly formed, in habits thoughtlessly contracted, they may enter into a snare which lands them in the loss of all which they and their friends had ventured to calculate upon.

Another risk to be avoided is that of idling away time in fruitless efforts at study. A caution on this subject may seem hardly necessary to those who have passed their preliminary examination, and who thus, it may be supposed, have earned the character of students already. I have no doubt some of you are *students indeed;* and those of you who have made a good appearance in the preliminary examination, will generally be found carrying the same aptitude for study into your professional course. And it is for the purpose of insuring, as far as possible, such capacity of mind, and such development of the faculties as study is fitted to produce, that preliminary examinations are no longer conducted in the perfunctory manner once in vogue.

I daresay the stringent regulations now in force in regard to the preliminary examinations, and the greatly increased attention paid to the several departments included therein, may appear to students a most unnecessary exercise of authority upon the part of the different licensing bodies.

I know the licensing boards have been blamed by some for the imposition of an extended preliminary test, as a measure framed on very purpose to exclude all but a certain class from the portals of the profession. I know that some have regarded it as a measure which binds grievous burdens upon the shoulders of the present generation, which they themselves would not so much as touch with one of their fingers. But, however much force such statements may have in some special examples, in which cases of hardship have been adduced as originating in these regulations, I am confident the authorities, with whom lies the decision of such matters, have been actuated by no base, no mean, no class considerations, by no desire to exclude one rather than another from the practice of the profession, by no wish to diminish the number of worthy practitioners. Their aim has been to secure for the public, in their legislation for the student of medicine, who is some day to become a practitioner, such competent knowledge and information as shall fit him for commencing the study of medicine, well equipped for the course which lies before him.

This end is obtained in part by the varied nature of the subjects of ordinary English education embraced in the preliminary course, partly by the amount of classical and mathematical knowledge which must be professed.

A sound English education acquired before the commencement of your professional career relieves you of the necessity of abstracting time from your medical studies to gain what you must some day possess if you are to occupy the position of gentlemen in society. You may imagine that I am exaggerating when I speak of ignorance among young medical men of their own language, and of arithmetic. I am sorry, however, to tell you that the want of such ordinary schoolboy training is grievously complained of as characterizing many of the *candidates* who annually come to the Army Medical School. An intimate friend of my own, one of the professors in that school, tells me that, in addition to his own professional subjects of instruction, he has positively to teach the candidates vulgar fractions, the rule of three, and the working of decimals. Such things should not be; but while each licensing board must employ every means in its power to check the admission of young men to the study of the profession who are thus imperfectly educated, the blame of such ignorance should lie, not with the licensing board, but with the schools from which young men in this country are permitted to pass, while they have failed to learn what they were sent there to be taught.

In demanding a due acquaintance with classical literature and mathematics, the intention is to set you upon a mental elevation from which, at a glance, you may be enabled to obtain a wide reaching prospect of the field of science, mental and physical, in which, before the conclusion of your student life, you must plough, sow, and reap, the rich harvest you are designed to gather into your mental barns.

All that knowledge you have to acquire in preliminary study, to fit you for examination, will never make you, in the true sense of the word, learned men,—will never gain for you a position among your fellows as classical students, as proficients in literature, or as mathematicians.

To gain such positions in the world of classics or of letters, you must have trenched the soil far more deeply than is required by any preliminary test at present imposed in this or any other school. Your education, to meet the requirements of the boards, will, I think, *fortunately*, never be of such a kind as to enable you, with even a semblance of justice, to plume yourself on the knowledge of what is equally possessed by those with whom you will come in contact in after life; or, in other words, "to regard your education as valuable, because it enables you to look down upon the man who has it not."

But if your literary studies do not afford you a claim to be considered as learned men, it is not, therefore, a matter of indifference what amount of proficiency you have attained in your preliminary studies, or what amount of attention you have bestowed upon them.

If the mind has been properly engaged in these studies, then undoubtedly you come to the study of the science and art of medicine with the fruits of that preliminary discipline ready to your use. You commence, in other words, the study of medicine with the most important of all acquirements in ready exercise,—I mean the power to educate or teach yourselves. If you have not as yet gained that, then all your preliminary studies have been indeed in vain. If the various faculties of your mind and inward consciousness have not been exercised and duly developed thereby, then you have missed the great aim for which you went to school. Much of the information you there attained is probably for its own sake worthless; it certainly is, if we except the knowledge of the construction of your own language, an acquaintance with the meaning and derivation of the terms employed in medicine, and the art of spelling, writing, and ciphering. At all events, the other subjects of school-training leave a very transient impression behind.

But the grand aim of the years and money spent in gaining what is called a competent knowledge of the dead languages and of mathematics is to develop those faculties which must be rendered robust, and possessed in lively exercise if further attainments are to be made, whether in the study of medicine or of any of those other professions which imply head-work.

You are probably aware that a great philosopher, of whom Scotland and its capital must ever be proud, speaks slightingly of the education of medical men, and, alluding more especially to the graduates in medicine of the Scottish Universities, stigmatizes them as *Doctores indocti*. He certainly intended the phrase to be a taunt at the limited amount of preliminary education required in his day to obtain a medical diploma; but I really see no good reason for smarting under the lash; the statement is to all intents a true one. To be a learned man, a lifetime must be expended in the acquirement of that learning; but after all this has been gained, wherein would the student or practitioner be bettered, or humanity relieved? The time expended in becoming a man of recondite learning has been lost, so far as that knowledge which is profitable to the sick and hurt is concerned.

A happy mean has therefore been sought somewhere between the waste of time upon subjects preliminary to medicine, and a too early introduction to that study itself. Too much of the former renders the man little else than a mere student of books, a bookworm, or a visionary; the latter produces a creature but one remove from the village charlatan. The one will be so profoundly speculative as to lose time, his patient's confidence, or even his life in metaphysical fancies; the other will be so grossly material, and abruptly practical, as to be incapable of an idea beyond himself and his own actions.

I am convinced that the great difficulty which obstructs many students at the outset of their career, consists in ignorance of *how to study*. By study, I do not mean to dream over a book, or to spend hour after hour in the fruitless occupation of reading so many pages. It does not consist in accustoming yourself to late hours and strong tea,—sitting,

> "Till on the drowsy page the lights grow dim,
> And doubtful slumber half supplies the theme."

To study implies not only to read, but to think as well; and to think in such wise that you may learn the very essence of what you read. It demands not merely the attention of the eyes, but of the whole faculties of the understanding quickened into lively exercise.

If a lad has been taught to study in real earnest when at school, he should be able to teach himself any new language, ancient or modern, without further extraneous aid than a grammar or a dictionary. If he finds himself incapable of this, I have very grave doubts of his capacity to teach himself anything in medical art or science, or to study a work on natural or physical science, so as to make the knowledge serviceable to him.

I do not deny that some men who have failed in boyhood and youth to acquire the powers of discrimination, continuous attention, and critical reasoning, from the study of the dead languages, may prove good sound practitioners in after years. They have perhaps failed to apprehend the use and advantage of these school occupations, and only with the dexterous aid of tutors and grinders have drudged up to the threshold of the medical schools. But if they wish to succeed now and in the future, they must somehow discover the use of those faculties which have so long lain dormant; they must, when at length placed face to face with facts and theories which possess an important bearing upon their future professional course, make up for lost time, and discover in study a pleasurable exercise of their mental powers which they had previously failed to awaken. They must recollect, however, that they will find themselves embarrassed with the double weight of learning to learn, and of storing up what they now for the first time begin to learn in good earnest. To such we would say,

> "Fall to them, as you find your stomach serves you!
> No profit grows where is no pleasure ta'en;
> In brief, sir, study what you must affect."

Remember this, that "Every man has two educations—that which is given to him; and the other, that which he gives to himself; of the two kinds the latter is by far the most valuable. Indeed, all that is most worthy in a man, he must work out and conquer for himself. It is this that constitutes our real and best nourishment. What we are merely *taught* seldom nourishes the mind like that which we teach ourselves."—"And as a man under God is in all circumstances the master of his own fortune, so he is the maker of his own mind. The Creator has so constituted the human intellect, that it can only grow by its own action: it will certainly and necessarily grow. Every man must therefore educate himself. His books and teacher are but helps; the work is his."

It may seem in the retrospect a long period of apprenticeship through which you have been obliged to pass,—these eight or ten years which you have spent at school in acquiring the art of teaching yourselves,—but if the result has really been attained, it has been time well spent. To most of you, I daresay, your schooldays have been a period of bondage, in which the force of necessity rather than any pleasure or interest in your study has compelled you to work steadily.

I can scarcely suppose you have felt practically persuaded of the force of the homely maxim, which I doubt not has been rendered sufficiently familiar from frequent repetition—"Duty first—Pleasure afterwards."

In schooldays it is rare to find the boy who prefers his books and his study to a holiday, or who can concentrate his attention on his Homer or his Virgil to the entire oblivion of a game of cricket, a fishing expedition, or a paper hunt. Now, however, that you have entered upon your life's study, you are supposed to have put away childish things, and to have disciplined your mind in such fashion, that present duty and duty alone shall take the pre-eminence. If such a state of mind has not yet been reached, the sooner with an *Excelsior* spirit you attain to it the better; for the profession upon which you are about to enter certainly entails a greater—a far greater, amount of self-denial and sacrifice than any other.

If you ever expect any one to intrust his own body, and that of those nearest and dearest to him, to your care, it can only be because you are steadily devoted to what you profess to have made your life's avocation, and because you manifest, by something else than words, your intention of allowing no pleasure or self-gratification to interfere with your devotion to work.

I can believe that such a sudden awakening to the duties and work of life is sometimes a very unpleasant sensation. I daresay we have all met with cases where men have lived the greater part of their lives failing to apprehend that they have been sent into this world for any other purpose than mere sensuous enjoyment, "*Natos consumere frages;*" but who, suddenly deprived of money and friends, have had the painful fact, that they must work if they would live brought home to them in a single day.

But, if the realization of the necessity of work is ofttimes unpleasant, be comforted by this consideration, that the duty itself, honestly undertaken, will, in the very act of its performance, become a source of real pleasure, and may carry in its train honour and renown.

> "Not once or twice in our rough island-story,
> The path of duty was the way to glory:
> He that walks it, only thirsting
> For the right, and learns to deaden
> Love of self, before his journey closes,
> He shall find the stubborn thistle bursting
> Into glossy purples, which out-redden
> All voluptuous garden roses.

Not once or twice in our fair island-story,
The path of duty was the way to glory:
He, that ever following her commands,
On with toil of heart and knees and hands,
Thro' the long gorge to the far light has won
His path upward, and prevail'd,
Shall find the toppling crags of duty scaled
Are close upon the shining table-lands
To which our God himself is moon and sun."

By steady work you will find the sting removed from the primeval curse of labour, and, step by step as you advance, the sweets of active constant employment, mental, physical, or both practically realized. There is no man so happy as the busy man, and none so likely to take offence, to keep up a quarrel, or suffer from a painful self-consciousness, as the idle, or comparatively idle, one. You will never meet with a hypochondriac, except among the ranks of those who have more time on their hands than they can occupy with legitimate work. The *otium cum dignitate* for which some men sigh is usually a second childhood,—a wretched, peevish, puling, period of octogenarian imbecility.

Pray set yourselves to gain good working habits, get into harness as soon as you can, put the collar on, and throw your physical and mental strength in steady continuous strain into your work.

You can scarcely imagine, till you have practically tested it, the power which steady working habits will give you. A very moderate degree of ability will, with steady exercise, do a far larger day's work—month's work—life's work—than the spasmodic efforts of great genius. The former is the slow steady crawl of the tortoise which beat the fleet-footed hare of the fable. Genius and steady working power when concurrent—but are they ever concurrent—must win the race; but when only one talent is given, between genius and working power to make a choice, the latter is the more desirable. But as genius—true genius is a rare gift, the mediocrity of ability bestowed on most men needs all the help which steady conscientious labour can bestow to win the prizes of life.

There is nothing worse than spasmodic effort in your work, unless it be labouring with a strain upon your mental energies which no brain could endure for six months. The latter must speedily eventuate in either a mental break-down, or in forced relaxation from all head-work of any kind. I admit, gladly, that such consequences are rare, overwork being a mistake seldom committed by young men of the present generation. Still examples have occurred, once and again, where richly gifted intellects have given way under the strain of continuous unremitting study; or when physical disease, rapidly pursuing its fatal termination, has quenched for ever the light and pride of some happy home. Scarcely a session passes but we have to mourn the loss from amongst us of some favourite pupil, endeared to us not more by his mental abilities than by his diligence and perseverance in work: but in whose delicately moulded face, where thoughtful care mingled with the winning grace and loveliness of youth, the too bright eye, the spiritual head, the lips that pressed each other with such high resolve and courage of the heart, the slight figure, firm in its bearing and yet so very weak, we could not fail to recognise that the few short years of the earthly span were in his case fast drawing to a close, and that in his earnest walk he was travelling along that way whence he should not return.

"Take them, O Death! and bear away
Whatever thou canst call thine own!
Thine image stamped upon this clay
Doth give thee that—and that alone!

"Take them, O Grave! and let them lie
Folded upon thy narrow shelves
As garments by the soul laid by
And precious only to ourselves!

"Take them, O great Eternity!
Our little life is but a gust
That bends the branches of thy tree,
And trails its blossoms in the dust."

"Sic rosa sic violae primâ moriuntur in herbâ
Candidă nec toto lilia mense nitent."

Again, spasmodic—by which I mean irregular—efforts in study are fraught with evil. For by them the attention is distracted, and the continuity of effect so broken as to render the result quite unequal to the time and energy bestowed. Read and study *much*, as much as you can, by all means, but not *many subjects* or *many books* upon the same subject, or at the same time. And when you profess to have engaged your mental powers in the consideration of one topic, do not let it wander vaguely from one book to another, or from the words of your teacher to something else quite irrelevant.

The physical and mental constituents of our being naturally correspond; and as the physical are more easily apprehended than the mental, I shall illustrate the evil effects of irregular efforts, and the satisfactory results of steady efforts in study, by an example from the homely and better known instance of the power of the physical frame.

As students of surgery, you will soon come to know that, in the reduction of dislocations, your extension efforts intended to overcome muscular resistance must be steady, uniform, and maintained, a fact not always sufficiently brought home to the attention of our pupils, as the following anecdote will testify.

A famous London surgeon, chancing to visit his hospital at an unusual hour, found the resident staff of the establishment busied in ineffectual efforts to reduce a dislocated shoulder. There sat the patient, a brawny, broad-shouldered Hercules, who, spite the pain he suffered, was inclined to ridicule the puny efforts of four young gentlemen, who for twenty minutes had vainly expended all their powers upon him. It was before the days of chloroform; and with all their tugging and hauling there sat the patient unsubdued, and the dislocation unreduced. The surgeon, appealed to, had the patient firmly secured to an iron post in the waiting-room, and directed one of the young gentlemen to lean his dead weight upon the extending apparatus. This arrangement accomplished, the patient ridiculed the idea that in ten minutes the reduction would be effected. He was jocose for the first few minutes. Then he began to fidget. Then, as the sweat rolled in great drops off his whitening face, he anxiously inquired if time was not "up," and before the ten minutes were expended, the dislocated bone returned to its socket under the mere influence of the dead weight wearing out the muscles of the half-fainting man.

The moral of this story is, not to tell you how to reduce dislocations,—with that at present I have nothing to do,—but to indicate the principle upon which you are to set yourself to exhaust the whole circle of the art and science of Medicine. This is to be done by the sheer influence of the dead weight of constant, unvarying application. With this the greatest difficulties will be overcome in a much shorter time, and with less expenditure of power, than by the employment of great but fitful effort.

By this I do not mean to teach you that you are never to employ your reserve energy. There are times when it should be called forth, when every exertion of which you are capable should and must be brought into likely exercise. But to have such power capable of being brought into use, it must not be constantly strained, else when the time comes for its advantageous employment you will find it exhausted through fatigue. No general ever sends his entire force into action, or dashes the whole mass of his battalions against the foe. He keeps what in military language are called his reserves, and success often depends more upon the judgment exercised in keeping them out of sight, and certainly out of action, till the decisive moment arrives, when either a feeble position has to be strengthened, or a routed foe needs to be driven from the field.

Employ your mental forces much in the same way, keep a strong reserve ready and on the alert for any special occasion, but do your daily plodding work with your whole heart certainly, but with such energy alone as is sufficient to overcome the resistance which you find any department of study opposes to your steady effort.

Let the work of each day as it occurs be thoroughly concluded in the course of that day. There is no habit more easily contracted, none more dangerous than delaying till to-morrow the duties of to-day. Sufficient for the day is the evil thereof. So, pray, do not condense the inevitable labours of two days into one, else the back will soon grow feeble under the burden. Especially, avoid the habit of condensing a whole week's work into the Friday and

Saturday evenings, for such a habit will soon make you no better than the old woman rendered proverbial by commencing her week's work at eleven o'clock on the Saturday night. Take time by the forelock, and rather leave your Friday evenings,—certainly your Saturday's,—as free from work and as disencumbered for social enjoyment as you possibly can. The bow must be unbent for a time, else it will lose its elastic tension; and your easy working capacity will likewise largely depend on the judicious alternation of work and relaxation.

When you do work, let it be serious, good work, engaged in as such, with an undivided attention. When it is relaxation, let it be as heartily enjoyed, and the work for the time as completely forgotten as is possible. No other mode of union is possible with these two unmixable quantities. They are like oil and water. When shaken together they may appear to mingle, but as speedily separate again, affording no composite result,—they are oil and water still. There is a good Spanish proverb,—by mixing two good things you have made a bad *third*. Well, work and pleasure are just as incompatible; so take the work, which is to be your life's labour, first; give it the chiefest place, and let the pleasure rather resemble the oil floating like a film upon the surface. Floating there, let it prove a protection against the chilling frosts of a cold selfish world, let it smooth the stormy billows which will buffet and may overwhelm your little craft. Let the social pleasures and the playtime of your existence be like oil poured upon the troubled waters of an active, a useful, and a busy life.

> "All things have rest, why should we toil alone?
> We only toil who are the first of things,
> And make perpetual moan;
> Still from one labour to another thrown;
> Nor ever fold our wings,
> Nor cease from wanderings."

I have little to say as to the arrangement of your course of study which implies any important differences of opinion from those who have wisely framed the regulations of our sister colleges.

You will bear in mind, however, that the boundary within which the subjects of study are comprised is a limited one. What is included is not all that may be,—shall I say, all that should be,—but simply what is barely necessary. What is circumscribed within the line is nearer the minimum than the maximum of study needed to make you good practitioners. But the regulations of the boards do not restrict you to such cramped limits, and the more you can add to such a course without interfering with the efficient pursuit of what is essential, will certainly be of the very greatest importance to you. It will not eventuate satisfactorily for you to neglect any of the subjects demanded for examination. The licensing boards stringently require a competent knowledge of every department of study. They leave no margin for a young man to pick and choose for himself, nor do they admit pre-eminent acquaintance with one subject to excuse ignorance of another. You need not hope that a thorough acquaintance with anatomy will prove any extenuation for a perfunctory knowledge of chemistry, nor suppose that your great practical acquaintance with midwifery will excuse your ignorance of surgery or medicine.

Some of the subjects which you must now study may never require your attention after you have entered on professional life. Nevertheless, till your diploma is obtained, make that subject, as well as every other, a special study, cultivating your interest in it, and devoting all your energy to master it, as if on it, and it alone, depended your future success.

This is specially true of the three departments which you are required to study, both theoretically and practically,—I mean medicine, surgery, and midwifery. You may possibly, in after years, from a special proclivity to one or other of these departments, from the circumstances in which you are placed, or some other cause, have your time fully occupied in the practice of only one department of your profession; but such an anticipation, should it exercise any influence whatever upon your present pursuits, should only incline you the more diligenily to make yourself master of those with which you are least likely to come in contact in your practice in after years.

No department of the healing art can be satisfactorily, I may say safely, studied or engaged in without at least so much knowledge of every other as you can hope to obtain during your student life. And to obtain even so limited an acquaintance with each and all of them as shall satisfactorily influence you in the practice of any one, must engage all your diligence and every effort of your attention.

I know perfectly the wild dreams of a practice which shall consist of nothing but *pure* surgery or of *pure* medicine, and the arguments some have urged in favour of such subdivision of labour, parodied in the well-known lines,—

> "One science only will his genius fit,
> So vast is art, so narrow human wit!"

These are only dreams, from which the man who indulges in them,—unless, indeed, he possess an independent fortune,—will some day awaken to find that, in spurning golden opportunities, he has destroyed the only chance by which the result of his cherished ambition could have been practically realized. Or, after spending the best years of his life in *pure* idleness, after souring his temper, and making himself the laughing-stock of his friends, should his patient endurance be rewarded by the windfall of some coveted appointment, he finds it to be but vanity and vexation of spirit,—the gilded shell, the wasted kernel.

Do not be misled by high sounding titles, but recollect that to obtain, not eminence, but simple mediocrity as a physician or a surgeon, you must have qualified yourself practically for both. If you are ignorant of the one branch of your art, you will inevitably commit errors which will not only make you blush for your own negligence, but may cost your reputation a heavy detraction.

Nice distinctions between physic and surgery are no more understood than is professional etiquette by the world outside, and you will find your names handed from mouth to mouth rather as associated with your mistakes than your successes.

A deformed limb from an overlooked dislocation will scarcely be forgiven in the popular aspect of the question, because you are pleased to call yourself a pure physician, and say, after treating it as a bruise, that the case was not one suited for your proclivities.

A case of so-called gouty rheumatism, and treated with oceans of physic and no end of baths at some fashionable watering place, will scarcely seem to have had fair play when another more surgical gentleman discovers a stricture of the urethra, by treating which the patient immediately recovers.

On the other hand, too *pure* a surgical tendency may inflict an equally irreparable damage upon a patient's frame. It can scarcely be satisfactory to a patient to find, after the surgeon has ineffectually applied the actual cautery to his back or hip, that he speedily recovers under the use of the hot douche and colchicum or iodide of potassium. It is rather too late to discover that a patient is suffering from pulmonary disease of a rapidly fatal kind after you have operated on a painless fistula. It can scarcely be regarded as a triumph of surgery to have operated upon an external aneurism when a much larger and more impendingly fatal one exists within the chest. Illustrations of the evil of exclusive surgery are neither far to seek nor ill to find. A patient labouring under symptoms of stone in the bladder presents himself to a surgeon. He is sounded, and the existence of a calculus ascertained. But the patient is perhaps in a most unfavourable condition for the performance of any operation for its removal, which is to be discovered only by a careful investigation of the state of his general health. He may have untoward symptoms, which are easily overlooked by the surgeon who fixes his attention on the local disease. His tongue may be furred, his pulse quick, his digestive organs deranged, with loss of appetite, and want of sleep. He may suffer from pain in the loins. His urine may be coagulable, and of low specific gravity, and other symptoms may pass unnoticed which indicate the existence of a renal calculus, or of inflammatory affection of the kidneys. If any operation, be it lithotomy or lithotrity, is performed under such circumstances, it nearly certainly proves fatal; whereas, by previous medical treatment and regulation of diet, his general health might have been so improved that the operation might have been performed with a good prospect of success, or the existence of further disease might have been discovered, contra-indicating the propriety of interfering by surgical operation at all.

Whatever scope there may be in the larger cities of the empire for subdivisions of medicine and surgery, the provincial practitioner must be equally equipped in knowledge and skill for the duties of every branch of practice.

Those of you, too, who enter the public services will find that such nominal distinctions have no place. You must be as ready as a surgeon to spend your day after an action in your regimental shambles as to devote the next to the care of sick sinking under the ravages of some camp epidemic. You will not find your surgical tendencies any excuse for neglecting the inspection of food, privies, or dunghills, or for refusing to attend the women and children of the regiment, while a whole college of physicians cannot absolve you from attending upon punishment parades or the marking of a deserter.

I might, without drawing upon the resources of imagination, narrate instances enough to show the folly of the neglect of any department of the medical art. But I will only mention one such example which occurred a few years since in my own personal experience.

One Saturday in the end of summer, I was called away from town, to a part of the country remote from railways, and from which I could not return till the ensuing Monday. I looked forward when I retired to rest that night, to a quiet undisturbed Sunday in the country, beyond the range of patients and door-bells. Next morning, breakfast was scarcely over when an urgent summons called me to one of the lodges to see the wife of a gardener. You may imagine my consternation when I found this worthy woman in labour, and that my assistance was all the more earnestly requested because in former labours she had twice nearly perished from flooding. Had the case been one of surgical hæmorrhage, I should have felt great satisfaction in being able to render assistance; but to one who had only attended three cases of midwifery, and those at a very early period of my student life, the position was a very embarrassing one. As hour after hour wore on, and the labour seemed making little or no progress, you may imagine the frame of mind in which I moralized upon the worse than fatal folly of my early negligence. Fortunately, however, for me, and for my reputation, all went well with both mother and infant. And I need not dwell upon the satisfaction I enjoyed, when a few weeks later I acknowledged a letter of thanks for my kindness and skill received from the grateful parents by forwarding some articles of baby clothing as a small thank-offering suited to the occasion of the baptism of my name-child.

In pursuing your prescribed course of medical study, there are specially three subjects I should wish to recommend to your most constant and persevering attention. These are, Anatomy and Hospital and Dispensary practice.

This is saying nothing else than urging you to lay a sound and wide foundation, and to build your superstructure of choicest materials; for Anatomy is the great foundation upon which your whole success must depend; and as the practice of medicine, surgery, and midwifery is your ultimate aim, the sooner you gain a practical acquaintance with them, the more readily will you be able to undertake the charge of patients on your own account.

It is almost needless for me to tell you, Gentlemen, that without a thorough knowledge of anatomy, no one need ever hope to be an accomplished physician or surgeon. The study of anatomy is, however, a very different matter from any of the other educational exercises in which you have hitherto engaged. In it you will probably begin for the first time to educate your eyes to observe accurately, and to employ your hands in delicate manipulation. Books, and plates, and mere memory, will not suffice to instruct you here. What is of most importance to recollect, what will prove most useful to you in any emergency, must be more indelibly impressed upon your consciousness, than by such indirect means for acquiring knowledge.

If you wish really to know your anatomy, you must know the body, and not the mere descriptions of it listened to in the lecture-room, or even as amplified by the study of manuals, however accurate and minute, and however profusely supplied with elegant woodcuts; your anatomy must be acquired in the dissecting-room, and that with diligence and perseverance. Those of you who can afford the time, and who really desire to distinguish yourselves in after-life as physicians or surgeons, must not be content with the amount of practical anatomy enjoined in your curriculum of study. You must spend much of your leisure time in the dissecting-room; you must familiarize yourselves with every tissue and every region in the body; and you must not only dissect the parts, but you must study them in relation to the diseases and injuries to which they are subject, and in connexion with every operation which you may be called on to perform. If your anatomy is anything less than this, you will merely accumulate a mass of facts upon the authority of nothing better than a text-book. The so-called knowledge of anatomy which a youth acquires with no laborious effort, and which he only retains by an exercise of memory, without either digestion or assimilation of the facts themselves, is no true knowledge at all. Such facts in no way nourish his mind, but deposited, as in a storehouse, are as utterly raw and undigested as when he swallowed them. He may be full of facts, but it is the fulness of a bottle, which will pour out only what has been poured in. He may be a convenient depository of other men's labours and thoughts, and he may have an admirable capacity for holding and preserving them, but to call such a man an anatomist is a misuse of terms, and to trust men to his superintendence and care on the strength of such information is a misuse of humanity. Costly apparatus, splendid cabinets, magnificent plates, abundance of material, lectures innumerable, and daily tutorial supervision, will never make you anatomists. These means to an end will only encumber the path, while the end is forgotten or hidden from view by the very embarrassment of present riches. Many a student with but a single text-book, and that perhaps an indifferent one, but who has spent his days in diligent dissection, observation, and comparison, possesses in the long-run far more real knowledge of the subject than others who, with every means at command which money could provide, have rested content with reading from the dissections of others, and looking at plates and woodcuts, instead of working with knife and forceps on their own account.

Do not permit yourself to neglect your anatomy because you find men who have attained great professional success as physicians and surgeons, and either knew little of it at any period of their career, or have forgotten what they once had acquired. Still less credit the assertion that too intimate a knowledge of anatomy embarrasses the operator, and unnerves him for hazardous ventures. The testimony of all our really great surgical authorities is unanimously opposed to such an opinion. "Above all," says John Bell (one of the most shrewd and observant surgeons of his day), "improve yourselves in anatomy. It was by their knowledge of anatomy that Paré (and some others whom he mentions) were distinguished among hundreds of other surgeons in the camp. It was their own intense diligence, and above all their acquaintance with anatomy, the very basis of our science, which made them the first surgeons of the chief cities of England, Holland, and France, the best authors in their own day, and the highest authorities in ours."—"If a surgeon," adds the same author, "ignorant of the facts of the human body, should be called to perform even an established and regular operation, which he has often seen performed, how must he tremble at the thoughts of what he has to do! Faltering and disconcerted! Hesitating at every step! Acting only as he has seen others act, he is interrupted, startled, perplexed, with any new occurrence. He has foreseen nothing, provided for no accident, and every accident alarms him. He moves fearfully and timorously forwards, like a blind man, who walks with an air of confidence on an accustomed road, but when any new object presents itself, or the road is changed, is bewildered and lost."

I would exhort you then, Gentlemen, *now*, when you have the opportunity, to be diligent above all in the prosecution of your practical anatomy. In after years you may have extensive opportunities of studying the practice of medicine and surgery; you may have time to study with care the literature of your profession; but it is very improbable that many of you will have the leisure or even the opportunity of acquiring that knowledge of anatomy which is only to be gained in the dissecting-room.

The great basis on which you may safely rest, the secure foundation on which you may build, the touchstone which will discover truth and evolve error, the loadstar of all future discovery in medicine or surgery by which you may safely and fearlessly steer your bark among the billows of surging error to the haven of your life's voyage, is Anatomy as known and vitalised by Physiology.

If you have such a knowledge of anatomy, it will mingle so insensibly in every mental act which guides your examination of a patient, or directs your operative interference with his frame, that your very finger-points will seem to know their anatomy; your knife will seem to guide its point and edge with so prescient a knowledge of the resistance of the tissues and the relation of parts, here working cautiously with deftest touches, there again with boldest sweeps, that all the hesitation of doubt, all the recklesness of ignorance, all careless expression of boldness, will equally be removed from your manner and your art. You will become daily more confident in yourselves, and thus capable of inspiring confidence in those who confide their patients or themselves to your professional care.

Your Hospital practice and Dispensary work constitute the other grand departments of professional study on which I should desiderate a greater bestowal of time and attention.

In former times when a preliminary apprenticeship to some busy practitioner was enforced by regulation of the colleges, a shorter period of hospital attendance might perhaps suffice. But now when apprenticeships are abolished, at least on this side of the Tweed, it becomes all the more necessary that every practical development of study at the bedside should be most diligently cultivated.

In the arrangement of your studies recommended by the Colleges of Surgeons and Physicians, your hospital attendance is supposed to commence in your second year. I see no reason why you should delay so long. I see no good reason why you should not commence your hospital studies the very first day you register yourself as a medical student. There are no unanswerable arguments I am acquainted with against this arrangement. The hospital hour is not occupied by any class. Familiarity with disease, which is to be your life-long work and occupation, can scarcely make you think little of it, or make you less kindhearted in dealing with suffering humanity. The hospital visit is not more likely to excite prurient thoughts in the first year of study than in the second, and the display of disease and death can hardly produce such emotions, except in a mind in which these thoughts are already harboured and encouraged. To observe the effects of disease and injury can scarcely diminish from the interest felt and the attention bestowed upon the more preliminary subjects, all of which are intended to culminate in the cure of disease and alleviation of suffering.

I would again remind those who would defer attendance upon hospital practice till a later period of study than their first year, that medicine is an art, and while certain sciences minister to its accuracy, and rescue it from mere empiricism,—as an art it must be learned. The eye, the hand, all the senses must be trained for its due cultivation. Just as a mechanic in any trade seeks to gain as early as he can the technicalities of his handicraft, so should you make use of every opportunity to gain a knowledge, not only of the mere manipulative requirements essential to the practice of medicine as well as surgery as studied at the present day, but of how to deal with patients, so as to obtain that mental control of them, without which all your skill and professional knowledge must be nearly worthless.

It has often seemed to me a thing passing strange, while in the humblest trade an apprenticeship of from five to seven years is required before a man is presumed to be fit to make a coat or tinker a kettle on his own account, that four years of study are supposed to perfect a young gentleman in the art of cobbling the stomachs, skins, and bones of his fellow-creatures. Four years, too, spent not in such stitching or cutting, such physicking or potioning, but rather in listening to musty doctrines handed down to the present day more or less directly from the times of Hippocrates. Four years spent in hearing the same pathological processes described by possibly every one of your teachers, and by each one in a totally different fashion. Four years spent in hearing, perhaps, from one that blood-letting is a panacea, from another that it is nothing short of manslaughter; by one that food and drink is the only true physic of their restorative system, and by another that starvation is the omnipotent heal-all suited alike to overfed dowagers and their pug-dogs; by one that the deligation of arteries makes a wound a dunghill, by another that needles and wires are the source of every possible evil that surgical flesh is heir to.

It seems to me, Gentlemen, that you have fallen on strange times for the study of your profession; one in which the old and treasured traditions of the past are fast melting away like scattered snow-flakes in early summer; one in which the once time-honoured landmarks of the profession are fast disappearing. And while all this whirlwind of conflicting opinion sweeps the face of medical science, the period of reconstruction out of the shattered ruins of the past is only commencing. The materials lie strewn about in vast abundance; there are plenty of young and vigorous builders, but the master mind who shall bring order out of confusion, and re-establish any substantial and permanent system is still awanting. There is but one kind of knowledge which alone stands the shattering blast of passing opinion, and that is personal, individual experience. To obtain your own due share of that experience must therefore be your grand aim, and where but in hospital or dispensary practice can you hope to secure it during your studentage.

The direction in which science seems to open a way in the further progress of the medical art, is to prevent, rather than to cure disease, and to recognise the influence which unaided nature exerts in the cure of disease and injury. This potency of nature encouraged by gentle means, by careful nursing, by appropriate dieting, by a knowledge of the *juvantia* and of the *ledentia* of the sick and injured, and as little as possible of the wholesale amputations and the half-poisonous administration of drugs which found favour in the times of our forefathers will manifestly constitute the scientific medicine and surgery of the future. The remedial agencies which in our days and in those of our children will alone flourish, must be supplementary to nature and not antagonistic to her efforts. Our highest aim must therefore consist in directing and sustaining the powers of nutrition and repair, which, as a *vis insita*, lurks in each particle of the living frame. To watch these powers to see how they may be modified most safely, how most disastrously, must in great measure constitute your chiefest duty in visiting the sick and hurt. Your object will not be served in studying at the bedside, if you are contented to acquire a knowledge of how to perform operations, how to prescribe drugs, or to modify aliments, but as well when to employ and when to withhold such measures. And in learning this, it will not suffice *jurare verba magistri*, if you hope to make any progress in the race for truth, but to learn by the observation of nature on the one hand, and by noting the effects of the procedures of art upon the other.

You will never gain this knowledge by idling in the passages of an hospital, or by crowding the wards of some favourite teacher, still less will you attain it by thronging the operating theatre in curious crowd, when some unusual case is to be exhibited, or some thrice bloody operation is to be performed.

The knowledge which is really valuable, which will make you good practitioners, and not the mere admirers of other men, is only to be acquired by steady observation, day after day at the bedside, where your own eyes, and your own ears, and no mere *dictum* of any teacher, however famous, assures you of the real progress of the patient. I do not wish you to neglect any means of study, and it is certainly as far as can be from my present intention to lead you to regard operations as the *opprobria* of *surgery*, as they have been called. Attend operations by all means; but, in seeing them, abstract as far as you can your attention from what is merely sensational, emotional, or spirit-stirring, and test your powers of observation by means of them, calmly considering, in the quietude of your own thoughts, each step, each act from the beginning to the conclusion of the performance. Unless you could do the same thing, unless you appreciate every element in the operative procedure, and have a knowledge of the reason for which it was introduced, you have spent your time in vain. You have in all probability, by your presence on that occasion, only encouraged the bad habit of looking without observation, and of seeing without the exercise of the mental process whereby you are cognizant of what has taken place in your presence.

The result of this vitiated or sensational method of hospital study is apparent at examinations when students frequently come up ignorant of the employment of the simplest appliances; incapable of amputating a finger or a toe; while they can discourse learnedly upon the twenty different methods of amputating at the shoulder-joint, or write a dissertation upon the various procedures in times past and present by which a stone may be removed from the bladder. They are, as a rule, far more ignorant of those matters of practice they are likely any day to meet with than of those with which they may never once be brought in contact during a long lifetime of practical experience.

I do not wish you to be inattentive to what are commonly called the greater matters of your professional study; all I beg of you is not to neglect the more homely examples of medical and surgical practice. In early professional life, if trusted at all, it will probably be in the lesser and not in the greater cases of medical and surgical diseases or injury. Do not, I entreat of you, when that trivial occasion arrives for showing yourselves competent practitioners, have the pain of finding yourselves incapable of doing what some one of *far* less skill and knowledge at once rectifies. Your object, as a young practitioner, will some day be to get over the stream of public want of confidence, which cuts you off as young men from the golden harvest which waves invitingly upon the other side. Most men pass quietly and unobtrusively across the stream by the ordinary stepping stones, and as they wait their turn among the jostling crowd of others anxious like themselves to take the first step, they make sure of their footing when the chance comes, and with greater or less rapidity and agility reach the other bank, passing from one stone to another. Be sure then, Gentlemen, that your education in medicine and surgery fits you to step out boldly and steadily in opinion and treatment when the first case is committed to your care, and let your patron or your patient see he has got the right man as a protégé or a practitioner when he comes to you with something in the scale of surgical sensationalism, as trifling as an ulcerated leg or a simple conjunctivitis.

In prosecuting your hospital studies you should take notes of the cases. In attending upon lectures I have said nothing as to note-taking, because it is advantageous or the contrary according to the idiosyncracy of each individual. But in attending cases you should undoubtedly take careful and accurate notes, however short. This practice you will find not only advantageous as an exercise in teaching you to observe symptoms and to detail them in a consecutive narrative, but also invaluable by indelibly impressing upon your memory the features and characters of these early observed cases, so as to constitute in after life excellent standards of reference. To them at some future period you may betake yourselves, much in the same way that an artist will place before his eye some bit of harmonious grouping, or felicitous colouring which he had copied from a great master when as a pupil he studied in the galleries.

There is still one other matter to which I must allude before I conclude this already too lengthy address, and that is, the necessity for sedulous attention to Dispensary practice. In the various dispensaries with which our city is fully supplied, the senior student is permitted to engage personally in the cure and treatment of the sick poor. He is thus early permitted to come in contact with the realities of disease, in a different sphere of life perhaps, but presenting much the same phase as he will eventually experience in actual practice.

He will then, if not before, discover what practice really is when stripped of the artificial circumstances of hospital residence. Here the study of disease, and of his fellow-man, when in his own humble dwelling laid low on the bed of sickness and death, will form a wholesome contrast in its sad and painful realities with what he had been accustomed to regard it when seen through the medium of clinical instruction. Here he will find himself at last combating disease, which for weeks has enfeebled the father of a family, and made a happy honest household destitute of the necessaries of life; or standing in the gap 'twixt life and death, when the last enemy threatens to lay his remorseless grasp upon the only stay of a widowed mother, he feels as if, holding in his hands the issues of life, the burden and responsibility of professional duty had fallen too suddenly on his youthful heart, and as, if under the continued strain of such anxieties, he could not long maintain his mental equilibrium.

Such a position of conscious responsibility is surely likely to make a right-feeling man repent any negligence he has committed in his earlier student years, and bring home to every one the necessity of due attention to every department of study which increases his practical usefulness.

But there are scenes still more painfully trying which he must encounter, and from which, if he has any heart at all, he cannot stay away. When it is his part, as it will be yours, to stand by the bedside while life's tide slowly ebbs away, and when in the now fast fleeting hours which alone separate the dying man from an eternity upon which he is about to enter without one ray of heavenly hope, with nothing to cheer the gloom of the dark valley, and with a mind panic-stricken by a fearful looking-for of judgment and fiery indignation, he has to listen to heartrending cries for aid when the damp, cold, clammy hand of the dying man clings fast to yours and refuses to let you go; when his eye seems greedily to scan your face, and while you read therein nothing but blank despair, would seem to hold you spell-bound with mute entreaties. Can you, then, forsake him in his extremity? will you leave him to all the blackness, and darkness, and tempest of mind and soul in his mortal struggle with the last enemy? or, will you soothe his dying fears with brandy and opiates, setting his only just-awakened conscience to sleep by drugging his brain? or, will you lead his mind away to other matters, and talking of old friends, of scenes of other days, of choice vintages and of cooling drinks, of a home, of friends and country far away, let him sink in the deep waters with a soul unsaved?

Will you not rather, if you have found for yourself the only balm to a stricken conscience, and a sin-guilty life; if you have laid all your own confidence and hope on Him who calls to all alike to come unto Him and be ye saved; if you have indeed learned savingly of Him who is the alone Physician of Souls,—will you not rather sit on through the dark and lonely hours striving to release that sin-stricken man from the bondage in which the arch-enemy would fain hold him, and in the hour of his sorest and direst necessity enable him rejoicingly to say, like the prodigal of old, I will arise and go unto my Father!

Will it not, in that dread moment of separation between the soul and the body, when all human skill to save life is fruitless, be a source of real joy and rejoicing to feel assured that your efforts have been blessed as a means of saving a soul from death, and of letting in the light and glory of heaven upon what in nature is the darkest, the gloomiest, the dreadest hour of human woe. But, perhaps, you think that in this sorest extremity of a fellow-creature, your duty as a medical man is past, and that such spiritual consolations belong to the function of the minister of religion. So, perhaps, they may; but even when they are so, your word in season can never come amiss. Recollect, however, that there are cases where an opportunity is never given to the clergyman to gain an entrance, or when his formal visit seems to freeze up the whole heart. In such an hour, too, a helping hand may not be near. I have known a clergyman refuse to visit a dying sinner whose soul was plunged in deep despair, because he did not believe in death-bed repentances. I have seen another read the service of his church with indecent haste, and hurry away paralyzed by his dread of infection. Are you, in such an hour, when your patient has fallen among spiritual thieves, to pass by on the other side?

The longer you live, and the more conversant you are with sickness and death, the more convinced, I am certain, you will become that your duties as a medical man will be but half performed if your functions cease with your efforts to heal the bodies of your patients, and you can or will do nothing for the sore diseases of soul which will plunge both soul and body into perdition.

I can imagine no more horrible source of self-accusing condemnation, crushing your own souls to the very verge of everlasting despair, than the conviction lying on your conscience, when laid upon your own death-bed, that this man and that has been carried away to everlasting ruin because your faintheartedness stopped your mouth while he wrestled with sore crying and tears in his last agony. I do not see how you can put it away from you that his spiritual death lies at your door, or how you can expect in your after-life, or when you are in the grasp of the last enemy, to escape the sore gnawings of a reproaching conscience for opportunities thus neglected.

I would have each one of you, therefore, in this the time of health and strength, to make sure of your own saving interest in the shed blood of a once dead and now risen Saviour. I would have you, as you would die in peace, dedicate yourself and your every work in life to His service; and whether you are medical missionaries in name or not, to be so in reality, living lives as well as speaking words which shall make you living epistles of Christ known and read of all men.

If you would see the lovableness and the beauty of this combination of the healer of the body and the physician of souls,—after the great prototype set before you in the Master himself,—I know no more refreshing spectacle than that which is afforded you in the Medical Missionary Dispensary of this city. There, the Christ-like combination of the two duties of caring for the sick body and ministering to the diseased soul, is clearly and practically set forth. And under the directing energy, and with the self-sacrificing spirit to guide you of one whose life, and health, and strength has been devoted to that work, I know nothing which is both so well calculated to be blessed to yourselves, and to make you in turn a blessing to others.

Remember in every study that to all some talent has been intrusted. To one more, to others less, but to all some; and in all for the great purpose of being employed in your life-service. See, then, that it is so occupied by you, and that in none is it wrapped up in the napkin of self-interest, worldly calculation, or the sordid desire to accumulate wretched pelf.

Be certain of this, that to spend and to be spent in the service of Him whose you are, and whom alone you ought continually to serve, is the highest wisdom and the greatest gain.

Entering on your work now,—His servant indeed,—devoting all your energies, all your affections, all your hopes and aspirations to Him and to His glory, however humble may be your lot and function here, in that day when all the follies and vanities, the names and the fortunes of this world will seem as less than nothing, to you will be addressed the welcome words from his lips, "Come, ye blessed of my Father, inherit the kingdom prepared for you from the foundation of the world."

ARTICLE II.—*Remarkable Case of Cervical Abscess in an Infant, caused by the Swallowing of a piece of Glass.* By WILLIAM JEFFREY, M.D., Jedburgh.

I AM induced to publish the following case, not certainly because there are any circumstances connected with it which can be told to my credit (rather, indeed, the reverse), but because I think it is very curious, and possessed of such surgical interest as to make it in some measure worthy of being placed on record.

On the 14th of March last, I was sent for to see D. B., an infant, six months old. As I was absent from home when the message arrived, my partner, Dr Ballantine, went; and on my return, two days afterwards, asked me to go and see the child, saying he thought it was the subject of a croupal attack. I went, and found the infant lying on its mother's knee, with a look of great distress in its face, very feverish, and breathing with some difficulty, respiration being attended with the peculiar noise characteristic of croup; the child swallowed quite well, apparently without any pain; it had no cough, and air was entering the lungs freely enough. I was much struck at this, my first visit, with the suffering the child seemed to endure whenever there was an attempt made to raise it to the sitting posture; and, when so raised, with the peculiarly stiff condition in which the neck was held and carried (like one suffering from caries of the cervical vertebræ). Inquiring into the history of the commencement of the attack, I learned that it had begun somewhat suddenly, with great prostration, feverishness, and slight difficulty of breathing. I therefore supposed that it was one of those cases of croup characterized by the suddenness of its onset, and prescribed accordingly. The child, however, grew gradually worse, the prostration and the difficulty of breathing increasing greatly; and it was the fact of these severe and serious symptoms continuing so long without proving fatal, that first made me suspect there was something unusual about the case. I therefore, at each visit, carefully examined the back of the pharynx (thinking of retro-pharyngeal abscess) and the neck; but it was not until the 27th of March, thirteen days from the commencement of the symptoms, that I noticed anything unusual. On that day, however, I observed a slight swelling in the lower part of the neck. To this I ordered poultices and warm fomentations to be applied. During the next few days, the swelling became gradually larger and larger; the child's difficulty of breathing increasing greatly, so as almost to threaten suffocation; and by the 3d of April, there had formed a regular abscess, pointing about half an inch or so above the sternum, on the left side, between the trachea and the inner edge of the sterno-mastoid muscle. This I opened, and a large quantity (fully half a teacupful) of healthy pus escaped, with great relief to the breathing. After I had opened the abscess, I introduced a probe, but could feel no foreign substance. Between the 3d and the 10th of April, I had the child closely under my eye. The neck was poulticed, and discharged freely, the symptoms continuing much the same as they had been all along, except that the breathing was not quite so difficult. On the 10th of April, I had a note from the child's father, saying that he had taken out of the wound a morsel of gutta-percha, and a piece of glass like a small chip from the mouth of a tumbler, and about the size of a pickle of barley. Here then, I thought, might be an explanation of the whole case; still, as I had not myself seen these articles come out of the wound, and knowing the love country people have for relating anything marvellous, I at the time felt inclined to doubt the possibility of such an extraordinary occurrence, and to suppose that they had just been sticking about the edges of the wound, having probably found their way thither upon some of the poultices. (I think it right to state here, that when I told Dr Ballantine of the above occurrence, he was of opinion that the pieces of glass and gutta-percha had been swallowed.) After this event the child grew more lively, and moved the neck with greater freedom; but still there was feverishness, the breathing was difficult, especially at night, and the abscess continued to discharge very freely. I, as well as Dr Ballantine, frequently examined the sinus with the probe, but never could detect any foreign body. On June 20th, Mr Syme saw the child, and wrote me about him as follows (I give Mr Syme's opinions, by his permission, knowing that they will add very much to the value of the case) :—" On the whole, I am inclined to think that the sinus did proceed from a foreign body; as it is precisely in the situation that I have repeatedly known matter to present itself when proceeding from such a source; as it is not in any of the ordinary situations of purulent collections about the neck; and as the statement of the father is so distinct with regard to the piece of glass. I carefully examined the sinus, and could not detect any remaining foreign body; and therefore conclude that the child will gradually recover, through time and care, from its present state of weakness. If you get any further light with regard to the case, I shall feel much obliged by your acquainting me with it, as the case is very curious."

The sinus, meanwhile, continued to remain open and discharge freely; the discharge, consisting of healthy-looking pus, every now and then ceasing for a short time; and when it did so, always greatly increasing the child's sufferings, especially the difficulty of breathing. He was beginning also to lose flesh very much; indeed, he was in a state of hectic fever, which was gradually consuming his strength. In the middle of June, measles entered the family. On the 13th of July, my little patient, then in a very weak state, became sick and more feverish than he usually was; on the 15th, the eruption of measles appeared; and, on the 17th, he died; having gradually sunk. On the following day, along with Dr Ballantine, I examined the neck. I first introduced a probe from the external opening; and, on cutting it out, found that it had passed along a pretty wide sinus, upwards by the side of the trachea and larynx, for nearly three inches, in an almost vertical direction. I then carefully dissected down to the left side of the œsophagus; and lying, as I afterwards ascertained, in front of the transverse processes of the fourth and fifth cervical vertebræ, I found a piece of glass tube, having the size and shape represented in the accompanying woodcut. It was sharp and ragged at its ends, was lying quite bare, and had its long axis parallel to that of the œsophagus. (This piece of glass was afterwards ascertained to be a portion of the end of the tube of one of "Maw's feeding bottles," from which the child had been sucking.) I then proceeded to make a most careful dissection of the front and sides of the neck. Examining the trachea and œsophagus *in situ*, I found them in every way quite natural. Next, tying these two structures together behind the sternum, I turned them upwards, so as to expose the back part of the œsophagus and pharynx up to the very basilar process. The whole of the muscular tract thus brought into view, was found to be quite entire, and very healthy looking. I then took out the tongue, with the pharynx, œsophagus, and trachea attached. I examined the interior of these, and found them perfectly healthy. Their mucous membranes were free from even the trace of a cicatrix; indeed, they were typically healthy looking.

Of course, the main object of a country practitioner in recording any unusual case he may happen to meet with, should be, not to give his opinions, or attempt to form theories, but to state plain, well-observed *facts*. In bringing this short notice to a close, however, I hope I shall be excused, if, before doing so, I make one or two observations on some points of interest connected with the case.

And, first, As to how the piece of glass entered the child's mouth: It must, I think, have done so in one of two ways: either, having been broken off in the feeding-bottle, it had there remained; and the child having been allowed to drink out of the wide mouth of the bottle, instead of sucking through the conducting medium, the glass had thus entered along with the food; or, having been lying about the room, the child may have got hold of it, and done what most infants are very ready to do with whatever they happen to have in their hands, carried it to its mouth. It is surely curious that the presence of such a large and irritating foreign body in the œsophagus caused no apparent pain or difficulty in swallowing, which may, perhaps, be partly explained by the fact that the child took no solid food. How and when the glass passed out of the œsophagus it is difficult exactly to tell. From the fact, however, that there was no cicatrix visible on any part of the canal, I think that it very likely rapidly penetrated the muscular wall (probably during a single act of deglutition) without giving rise to any great amount of ulcerative action; and this event, in all probability, occurred soon after the glass was swallowed, since it must have coincided, in point of time, with the commencement of the formation of the abscess. The fact that the œsophagus *was* penetrated by such a body, and suffered no permanent injury therefrom, is very interesting; it shows what efforts nature can successfully make (so far successfully at least) to get rid of an offending substance, and enforces the truth of Mr Syme's remark (Principles of Surgery, page 259) that, "Even if a foreign body should be actually detained in the œsophagus, it would be safer to trust its removal to the ulcerative process, which will soon be excited by its pressure on the tissues concerned, than to persevere in fishing for it with any of the ingenious but ineffective contrivances that have been devised for the purpose." The post-mortem, in this case, showed *how safely*, as regards the *œsophagus*, the foreign body had been left to find its way out. Then comes the question, Ought I not to have made a more correct diagnosis at first? and here I really feel that I can hardly blame myself. It is true that, after the child's death, I learned from its mother that the symptoms had come on *very* suddenly; the child having commenced to scream violently shortly after it left her in the morning, and having rapidly fallen into the condition in which I found it at my first visit; and had I inquired more carefully than I did into the history of the commencement of the symptoms, I might possibly have been put on my guard; still, with the exception of the suffering the child seemed to endure when raised to the sitting posture, the peculiar stiff condition of the neck to which I have alluded, and the fact that there was no cough, the symptoms were, for many days, very like those of croup; croup that had been sudden in its onset, and was violent in its nature. Besides, even *had* a correct diagnosis then been made, it would not have materially altered the treatment. Lastly, As to what would most likely have happened had no intercurrent disease carried off the child: It is, I think, just possible that by some sudden movement, or by a violent expiratory effort, the glass might have been dislodged from its position, and come within reach of the probe and forceps; but it is, I conceive, far more likely that, at no distant period, the child would have died from the effects of the hectic fever. It was, in point of fact, actually so dying.

Being in this belief, and before knowing Mr Syme's opinion (which I give at the close), I felt great regret that the piece of glass had not been discovered during the child's life; or, at least, that I had not done what I more than once thought of doing,—laid open the lower part of the sinus, and made a more searching exploration of the parts beneath; thinking that, had I done so, I might have detected the foreign body, and removed it by an operation similar to that of œsophagotomy. Mr Syme, however, in a letter to me, writes as follows (and with this I shall close): speaking of the piece of glass, he says, "If detected, it could hardly have been extracted from the situation where you found it, and where it was apparently occasioning little irritation, since the mucous membrane of the canal had healed."

ARTICLE III.—*On the Comparative Value of the Long Forceps and Turning in Cases of Contracted Pelvic Brim: with Cases.* By ALEXANDER MILNE, M.D.

(*Read before the Obstetrical Society, December* 12, 1866.)

THERE is no little diversity of opinion yet existing among obstetric practitioners with regard to the best line of procedure in those cases of disproportion, where the head, despite of good pains, becomes arrested about the pelvic brim. I allude more particularly to those cases of moderate contraction where the conjugate diameter ranges from two and three-fourths to three and a-fourth inches, excluding those more serious instances of diminution and distortion where craniotomy is inevitable. I say inevitable; for I am afraid we shall not be able to abolish this operation entirely, though limiting it as much as possible, any more than we have been able by means of ergot to bid adieu to the forceps, as was at one time fondly fancied, or to cure all maladies by the knowledge of the circulation and properties of the blood, as the illustrious Harvey—who was also, by the way, a respectable midwife—ardently dreamed.

A conjugate brim diameter of the above size, the head being of the ordinary dimensions, almost always demands the interposition of art, and the question is, what shape or form shall the interference take, in order best to fulfil the object ever before our minds, viz., the accomplishment of delivery in the way most conservative of maternal and fœtal life, least fraught with injury or sacrifice? An important question, too, this undoubtedly is, for in such cases two lives are somewhat in danger—a danger that may, and does at times, eventuate in death. Only two alternatives are presented to us, excluding craniotomy on the one hand, and on the other the induction of premature labour, which, however, implies a previous knowledge of particular pelves, an operation of paramount value where more serious diminution exists. These alternatives are the long forceps and turning.

I. *Forceps.*—I shall take the forceps first. Now, of course, I have got to deal with the long forceps only, or with what has been forcibly and significantly termed by Dr M'Clintock, the "high operation." The short forceps I will praise as loudly as any one; believing it to be a comparatively safe instrument, and that countless lives, both of mothers and children, have been rescued by it since the days of Paul Chamberlen. But can the same praise be awarded to the long instrument? Does the same value attach to it? Can it boast of as many achievements, unmixed with disaster? I fear not. What are its claims? The chief merit claimed for the high forceps operation is, that by it we may occasionally extract a child in cases of narrowed brim, where no other prospect is offered but the "sad and horrible" one of craniotomy! Now, granting that this happy result is obtained in some instances, and conceding further for argument's sake, that turning in these same cases could not have been accomplished, the question remains, Is the result worthy the price, or is the benefit obtained not outweighed by the attendant risks and dangers? Take the mother first, as the more important life, according to obstetric ethics—the life to be protected and spared in preference to the child's, according to the views alike of existing luminaries, and of the "great ones gone." What dangers threaten her, to what hazards is she exposed? In the first place, from the long delay, and delay is inseparable from such cases (the os being long in dilating), she may become exhausted, and suffer from a train of well-known painful symptoms; or the protracted pressure may lead to inflammation, suppuration, and sloughing, fruitful of those unfortunate maladies that afford scope for the ingenuity of the Sims and the Wells, the Browns and the Bozemans; or even rupture of the uterus, with its usually fatal termination, may be induced. Some, nay many, may escape with impunity; but it is undeniable that, in general, delay is pregnant with danger, or, as Sir James Simpson[1] pointed out long ago from the tables of Collins, "the longer any patient remains under labour, the less chance has she of recovering from her confinement." Such are the risks prior to the use of the instrument; in its application mischief threatens her probably even more. For example, the vagina, tumefied by the delay, may be readily torn, or some part of the uterus may be lacerated. Indeed, no one can tell what injury the point of the blade may do as it travels its critical way within the cavity of the womb any more than we can tell what lies at the bottom of a draw-well, or in a valley in the moon. Further, even when the instruments are applied and locked, there is the force required for extraction: often very great, as I have seen,—so great that it were almost a miracle to escape contusion and laceration of parts. And here I would just interpolate that I was very much struck when, at our last meeting, one of the respected Fellows of this Society declared that he was in the habit of keeping the forceps on for no less a period than two hours. Such a practice is, I should think, far from safe; nay, is big with risk, as the records of midwifery show. I witnessed one case where everything seemed to be going fair; the blades were fairly applied, though not without some difficulty, and the child was extracted, though not without a good deal of that article which sage writers justly declare should have no place in midwifery,—I mean force; but on the completion of delivery unequivocal symptoms of rupture were manifested, and the case terminated fatally. Many similar cases are on record, as, for example, cases 11 and 12[2] in the Clinical Midwifery of Dr Robert Lee.

Take now the case of the child. How fares it in general? Is it free from hazard in the "high operation?" Certainly not. In the first place, that very pressure to which I have referred as injurious to the mother, the pressure exerted by the uterine contractions ere the os is sufficiently dilated to admit of art stepping in, may destroy its life. Its brain will not tolerate prolonged compression with impunity, even while within the womb.[1] Of this the stethescope can inform us. Then, if it should escape this Scylla, there is the Charybdis of the forceps. The compression to which the head is exposed during the "high operation" is doubtless severe; and if, as is generally the case, the blades are applied obliquely, then the force is exerted in the antero-posterior axis of the head, which is believed to be the most dangerous direction. Lastly, if it escape with life, it is perhaps not very far-fetched to say that that life may be a sad one, for it is suspected that permanent brain injury—mental damage—may result from the severe compression of the head; and if such be the case, we may thus entail on a family a source of perpetual sorrow and compassion, and on the state a helpless and hapless member, incapable alike of protecting himself or others.[2] Peril to the mother, and risk to the child, then, always attend these long forceps cases; but even greater misfortune is not uncommon, for the operation may be altogether impracticable. Failure may attend the most dexterous efforts to introduce the blades. The non-success may arise from some part of the os being undilated, either from rigidity or pinching between the head and the pelvis, or from a jutting forward of the sacral promontory, or the want of sufficient space, or, as in one singular case that I witnessed, from a tight circular band of uterine tissue (probably an undeveloped or tetanized portion) in the cervical zone. Or again, one blade may pass, and the other fail, or both may enter, yet refuse to lock, being placed on divergent instead of parallel lines. This, of course, involves withdrawal and re-introduction, with delay and renewed risks, and possible failure a second time. Moreover, the instrument may be applied, and yet our efforts to bring down the head prove fruitless and unavailing. The object in these cases of contracted conjugate is to diminish the transverse diameter; but it so happens that the antero-posterior direction of the brim is the one that presents greatest difficulty; and therefore in applying the blades in the transverse or oblique diameters of the pelvis, which are the most accessible and feasible, we augment the transverse diameter of the fœtal head, the very one we require to lessen, and thereby aggravate the task of extraction. I am aware that the increase may be partly in the direction from the vertex to the chin, but it must also be to a certain extent from one parietal bone to the other, and even a small amount may prove an insuperable bar to delivery in such cases. I have now seen two or three where considerable force—probably a good way beyond legitimate or safe limits—failed to procure any palpable descent, and where the instrument had to be withdrawn and abandoned. I have no doubt that some of the Fellows of this Society could tell a similar tale. Such is also the experience of some of the veterans of our art, recorded as lessons to instruct, and beacons to warn, and

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[1] Monthly Journ. Med. Science, Sept. 1847.

[2] Clinical Midwifery, 2d ed. p. 10.

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[1] See Cases 19, 20, 21, 45, 51, 54, etc., Clinical Midwifery of Dr R. Lee, 2d edition.

[2] See Article, by Dr A. Mitchell, on Traumatic Idiocy in this Journal, April 1866.

"Felix quem faciunt aliena pericula cautum."

In proof, I may just point again to the "Clinical Midwifery" of Dr Robert Lee,[1] a work that contains a most heartrending account of the repeated "massacre of innocents," and the perusal of which has helped more strongly to wed me to that other alternative of which I have to speak,—an alternative which, I cannot help thinking had he adopted oftener, might have lessened the horrors of his otherwise instructive pages. At the same time it would doubtless be unfair not to add, that in many of the cases where his children were sacrificed,—"put in bottles," as a famous anatomist was wont to say,—he had, like the reinforcements of an unlucky army, been too long in arriving on the field of action. I might have quoted from various authors, with the view of strengthening, if need be, the foregoing views, but you are all as familiar with them as I can be. You know how, in the case of some of the older writers, such as Hunter and Denman, the dread of injury from instrumental interference impelled them into the opposite and no less dangerous extreme of trusting all to nature; how men like Osborne only came to the rescue when there was hardly a plank of the goodly vessel to save; and I need not repeat to you the striking, but somewhat hackneyed, sentences of Blundell[2] about force. Suffice it, that I give you a line or two from Ramsbotham, all the more valuable as emanating from one who is an advocate of the long forceps. He says[3]—"I must not close my remarks without coinciding in opinion with Professor Davis, that the long forceps, although very powerful and valuable, is at the same time dangerous in its use; that it should not be taken in hand except by those who have acquired some proficiency in obstetric operations; and that it is to be had recourse to more as an experimental measure for superseding the necessity of destroying the child, than as one of the common resources of our art."

It is undeniable, then, that the "high operation" is often beset with great difficulty, always attended with no small risk, and frequently with utter failure; and therefore, considering the molehill of advantage and the mountain of hazard, it need not awaken our surprise that many view it with disfavour and dread, and seek an escape from its evils even in the cruel alternative of craniotomy, which they believe to be more favourable to the mother. I, for one, view it with similar misgiving and dislike, though I prefer betaking myself to what I believe to be a much less perilous and certainly a more agreeable line of action, viz., the operation of turning, and I am abundantly thankful that such an alternative is open to one, and buttressed too by some of the strongest pillars of the profession. Of this I shall now speak.

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[1] Second Edition. See, especially, First Report, embracing eighty cases.

[2] Principles and Practice of Obstetrics, p. 509.

[3] Obstetric Medicine and Surgery, 4th edition, p. 302.

II. *Turning.* — This operation, known to the ancients and practised by the older authors, was especially much in vogue after its advantages had been demonstrated by Paré and his pupils; but in this country at least, the introduction of the forceps eclipsed it somewhat, and it fell into comparative disuse for a lengthened period. In France, happily in my humble estimation, turning did not receive so rude a shock, but stood its firm ground, and was had recourse to by many as before, and it would have been interesting to have known whether a genuine conviction of its superiority, or jealousy of the Englishman, or Hugh Chamberlen's unfortunate failure in Mauriceau's case, contributed most to render the march of the forceps among our Gallic neighbours less triumphant than in Britain. Certainly it could not be from any aversion to mechanical contrivances on the part of a people who have shown the greatest aptitude for such; whose inventive genius has delighted in multiplying instruments alike attractively beautiful and repulsively horrible; who have produced a Dr Guillotine, with his machine for chopping off heads in the twinkling of an eye; a Baudelocque, with his formidable, but sometimes useful cephalotribe; and a Chassaignac, with his valuable écraseur for twisting off uterine polypi, severing even the more massive limb, or crushing away any portion of diseased and doomed structure. But whatever may have been the cause, the fact remains that there was no such truce to turning across the channel as occurred in this country, and we cannot but utter a regret—a useless one, no doubt—that at home there should have been such a pause. At length, however, it was broken, and by a strong voice—a voice that at all times awakens many echoes—by one who in himself is a host in all obstetric questions,—our distinguished president Sir James Simpson, whose powerful advocacy communicated a fresh and unwonted impulse to the operation. Since that time not a few eminent men have accorded it their approval and support, and set themselves earnestly to work to test its value; and a gradual accumulation of favourable statistics will doubtless serve to disarm hostility, to dispel doubt, and to win more general adhesion. Among those who have experienced and acknowledged its worth I may mention Dr Barnes, my very able practical friend Dr Keiller, Dr Graily Hewitt, Dr Wilson of Glasgow, and Dr A. Inglis. Dr M'Clintock must also be classed among the versionists, if we would maintain congruity, for his seventeen cases[1] are very valuable arguments in favour of the operation. Surely nowhere but in the Emerald Isle—land of warm-hearted but strangely misjudging men, where favours seem only to foment disaffection, and good-luck like bad-luck only serves to discourage—surely nowhere else could unvarying success in any cause lead to its condemnation. Even Dr Ramsbotham,[1] able, and of vast experience, though beginning by deprecating it, ends with something that would seem to indicate that he too had been staggered by the facts and arguments of the learned professor, and may perhaps, in the next edition of his beautiful book, favour us with something less feeble—nay, less damnatory—than "faint praise." Churchill too,[2] learned, painstaking, and practical, if he has not gone the length of a qualified assent, has at least not given in an unqualified dissent; and as he is much run upon by our medical youth, I trust we shall soon have him further on the side of truth, seeing that with him error is by no means fleet of limb.

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[1] Obstet. Trans., vol. iv. p. 175.

But, to the merits of the operation. What claims has version to our confidence and support? In the first place, there is a mechanical advantage obtained by it, as pointed out by Baudelocque[3] and Sir James Simpson.[4] This arises from the fact that the diameter of the base of the fœtal head is less than that of the interparietal, and because the transverse diameter, which is the most important in most cases of narrow brim, may be reduced to a greater extent when the base is made to enter the pelvis first. There may be something also in this, that whereas in the case of the forceps, as we have seen, one diameter alone is reduced (which reduction involves increase in another direction); in traction, all diameters are compressed and lessened more or less, and the fœtal head becomes moulded into the particular shape of the pelvic brim. These views are doubted by some and denied by others, but surely a succession of cases where turning succeeded, after the forceps failed, ought to cut away any ground for scepticism or dissent, and win acquiescence and approval. Among the cases I am about to submit, No. VII., I think, looks not unlike a demonstration of their truth; others have been detailed from time to time. If, therefore, it be the case that we have a virtual increase of space—that the head will pass a more limited space in this way—the advantage can hardly be overestimated, the gain is unspeakable, for we may thus, in many cases, avoid the woful work of perforation, and bring home even a living child.

Another advantage pertaining to turning is that it may be performed at an earlier period of the labour. In the case of the forceps, it is alike impracticable and unsafe to apply them unless the os be almost entirely dilated (and the expansion and disappearance of its disc may take a long time indeed); in turning, on the contrary, the hand may be often gradually and safely inserted when the os is only the size of a half-crown. There is here a twofold benefit. On the one hand, and on the side of the mother, we avert the chance of exhaustion and the grave results that spring from prolonged pressure; and, on the other, we avoid the protracted compression of the fœtal head, which may prove fatal, time being equivalent to force. The full benefit of this early action and interference is best obtained, and its value and beauty seen, in those cases where we have become acquainted with narrowed pelves, and are early on the ground watching for the earliest fit and practicable moment. In such instances, if we are present before the escape of the waters especially, and if we employ chloroform, delivery may be effected in a very few minutes, and while the patient is in a pleasant trance. How marked again the contrast between this process and that of the forceps, with its long, and anxious, and painful delay, and probable chain of formidable ills!

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[1] Obstetric Medicine and Surgery, 4th edition, p. 221.

[2] Theory and Practice of Midwifery, 4th edition, p. 313.

[3] Par. 1295, Trans.

[4] Obstet. Mem., vol. i. p. 506; and Prov. and Surg. Journ., Dec. 1847, p. 674.

A third recommendation may be noticed briefly, and that is, that the performance of version demands no other instrument than the soft hand. The essence of the merit here lies not so much in the fact that it can be done in the absence of obstetric instruments, for these ought always to be forthcoming, if absolutely indispensable, but because the avoidance of hard steel is tantamount to the avoidance of probable damage and possible disaster.

Such are the principal advantages of turning in cases of disproportion at the brim; but I must now glance at the other side of the picture, for there are certain demerits, disadvantages, and disabilities with which the operation stands chargeable. As in the case of the forceps, I begin with the mother. What are the alleged risks and dangers incident to her? It is said that her soft structures, the vagina and the uterus, may be bruised or torn by the hand of the operator, and that laceration may also be occasioned by the movements of the fœtus as it revolves round its own axis in the process of turning. Now, surely, the soft hand, endowed with sensation, feeling its way as it cautiously enters the uterus, is far less likely to injure than the hard and sensationless iron. The hand can detect obstacles and recoil, again to surmount them, while the unperceiving metal, even cautiously inserted, may force its destructive way through them. Then, in the gradual revolution of the fœtal body, there cannot surely be much danger, seeing that with care nothing but smooth and convex surfaces need be presented to the uterine wall. Of course, when the head is being dragged through the brim, the uterus is liable to be crushed at any prominent pelvic points; but the hazard here cannot be so great as in the case of the forceps, where the iron lies along the fœtal cranium. This is not mere conjecture, for experience goes to show that the operation is not by any means a dangerous one for the mother; and in proof let me again quote from Ramsbotham. In a foot-note,[1] he observes, "Between the years 1823 and 1852, I delivered nearly two hundred women under transverse presentations. Many of these cases presented a formidable appearance; for in one the membranes had been ruptured a whole week; in another sixty-nine hours; in a third forty-eight," and so on. "In not one of these instances," continues he, "was any injury inflicted on the uterine structure by the hand, so far as I know; nor did any permanent evil arise that could be attributed to the operation." Such is the testimony of an able and experienced accoucheur and author; and could stronger proof be adduced of the safety of turning, so far as the mother is concerned? I think not. I may also add, that in not one of the cases in which I have operated, and these amount to twenty-seven in all, including a few transverse presentations, etc., were there any evil consequences to the mother.

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[1] Obstetric Medicine and Surgery, 4th edition p. 376.

But it is alleged further, that after bringing down the feet we may fail to make the head pass the brim, and thus have to perforate in a more unfavourable position. Now, I think that if a careful gauging of the brim be made beforehand by that natural pelvimeter, the hand, preferable in my opinion to the instruments of Baudelocque and Coutouli; if the sacro-pubic diameter be thus previously measured, the chance of failure is less likely to occur; and even were it unsuccessful, I am not so sure that craniotomy would be much more difficult. Having, however, never failed myself, after getting hold of the feet, I can give no opinion founded on personal experience.

Lastly, it is said that another dilemma may be in store for us; we may in some cases fail to reach the feet. The head may be filling the brim too closely for the hand to pass, or the uterus may be contracting so vigorously as to defy the most skilful and persistent attempts. Now, cases of this kind do occur, but not very often. In the two hundred cases before alluded to, Dr Ramsbotham only failed fourteen times, and these were instances of extreme difficulty. It is wonderful how easily the head may be pushed out of the way, and the hand glide up to the fundus of the womb, unless in those cases where the organ has taken on permanent contraction, and is therefore considerably diminished in size. Besides, incalculable benefit is to be got from chloroform in the way of overcoming the force of the uterine contractions, and inducing relaxation of parts; and I am not so certain whether Dr Ramsbotham might not have lessened even his few failures had he been a greater believer in the wondrous anæsthetic; although no doubt the greater bulk of these cases occurred before the introduction of chloroform.

I come now to the child. What are its prospects in turning in these cases? Those who oppose version found much of their hostility on the dangers to the infant. They say that it is jeopardized in two ways,—two swords as it were hang by a slender thread over its head; if it escape the one, it is almost certain to perish by the other. There is, first, the risk of pressure on the funis; and, secondly, that arising from over-extension of the neck. Now, it would be absurd to deny that these are serious dangers, but yet it is wonderful how often they are, or may be escaped. For instance, with reference to the cord, there is the chance of its getting a safe lodgement in some niche of the pelvis, and the probability of fatal compression will be diminished by effecting the passage of the head as speedily as possible. I need hardly add, that the most persevering efforts should be made if the child when born should not breathe; in one case of suspended animation, I was rewarded with success after twenty-five minutes' exertion.

Then, with reference to the extension of the neck, I have only to say (while admitting freely that mischief may occasionally be inflicted in this way) that it is truly surprising what an amount of trailing, or traction rather, these "little clouds of glory" will bear. The strength of their neck is quite wonderful; and probably, as the head will bear more compression with impunity while the child is in utero, so the neck will tolerate greater extension. At all events, I have pulled pretty strongly for a minute or two, without entailing even ephemeral injury, as evidenced by the liveliness and contentment of the child when it was ushered into the world. Moreover, it should be borne in mind, that, owing to the head when it enters with the base first, adapting itself more correctly to the brim, the points of resistance are diminished, and less force is required to pull it through. I have seen more than one case where the head paused about the brim for hours, despite vigorous pains, and yet when the feet were caught, no more force was required to bring it through than was needed for the other parts of the body.

Such then, are, I think, the principal points connected with turning. It is in my opinion a valuable operation in cases of moderately contracted brim, and my own experience of it leads me to commend it strongly. It is by no means dangerous to the mother as a rule,—I think much less so than the high forceps operation; and it is yet an unsettled point whether it is fraught with more peril to the child. I mean, of course, in the aggregate of cases, for I am contrasting it with the long forceps, with its delay and severe compression, and the results of these and probable failure with craniotomy looming ugly and sombre. But even admitting for argument's sake that the hazard to the child is greater in turning, if it be conceded that the danger to the mother is less, then we are shut up to version as the proper and preferable course, if we subscribe to the obstetric doctrine that the mother's life is the most valuable. Turning, then, should be the rule, and not the exception; and instead of waiting long and weary and, for the patient, critical hours, until the head makes its dubious descent far enough to render the forceps practicable, hours during which the unfortunate woman may, like the Congress of Vienna, as Talleyrand remarked, dance and tumble long enough, without the head advancing any, we should proceed to seek for and bring down the feet, and thus deal most mercifully and conservatively with the dearest life, and probably no less safely with the obstetrically cheaper life of the child. Doubtless more statistics are desiderated to carry conviction with some; these I trust shall be forthcoming. In the meantime I conclude by presenting a few as my instalment to the growing offertory, hoping that they may help in a small way to further the solution of an obstetric question in which is bound up the safety of many mothers and children.

Case I.—On the 7th of May 1863, I was requested by Mr Messer, now Dr Messer, Penicuik, and Dr Ducat, now of London, to see Mrs C., æt. 26, residing in White Horse Close, who had been about thirty hours in labour with her second child. The membranes had been long ruptured, and the os was almost entirely dilated, but a tight band existed higher up. The woman's strength was beginning to fail, and the pains were becoming feeble and less regular; the vagina was swollen, hot, and tender. The head had become arrested for several hours, and the great bulk of it was above the brim. I tried the forceps, but could not get a proper hold owing to the tight band that existed about the junction of the cervix and body of the uterus, as well as the unusual prominence of the sacral promontory, which was the cause of the arrest, and which could not be "humoured," as Smellie phrases it. Dr Alexander Simpson, who had been also sent for, had by this time arrived, and endeavoured to pass the blades, but his well-known dexterity was also baffled. After measuring the pelvis, it was agreed that an attempt should be made to turn, more especially as we were not certain that the child was dead, and in order if possible to avert the disagreeable operation of craniotomy. The woman was deeply anæsthetized, and I introduced my left hand cautiously, and indeed very slowly owing to the condition of the parts, until I came to the head. This receded before a good deal of force, and the hand ascended with great difficulty owing to the want of room and the resumption of strong uterine contractions. In about seven minutes, both feet were grasped and brought down, the head being helped up by external manipulation. Considerable force was required to bring the body through, and the passage of the brim by the head demanded still stronger traction, and occupied about ten minutes. The child was dead, and the left parietal bone was deeply indented. The mother made a good recovery.

Case II.—On the 12th of March 1864, I was requested by Dr Smith, now of East Linton, to see a woman in Trunk's Close, who had been in labour with her first child for upwards of twenty-four hours. The pains had continued pretty strong, but progress was unsatisfactory, and the head had come to a stand-still. The waters had escaped early. I found the os dilated, with the exception of a bit anteriorly that was pinched between the head and the pelvis, but the parts were hot and somewhat dry. Part of the head had entered the pelvis, but more lay above the brim,—in fact, it was chiefly the tumefied soft parts of the cranium that had cleared the upper strait. I waited on a little to see what the pains might accomplish, but it became evident that the head had become arrested. I therefore resolved to employ the long forceps. The woman being chloroformed by Dr Smith, I managed to pass and lock the blades with some difficulty, but extraction was tedious, much force being required. The child lived, but was a good deal bruised; and the mother recovered but slowly, having threatenings of uterine inflammation, requiring careful watching and active treatment for some time.

Case III.—A fortnight ago, 27th October 1866, I was sent for to visit this woman, who was again in labour. She had engaged a dispensary pupil, but he had gone out of town, and she was left in charge of a midwife. Strong pains had continued for about twelve hours, but little good was being done. I found the os dilated, the bag ruptured some time, but the parts cool and moist. The head was lying high up in its old locality. I resolved to turn forthwith. Placing the woman under the spell of the anæsthetic, I reached the feet in a short time, and little force was required to make the head pass the brim. The child cried immediately, and the *mother made a speedy recovery.*

Case IV.—On 11th March 1861, I was in attendance on Mrs H., æt. 28, residing in Cumberland Street, who was in labour with her first child. The pains at an early period were excellent in point of force and regularity, and the os comported itself in an average way, dilating slowly but almost completely in the course of ten or twelve hours. The membranes gave way rather early. The head, however, kept high up, and at the end of thirty hours two-thirds of it or so remained above the brim. At this period, the pains becoming weak, and the head being stationary, I requested the advice and aid of Dr Keiller. That gentleman very kindly consented to come; and on examination it was resolved to deliver with the forceps. Dr Keiller applied the blades most skilfully, notwithstanding the little available space, but considerable force was required to bring the head through, and the child was dead. The mother suffered a good deal of pain, chiefly in front for a few days, and required the frequent introduction of the catheter, but was able to be out of bed in a fortnight. Dr Keiller suggested that turning might be tried if she should again become pregnant.

Case V.—On 13th of February 1863, I was called to the same patient, who was again in labour at the full time. The membranes had been ruptured a few minutes before I entered, the pains were good, but the os was somewhat rigid, and dilated to about the size of a florin. The head could be felt high up, above the brim. I resolved to wait on for a while, to see what the natural powers might accomplish. About eight hours after the commencement of labour, and five hours after the escape of the liquor amnii, little progress being made, and the os, though still undilated, being dilatable, I anæsthetized the lady, and delivered by turning. The operation, even though the parts were somewhat dry, and the uterus contracting firmly, was not very difficult, and only occupied a few minutes. There was a slight hitch as the head passed the brim, confirming what indeed I made out in the process of turning, that the brim was narrowed from a jutting forward of the sacral promontory. The child cried almost immediately, and did well, with the exception that, for a day or two, it moaned a little, as if injured slightly either by the turning or the traction. The mother was out of bed on the seventh day.

Case VI.—On the 12th January 1865, I was again in attendance on Mrs H. I was called at 3 o'clock P.M., and arrived soon after. I found the pains moderate, the os about the size of half-a-crown, soft and dilatable, and the membranes intact. Knowing the kind of pelvis I had to deal with, and that nature could probably at most only drive the head into the brim and impact it there after many hours of exhausting labour, I resolved to turn forthwith. After chloroforming the patient, and that too with her warm consent, nay, at her urgent solicitation, I introduced my left hand, its back well besmeared with adeps, and the arm bared to the elbow, and penetrated the bag high up. The feet were found with remarkable ease, in fact, they almost rushed into my hand, instead of having to be searched for anxiously, and as if with a lighted candle, favoured no doubt by the gush of waters. Moderate traction with the left hand, the right helping up the head, effected delivery in a brief space; in truth, all was over, including the extrusion of the placenta, fifteen minutes after I had made the initial examination. The child cried lustily the moment it was born, and got on well. The mother made a speedy recovery, unvaried by a single trifling drawback.

Case VII.—On the 27th February 1863, I was called to see Mrs M., æt. 44 years, residing in Holyrood Street, who was in labour with her twelfth child. I had been engaged to attend her some time before, and was informed that she had always had difficult labours,—a bit of candour by no means so common as reticence, or evasion, or even imposition. On my arrival, she greeted me as follows: "Now, doctor, just put me well over with chloroform, and haul it away with the irons, for it never comes without them." I found that the membranes had been ruptured for some time. The os was also almost fully dilated, and the soft parts in good condition, neither hot, swollen, nor tender. The pains were regular and strong. The head was not engaged in the brim. I resolved to wait on for a while to note what progress might be effected by good pains, supplemented by voluntary effort, but it became evident that the pains were next to thrown away; for after the lapse of several hours, during which I kept the woman pretty well under chloroform, both from a conviction of duty and of its utility, and in answer to her importunate entreaties, the greater part of the head remained above the brim. In these circumstances, and seeing that the pains were abating, and strength ebbing away, I determined to try the forceps. Procuring the assistance of Mr Walker, medical student, the patient was placed well under the influence of chloroform, which she inhaled with extraordinary avidity and satisfaction, and drawn to the edge of the bed on her back. The blades were applied and locked fairly, though not without some difficulty, but considerable yet cautious force, keeping in mind at the same time the position of the head and the axes of the pelvis, failed to make the head advance. I therefore at once withdrew the blades, introduced my left hand, pushed up the head, seized the feet, and effected delivery in a few minutes. The head passed the brim with less difficulty than I anticipated. The child did not breathe, but the heart pulsated, and artificial respiration restored it in about twelve minutes: it got on well afterwards. The mother made a rapid recovery. This case impressed me more than any other with a sense of the great value of turning. I may add, that the obstetric history of this woman is replete with wonder and interest. She had given birth, as before stated, to twelve children at the full time, but not one of them was expelled naturally. Her earlier confinements occurred at Corstorphine and Portobello, and to both places metropolitan aid was frequently summoned—"the professors," as she said, having delivered her frequently. Several of the children were born dead, and one, at least, I could make out, was craniotomized. She had been urged frequently to submit to the induction of premature labour, but would not consent. I have just to add, that, about twelve months ago, I delivered her of her thirteenth child, at the full period, by turning; the operation being comparatively easy, owing to my being present before the rupture of the membranes. The child lived, and Mrs M. was moving about within a week. I was informed by her, that though she frequently made good recoveries, on two occasions she suffered from what she termed inflammation, and was extensively leeched for the same. On none of the confinements did she recover so rapidly as during the last two, with one exception, when, at the request of Dr Hill, "one of the professors from Edinburgh attended, and," as she said, "brought the child away by the feet."

Case VIII.—On 21st August 1865, and while attending a primiparous case, I was requested by Mr Sullivan, medical student, to see his landlady, Mrs M., residing in Clermiston Buildings, who had been in labour upwards of twenty-four hours with her fifth child. The waters had drained away hours before, the os was well dilated, but great part of the head remained above the brim, notwithstanding a continuance of pretty good pains. Seeing that no progress was being made, and that the woman was becoming restless, and the vagina somewhat dry and hot, and finding, by pushing up the head a little, that there was a likely chance of getting the head through, I resolved to bring down the feet. These were reached with some difficulty, owing to a resumption of powerful uterine contractions, as well as to the force of the abdominal muscles in downbearing, which opposing forces I was unable to check, having unfortunately left my chloroform bottle behind. However, delivery was easily accomplished after getting hold of the feet; and the child cried almost immediately. The mother made an excellent recovery. The cause of the delay, or rather arrest, in this case, was chiefly due to the size of the fœtal head, which measured fourteen inches in circumference, and was very unyielding. The brim was also rather under the average, for previous labours had been very tedious.

I have notes of a few more cases somewhat similar, though not more important; but shall not occupy the time of the Society longer on this occasion.

ARTICLE IV.—*On the Legal Relations of Insanity: The Civil Incapacity and Criminal Responsibility of the Insane.* A Second Address delivered at the Royal College of Surgeons, 18th January 1867. By DAVID SKAE, M.D., F.R.C.S., Physician to the Royal Edinburgh Asylum.

THE principal object of the present address is to inquire into the cause of those discrepancies between medical witnesses in our courts of law in regard to insanity, which, I frankly admit, have brought medical testimony into no little disrepute of late years, and have led to a good deal of distrust in medical evidence, as to insanity, with the legal profession and the public.

I shall also offer to your consideration those alterations in our mode of giving evidence, and in our laws and legal procedures, which, in my opinion, would put an end to those discrepanies between medical witnesses, and re-establish medical evidence in the confidence of the public and of our courts of law.

In this inquiry, I shall have occasion to examine the legal notions of insanity, both as regards *civil incapacity and criminal responsibility;* but as I shall approach those difficult subjects from their medical aspect, and view them as they affect our position and our difficulties as medical witnesses, I trust I shall escape from the suspicion of impertinence in offering any comment or critique upon the *laws* to an audience comprising, I believe, many eminent lawyers.

The greater part of this address, therefore, will, in point of fact, be on the *civil incapacity and criminal responsibility of the insane,* as seen from a medical point of view.

I. The Civil Incapacity of the Insane.

On the 1st of March 1861, I had the honour of delivering an address in this hall on "The Legal Relations of Insanity." The object which I proposed for myself in that address was to lay before you the numerous and serious difficulties which surrounded the subject of insanity, and to point out, as seen from a physician's point of view, how these difficulties arise, and how they may be obviated.

The difficulties I referred to in that address were chiefly those connected with the detection of insanity in its earliest stages, the difficulties involved in determining when a patient should be separated from his family and placed in an asylum, and the delays and difficulties encountered in getting the requisite medical certificates and the sheriff's warrant for his removal to an asylum. I commented, also, on the penalties and actions for damages to which medical men are exposed for sending insane persons to asylums, or for detaining them there.

Before discussing these difficulties, I thought it would simplify the whole subject, and render my remarks on the means of obviating them more intelligible, if I first determined what *insanity really is.*

A great portion of that lecture was accordingly devoted to the purpose of showing that insanity was a *disease,* and, as such, was as much within the province of the physician as any other disease to treat and care for, according to the rules of his art. I endeavoured, further, to prove, what is indeed admitted now by all physicians and physiologists, that it is a *disease of the brain affecting the mind.* And lastly, I arrived at the conclusion that insanity might be defined to be an (apyretic) affection of the brain, in which emotions, passions, or desires are excited by DISEASE, or in which *conceptions* are, in consequence of *disease,* mistaken for acts of *perception* or *memory.*

"This definition," I added, "appears to me to comprise everything. The first part of it defines *moral insanity,* in which the propensities, emotions, and desires are *morbidly* affected; and the second part of it defines *intellectual insanity,* in which there are actual delusions or hallucinations, so long considered the essential features of madness. If I would add anything to this definition, it would be *the loss of self-control* or *self-direction,* which appears to me to be the peculiar characteristic of all forms of insanity; that loss of self-control over the actions, which permits them to be restless, violent, or extravagant; or a loss of control over the passions, which permits them to overrule the judgment and the conscience, and leads to acts of vice, debasement, or violence; a loss of control over the succession of the thoughts, which permits them to be incessant, rapid, and incoherent; a loss of control over the ideas, which precludes the insane from the exercise of comparison and judgment, and leaves them (as Dugald Stewart remarked) like persons in dreams, to mistake the objects of reverie or imagination for realities." In fact, I know of no designation for insanity which more clearly indicates this special defect than the old Scotch name for it,—a man who has *lost his judgment.*

I do not attach any great value or importance to my definition of insanity. I have used it in my lectures for thirty years, and I am gratified to find that it is nearly the same as that given by Dr Bucknill in his admirable essay "On Unsoundness of Mind in relation to Criminal Acts." What I do attach value and importance to is the fact that, along with him, I consider it essential to any definition of insanity, that it must be defined to be A DISEASE,—*a disease of the brain affecting the mind.*

The symptoms, whatever they are, whether delusions, hallucinations, violence, or uncontrolled propensities, must be referable to *disease*, and the result of *disease.* If evidence of disease is required, and if *disease* is regarded as essential to the proof of insanity, then, I contend, that insanity falls as much within the province of the physician to diagnose and to treat as does a case of typhus fever, cholera, or pneumonia.

Unless insanity is *a disease*, a disease of the brain affecting the mind, I do not see what we have to do with it more than other people: but if it is a disease, I maintain that we are bound to know more about it than other people; to make it a special object of study, so as to be able to recognise its incipient and earliest symptoms,—to ward off its attack if possible, and to treat it according to the most approved principles of our art; and, lastly, that, having so studied it,—in all its phases, in its premonitory and incipient symptoms, its gradual or sudden development, its peculiar and varying symptoms, changes, and progress,—we are, or ought to be, more competent to give evidence as to its existence than other persons.

A statement so simple as this, one would think, requires no comment. Surely those who are required to make any subject a matter of special study must know more about it than those who never have studied it, and are not possessed of those subsidiary means of making careful observation which a study of medicine or disease, in all its different aspects, confers.

Yet, against this simple and obvious truth, which I endeavoured to illustrate and enforce in March 1861, the Lord Chancellor of England enunciated a most emphatic denial in the House of Lords in March 1862. He there denounced what he called "the vicious principle of considering insanity as a disease, whereas the law regards it as a *fact*, which can be ascertained by the evidence in like manner as any other *fact.*" This vicious principle, his Lordship stated, had led to the "evil habit," which "had grown into a precedent with judges and juries, of assuming that insanity was a physical disease, and not a subject of moral inquiry." "Was it indispensable that persons should have studied in the schools of medicine, listened to lectures, and walked the hospitals, in order to form a conclusion whether a man was or was not a lunatic?"

"Yet, by the existing law, that was the very absurdity committed." Insanity was a fact "upon which every man of rational understanding was competent to form an opinion."

I do not consider it necessary in this presence to enter into a detailed refutation of the errors contained in these statements made by the grand curator of all English lunatics. It may suffice to say that insanity has been regarded as a disease by all writers of note, legal or medical, for more than two thousand years; that it is universally regarded as the result of some disease of the brain by all modern physiologists and psychologists; and, I think, I might add, by all modern metaphysicians and thinkers of any note. Our distinguished and acute Lord Justice Clerk had more regard to the universal belief in regard to insanity when, in laying down the law to the jury in the trial of George Stephens in Aberdeen in 1865 for murder, he stated that *brain disease was not insanity;* but he added, insanity was a mental *disease,* and brain disease might be the cause of insanity. I accept this distinction most fully,—it exactly illustrates my position. Disease of the lungs, or of the heart, is not a *cough;* but a cough proceeds from some affection of the chest; and although every man may recognise a cough when he hears it as a *fact,* it requires a medical man, skilled in the diagnosis of disease, to say whether that cough proceeds from inflammation, or pleurisy, or bronchitis, or consumption, or heart disease. So it is with the symptoms of insanity. A vicious propensity or habit, or a peculiar opinion or belief, may, or may not, be the result of mental or brain disease; and surely those who have made mental disease a subject of special study, and know all its symptoms from their earliest to their latest stages, are more competent to say whether these symptoms proceed from disease that is insanity, or from other, that is from natural, causes.

Insanity is doubless a *fact,* but it is only a fact in the same sense as any other disease is a fact—that is where its existence is established by the opinions and evidence of those most competent to determine the fact. Common sense, of which the late Lord Chancellor spoke in such high terms of commendation, leads me to the conviction that the existence, and nature, and extent of insanity, like the existence and extent of heart disease, or any other disease, must be most satisfactorily made out by those who have given to those subjects the greatest amount of intelligent and careful observation.

The question, as to whether John Doe and Richard Roe are or are not *insane,* is a matter of *opinion,* until it has been determined by the verdict of a jury or the judgment of a court that they are insane, and then (and then only *legally*) it comes to be a *fact,* as far as their interests are concerned. The peculiar opinions, or, let us call them, delusions of John Doe and Richard Roe may be facts readily proved to a jury; but whether they are proofs of *insanity* or not, is just, in my opinion, a question of the same kind as whether a *cough,* proved to be a *fact,* is a proof of pneumonia—the cough may not be, and after a careful examination by a skilled medical man, it may be found that the cough is a symptom of pleurisy, or of consumption, or of disease of the heart. So in the case of John Doe or Richard Roe, the peculiar opinions or delusions which are proved to be exhibited, may, by the careful examination of a skilled and experienced medical man, be proved either to be mere eccentricities or foibles, or mistakes, or to be the result of *disease of the brain affecting the mind.* After all, it is a matter *of opinion*, and only a matter of fact, when judged to be so; and it is a matter of opinion regarding *disease*, in which a man skilled by study, instruction, and experience is more likely to be correct than a man having none of these advantages.

This statement of the Lord Chancellor, that insanity was a fact, upon which every man of rational understanding was competent to form an opinion, and about which it was absurd to consult medical men, had an ample refutation in a trial for murder which took place at Newcastle shortly before his Lordship made this statement.

In this trial, that of George Clark for the murder of Mark Frater, the case was left entirely to the court and the jury, the prisoner being undefended by counsel or medical witnesses. What were the results? The prisoner defended himself in a manner which could leave no doubt in the mind of any thinking person that he was insane. He talked incoherently. He displayed the existence of insane delusions. It came out in the course of evidence that he believed himself to be Jesus Christ, and that he believed he was alive in Palestine twenty years before he was born, etc. etc. Yet the jury, under the direction of the judge, found him *guilty*, and sentence of death was pronounced by the judge, who, in doing so, approved of the verdict of the jury as being in accordance with the evidence and the laws. Yet the judge himself wrote to the Home Secretary next morning recommending a respite, and, by the interposition of medical opinions and public memorials, the sentence was ultimately commuted.

But let me come to those questions which I have proposed for solution. What are the causes of the noted discrepancies between medical witnesses?

I think I may venture to affirm, that medical witnesses are not more inclined to lean to the side on which they are cited than other witnesses. That seems a natural weakness on the part of almost all witnesses, and one from which lawyers themselves are not exempt. If medical witnesses differ more frequently and more widely than other witnesses, it is due, I think, to the two following causes, viz.:—

1*st*, The questions upon which they are examined are questions of science, requiring previous study and experience, and calling for the exercise of careful and skilled observation and judgment, in order to arrive at a correct conclusion; and not only are the questions they have to answer difficult, but they very often involve *opinions*, about which medical men do differ, just as theologians differ on points of doctrine. Chemists, geologists, and naturalists differ upon many points, and exhibit as much discrepancy in their evidence as doctors, and for exactly the same cause; they are sometimes talking of principles or facts which are not yet settled or defined. They all differ, for example, as to whether a certain substance is a *coal* or *not*, each deriving arguments from his special science to prove that it *is* or is *not*, and all alike differing for the same reason, namely, that *coal* had not before been strictly described or defined, either chemically, geologically, or commercially.

2*d*, This brings me to the second and principal cause of the differences in our courts between medical witnesses. It is one which is very distinctly stated by the late Lord Chancellor himself, in the speech referred to. His Lordship said—"In law there were terms which were annexed to certain ideas and certain conclusions; but when they came to the medical vocabulary, they found different notions and different conclusions attached to the same terms. It frequently happened that medical men used particular phrases and terms known in law, and to which the law attached certain conclusions of fact; while in reality those phrases and terms were used with different meanings by different medical men." Or, to put it in my own words, in law there are certain ideas regarding insanity, and certain terms to denote those ideas, while medical men have different ideas of insanity which they denote by the same terms. Hence all the confusion. The law takes its ideas of insanity from the precedents afforded by the charges of deceased judges or old writers, such as Coke and Blackstone; and whether those ideas, formed several hundred years ago, are right or wrong, true or false, they adhere to them, and to the terms that denote them. The medical men arrive, after a careful study of mental disease, pursued by thousands of medical men as a special subject of observation, up to the present day, at their ideas of insanity, which they designate by certain names. The result which must ensue when the lawyers and the doctors meet in a court of law is obvious—the two parties are talking about different things, very often having the same terms to denote them. How can there be anything but confusion? And the confusion is worse confounded by the lawyers persuading some of the medical men (when it suits their side) to use medical terms in their legal meaning, while the other medical witnesses use them in their medical acceptation. The Chancellor, when he illustrated his statement by a case, seemed to think he had demolished the value of medical testimony. But what was the case? "Half-a-dozen medical gentlemen gave their opinions that there was no lunacy, because they attached to lunacy the existence of delirium or delusion." That is the legal notion of insanity, and should have pleased the Lord Chancellor. "But half-a-dozen others," his Lordship added, "contended that lunacy in the case was shown from the fact of the patient being unable to manage herself and her affairs." The latter is the medical notion of some phases of lunacy and imbecility, and was the practical question at issue here, which was very probably settled the wrong way, because this notion of lunacy did not coincide with the legal one.

The noted Wyndham case, which was the occasion of this speech, and of the Lunacy Regulation Bill brought into the house by the Lord Chancellor, afforded another apt illustration of the same thing. The practical question to solve was obviously, whether Mr Wyndham laboured under such an amount of mental disease as to disqualify him for the management of his own affairs. But how was it put to the jury? The issue which the jury had to try was, whether he was of *unsound mind* or not. Now, what is *unsoundness of mind*, as defined by English judges? They say it is *neither idiocy*, *nor imbecility*, *nor insanity*, but something different from either, such a state as renders a man incapable of managing his own affairs. Lord Chelmsford said, "A verdict founded on *imbecility* or *weakness of mind* only, would be set aside as contrary to law" (*Times*, 12th March 1862).

What are the facts of this case, as seen from a medical point of view? We include every form of mental disease or unsoundness under the terms *idiocy*, *imbecility*, and *insanity*. We do not know of anything that can be called *unsoundness of mind* which cannot be referred to one or other of these three. What was Wyndham's state as understood by our terminology? Why, obviously that he was a person of weak or imbecile mind, and had been so from childhood. But how are we, the medical witnesses, to testify? We are told he must be made out to be of *unsound mind*, and *imbecility* is not *unsoundness of mind*. He was not a lunatic. Therefore there could be no agreement between the lawyers and the doctors about him; and there was none, after a thirty days' warfare. One set of medical men testified that he was only of weak or imbecile mind, and therefore not legally of *unsound* mind; another set tried to make him out to be in a certain view more or worse than a simple imbecile, and so to drag him within the legal limitations of the verdict required.

Now all this appears to me very much the same thing as if it formed a part of the issue sent to a jury in any case to decide whether a *whale* was a *fish* or *not*. The lawyers cite a dozen medical men or naturalists to aid by their wisdom in the decision of the question. The result probably would be much the same as in the Lord Chancellor's case,—half-a-dozen of the witnesses depone that the animal was no fish, but one of the mammalia, and suckled its young;—the other half-dozen might be induced to say it was a fish *in a legal sense*, because it was possessed of fins and a tail, and lived in the sea, and was included in the whale-fishery acts.

Now, I ask, how all this nonsense and confusion is to be got rid of? Only in one way. The parties must be agreed as to what they are talking about, and must use the same terms for the same things. The issue sent to the jury must be clearly fixed and settled by law, and the meaning of the terms defined, so as to leave no room for misapprehension as to what truth is sought after, or what fact is to be determined.

Now, it appears to me to be further obvious, that if the aid of medical experience and advice is to be taken, the terms used for the various forms of mental disease should be the same as those used by medical men, who have derived their knowledge from the observations of nature and facts, and not those derived from the varied and contradictory opinions of judges, who had paid no special attention to the subject. As in any trial for poisoning, the names of the poisons are those used by modern chemists to describe our present chemical agents, and the names used to describe the symptoms and mode of death of the victim are those of modern medical science;—so I contend the terms used to denote the various forms of mental disease should be those in common acceptance among medical men, and not those of Coke and Blackstone.

If medical men are agreed that the terms idiocy and imbecility comprise all mental defects which are congenital or the result of diseases nearly coeval with birth, and that they vary only in degree from complete idiocy up to the smallest appreciable amount of mental incapacity which distinguishes a man of weak mind from a man of sound mind, and that the line of demarcation is nowhere fixed which distinguishes any one link of the graduated scale from another,—then let us, I say, or the law rather, accept this as a matter of fact and observation; let the legal terms mean what ours mean; let the lawyers not invent and define terms for conditions which have no existence in nature, and then ask us to agree in our opinions about them; let them send such an issue as this to a jury, Did the party labour under such a degree of idiocy, or imbecility, or insanity, as rendered him incapable of managing his own affairs? and there would cease to be those differences in medical opinions which depend so much more frequently upon the differences in the meaning of legal and medical terms than in any real differences between the medical witnesses. By using medical terms for mental diseases, I mean only such terms as are in general acceptance among all medical men to denote the great and general distinctions of idiocy, imbecility, and insanity, or their marked varieties,—not those refined distinctions which enter into the writings of some psychologists; or this, at least, I contend for, that whatever terms are permitted to be used, should have the *same legal* and the *same medical meaning*, and denote the *same ideas.* The real question in almost all such cases is a question of *degree*, not one of the existence of a mental condition which can be absolutely limited and defined by any one term.

These remarks will apply equally to the Scottish legal procedure for securing the care of the person and property of one of unsound mind. It can only be done by a process called *cognition*, in which, under a brieve of *idiocy*, *fatuity*, or *furiosity*, the condition of the individual is tried and determined by a jury before the sheriff of the county. Now, in every case of this kind which has been tried in my experience, and I have been engaged in most of them, the sheriff lays down the law to the jury, to the effect that in a brieve of idiocy the party must be proved to be an *absolute idiot;* in one of fatuity, that he must be *absolutely fatuous ;* and so, I presume, of furiosity, that he should be *absolutely furious.* No case, however, has ever been brought before our courts where the party sought to be cognosced was *absolutely idiotic* or *fatuous.* He could either count five, or repeat a verse of the Bible, and therefore the jury invariably, under the direction of the sheriff as to the legal meaning of the *terms,* has brought in a verdict refusing to cognosce the party. In fact, the process is obsolete and useless. There does not exist any person in Scotland who is absolutely idiotic, fatuous, or furious,—or if there are any such, they are in a condition which precludes them from ever being brought into a court of law. So that, at the present moment, we possess virtually no legal means in Scotland by which we can legally appoint any one to take care of the person of an idiot or a lunatic, and all this arises out of the fact that the law will not send the simple issue to a jury to determine what is wanted; and that is simply, whether a person labours under *such a degree of idiocy, imbecility, or insanity,* as to render him incapable to take care of himself and his property.

The laws, as it has been frequently observed, take better care of the property than of the person of lunatics; and we have, I must admit, in Scotland, a very admirable, simple, and cheap system for providing a *curator bonis* for lunatics, which effectually protects their property, and which it would be well for English lunatics to have introduced into England instead of the present expensive process required there to place their property under the jurisdiction of Chancery.

II.—The Criminal Responsibility of the Insane.

I now come to the very important and interesting subject of the Criminal Responsibility of the Insane, which I propose to examine from the same point of view from which I have examined the difficulties surrounding inquiries regarding the civil incapacity of the insane. Proceeding from the stand-point that insanity is a DISEASE *affecting the mind,* and that the terms which are used alike by legal authorities and medical authorities are often used to denote different ideas about it, I proceed to inquire how far these facts may afford a solution of the discrepancies between medical witnesses in criminal trials where insanity is pleaded, and how far they may suggest suitable remedies for the evils complained of.

Much obloquy has been thrown upon medical witnesses, not only for their differences of opinion in criminal trials, but also for their endeavouring to stretch the plea of insanity too far, so as to screen the guilty and responsible from punishment. It has been said that we would exonerate *thieves* on the plea of *kleptomania*, protect *fire-raisers* from the penalties of arson on the plea of *pyromania*, and interpose between the murderer and the last penalties of the law the fallacious and silly excuses of *a low type of organization*, *imbecility*, the *absence of motives*, *irresistible* or *homicidal impulse*, *moral insanity*, and so forth.

Without denying the truth or justice of these allegations, it might be a sufficient defence of medical men, if I were to say that there exists quite as much difference of opinion in the public mind, and in the minds of lawyers, and that the difference and uncertainty of opinion is continually manifested in the actual administration of the law. Criminals are constantly condemned to death in cases where the plea of insanity has been scouted by the court, and as constantly is the sentence afterwards commuted,—often on the recommendation of the very judge who had solemnly told the prisoner, as in the case of Clark, that he was satisfied he was responsible for his actions. Not only are sentences constantly commuted after the condemnation of the prisoner, but no one can have failed to observe that the plea of insanity is accepted by the court in one case, while in other cases of an exactly similar character it is repudiated, and the sentence of the law is carried into execution.

The uncertainty of the law resulting from this state of things is most injurious to the interests of justice, and cannot fail to impair its efficiency as a motive for deterring from crime.

If we ask for the cause of this uncertainty in the administration of the law, and for the conflicting opinions that prevail regarding the Criminal Responsibility of the Insane, we have not far to look for it.

It has been uniformly laid down as the law, by all our judges, that the only test for *responsibility* is the KNOWLEDGE of *right* and *wrong*. No delusion, no amount of imbecility or insanity, will relieve any one from *responsibility* or exculpate him from the crime of murder, if he knew at the time he committed the act that he was doing what was wrong, and that the act exposed him to punishment.

Under the operation of the law, as thus fixed by the successive charges of all our judges, and confirmed by the opinion of the twelve judges, it cannot be doubted that a large number of confessedly insane persons have suffered the last penalty of the law, from the time of Bellingham, who shot the Hon. Spencer Perceval in 1812, down to the present day.

Against this operation of the laws as taking away the lives of persons confessedly insane—diseased in mind, but still preserving enough of the light of reason to tell them that they were wrong, and liable to punishment—a strong feeling has been gradually gaining ground in the public mind, from a conviction that it is inhumane and unjustifiable, as well as inexpedient, to execute such persons; or to put it in the strong words of Sir Edward Coke himself, "the execution of an offender is for example," "but so it is not when a madman is executed; but should be a miserable spectacle, both against law, and of extreme inhumanity and cruelty, and can be no example to others."

An equally strong feeling has been gaining ground in the minds of all medical men who have given attention to the study of insanity, against the present state of the law, not only because they regard it as a piece of inhumanity to execute a person for crime perpetrated under the influence of motives acting on a mind palpably *diseased*, but because they consider the legal criterion of responsibility (KNOWING *right from wrong*) to be entirely defective and erroneous, as applied to the insane.

This conviction on the part of medical witnesses, that the knowledge of right and wrong is not the proper test of the responsibility of the insane, or that the *knowledge* is not in them always accompanied with the *power* to do the right or withhold the wrong, has doubtless been one of the principal reasons why they have exposed themselves to the charge of overstraining their evidence in behalf of the accused.

The question is never put to them, whether they consider the accused party to be responsible or not—that is a question for the jury; they cannot but admit that he knew the effect of his act,—perhaps the unhappy victim of disease committed murder for the avowed purpose of being hung for it; they cannot, therefore, but admit that he knew the consequences of his act,—and that is all that is required to convict him and hang him. They feel, however, that he was undoubtedly under the influence of disease, and that he ought, if possible, to be saved. This feeling leads the witness, under the persuasive and seductive influence of counsel, to stretch a point, as it were, and so bring his evidence into disrespect and distrust. Is he to blame? Not more so, I think, than the jury which convicts the prisoner, and the judge who condemns him, all according to law and legal precedents, and who both, after the trial, along with a sympathizing and indignant public, join the despised medical witness in recommending the condemned criminal to the Royal clemency; and the Home Secretary by-and-by, in obedience to medical and public opinion, steps in to prevent a judicial murder, commutes the sentence to penal servitude for life (for the poor lunatic), or sends him to an asylum; and so perpetuates a system of bungling and imperfect legislation, rendering the laws against crime daily more and more uncertain, and therefore more and more useless for the end for which they were passed.

The advance of public opinion on this subject, and the influence of medical opinion, has not only so far unsettled and rendered uncertain the administration of the law in regard to crimes where insanity is pleaded, but it has to a certain extent affected the modes in which the law is laid down by our judges of late years. At one time we were told that a man must be furious, or as mad as a wild beast, so that he did not know what he was doing, before the plea of insanity would save him. Then we were told if his delusion were such as if it were true, it would have justified the act, such as that he believed he was acting in defence of his own life, it would be sufficient. Now, in a trial for murder which took place in Scotland, that of Alexander Milne for the murder of James Paterson (February 1863), I was happy to observe that the Lord Justice Clerk, although he laid down the acknowledged rule of law, that if the prisoner knew the true nature and quality of his act, and apprehended its effects, he was responsible, added this remarkable admission: "If you are quite satisfied that the prisoner was under insane delusion at the time of the act, you need not inquire whether he knew right and wrong. If the delusion be once established, the law will presume from that, that he did not know right from wrong." A similar admission as to the error contained in the legal test of responsibility was made by an English judge, then Baron Parke, in the trial of Robert Hancock for the murder of his wife in 1855. "So far from adhering to the metaphysical test laid down by the judges, he charged the jury that if they were satisfied that the criminal act was the result of delusion, they must acquit the prisoner." In this case the delusions were such that, if they had been true, the prisoner would undoubtedly have been executed. This I consider a great step in the right direction, as indicating a change in the opinion even of our courts, and it is to be hoped these charges may be acted upon as precedents in future trials.

The progress of public and legal opinion is still further indicated by the Report of the Royal Commission appointed by the House of Lords to Review the Laws regarding Capital Punishments, which recommended that a verdict of murder in the second degree should be accepted from our juries.

It seems, therefore, to be admitted on all hands, by the public, by the legal as well as by the medical profession, and by the legislature, that the laws regarding the criminal responsibility of the insane require revision and amendment.

The cause of this, as I have said, is that the legal test for *irresponsibility* is insufficient. The knowledge of right and wrong is not always considered a sufficient ground by our *juries* for a verdict of *guilty;* and when that verdict is given, and sentence passed, the daily respites and commutations that afterwards take place are open avowals on the part of the legislature of the erroneous principle upon which the laws are founded.

The principle is erroneous for two reasons—first, because, if, strictly applied, it would be and is found insufficient to protect a large number of insane persons from the sentence of death, who are confessedly, as I have shown, not proper objects for punishment. In point of fact, almost all lunatics know right from wrong. How else are the large numbers of the insane who inhabit our large asylums governed? They are all more or less amenable to rules and discipline. Very few, even of the most ungovernable, are not open to advice,—very few of them fail to keep a promise with a punctilious regard to honour, quite as great if not greater than that of the sane of the same class. The great mass of the inmates of all asylums control their passions and desires under the influence of motives, just as sane people do. A great majority of the lunatics who have committed crimes showed that they knew the consequences of their act: some of them committed the crime for the avowed purpose of being punished. Hadfield shot at the king in order that he might be executed. Innumerable cases of a similar kind have occurred since. In the very latest trial which has taken place in this country for murder, where insanity was pleaded, that of William Gabbits for the murder of his fellow apprentice, the boy confessedly knew right from wrong in the act which he committed; he said his prayers while he was doing it, and immediately afterwards went and delivered himself up to the police. He was condemned to death, and, as usual, the sentence was afterwards commuted to penal servitude for life. "It is highly improbable," says Dr Bucknill, "that any criminal, whose insanity was not so obvious and pronounced as to leave no room for doubt, and no real necessity for trial, has ever deliberately taken away the life of a fellow-creature without the knowledge that, in so doing, he was rendering himself amenable to lose his own life."—(P. 53.)

But the second, and perhaps the most important, aspect in which the legal test of responsibility for the insane strikes a medical man as being erroneous, is that it assumes that the *knowledge* of *right* and *wrong* is always accompanied with the *power* of *doing* the right and eschewing the wrong.

Nothing can be more entirely at variance with the truth. Morbid emotions, passions, and desires are the most essential features of insanity, and these diseased feelings are generally entirely beyond the control of the sufferer.

Delusions, so long regarded in law as essential to the proof of insanity, may be entirely absent; some moral perversion, some morbid affection of the emotional part of our nature is always present. This is the most essential part of insanity, and the most unvarying and constant feature.

A mother, in violation of the common instincts of nature, hates her new-born child and murders it; a wife tells her husband that she fears she will kill him during the night, as she wakes with a horrible impulse to do it, and implores him not to sleep with her; a husband and a father voluntarily seeks protection in an asylum, which he refuses to leave lest he should murder his wife or some of his children, to which he is impelled by an impulse over which he has no control; a gentleman, who has been for a lifetime a model of temperance, and prudence, and piety, suddenly becomes drunken and lavish to folly, and impious and obscene in his language and habits, and may even lose all self-control over his actions and conduct, tear his clothes and bedding to ribbons, and dance round his room naked, laughing, singing, and swearing incessantly, and all this without any intellectual delusion. These, and many such cases, I might cite from my own experience, and add to them, were it necessary, hundreds more from the experience of others, to prove that insanity—disease of the mind—may exist without any delusions, and that the individual acts under the influence of diseased feelings or impulses, over which he has in most instances very little, and in many no control. This form of insanity, which is the cause of more suicides than any other, and the cause of many homicidal acts, is altogether repudiated in our courts of law. *Moral insanity*, as we call it, is scoffed at from the bench, and the judge will tell the jury that although all the physicians in Europe were to swear that the prisoner committed the fatal act under the influence of an impulse over which he had no control, yet, if he knew right from wrong and laboured under no delusion, it was his duty to tell them he was *responsible.*

No doubt this is *law*, and it is the duty of the judges so to explain it to the jury; but it is not *truth*, and although it may be received as law and truth by the jury, and lead to the condemnation of the accused, it is very often afterwards repudiated by the public and the authorities, and the common humanity and common sense of the community leads to a commutation of the sentence.

Why is all this uncertainty in the law perpetuated to the enfeeblement of its own ends? Because, we are told, if we were once to admit the doctrine of *irresistible impulse* as an excuse for crime, there would be an end of all distinctions between right and wrong, and every crime would be sought to be palliated by the strength of the impulse, be it passion, lust, or revenge, which prompted it. There must be a *delusion* to constitute the plea of insanity as an exculpation for crime.

To this, my reply is, that there is insanity without delusion,—insanity which leads to crime, to acts of homicide, over which its victim has no control, and therefore is not responsible. Truth is inexorable; there is no more doubt about the existence of such cases than there is about the existence of smallpox or cholera; all medical men have testified to their existence; the public knows it, and the Home Secretary is called upon to know it, and therefore it is time that the law should know it and recognise it, instead of making wrong judgments for the sake of being consistent with their precedents and the opinions of the twelve judges.

The danger of confounding insanity with mere vice and passion must be faced and disposed of.

The existence of such a difficulty, if it does exist, is no reason why the victims of disease should be condemned to the same punishment as the slaves of vice or of passion.

But there is in point of fact no such difficulty. If the legal test of knowing right from wrong is abandoned, and for it substituted the simple fact to be proved, namely, that the person laboured under *disease* affecting his mind; if the acts, whatever they are, of which he is accused, can be clearly traced to the existence of *disease*,—disease of the mind,—then certainly the pains and penalties of the law should be mitigated and appropriated accordingly.

I do not maintain that lunatics are not responsible; some of them are, and some of them are not. Certainly, however, those who labour under a disease which impels them against their own instincts to murder, are, at least, as irresponsible as those who commit murder for some fancied offence. So far from regarding all the insane as irresponsible, I have already stated that in the wards of every well-regulated asylum their conduct testifies to their responsibility.

What I maintain is this, and it is a point which if once admitted by our legislators, would go far to dissipate all the difficulties which surround the subject, that there are *degrees* of *responsibility* among the insane. They are responsible in proportion partly to the amount of their *knowledge* of *right* and *wrong*, and partly in proportion to their *power* of controlling their diseased impulses, or, in other words, of *doing right* instead of wrong.

The error which pervades the whole laws regarding idiocy and insanity is this, that they recognise them as something *absolute*. Now, it is not so; as in regard to civil incapacity, which I showed was a question only of degree, so it is with responsibility; it is a matter of degree. There are in all asylums many idiots, imbeciles, and lunatics, who are amenable to discipline, and whose conduct can be regulated within certain limits by motives, and who are responsible to a certain degree for their conduct, and treated accordingly. There are others who act under a blind impulse, and are utterly unconscious of what they are doing at all, and therefore entirely irresponsible. Between these two extremes there are many varying grades of responsibility, which are easily recognised, and the subjects of which are trusted and treated accordingly in the asylums where they reside, the degree of trust or liberty accorded to each being apportioned to his knowledge of right and wrong, and capacity for self-direction and control.

Why, I think it may be fairly asked, should lunatics out of asylums be treated on different principles from lunatics in asylums? Medical men, actuated by motives of philanthropy and guided by enlightened views of mental disease, have developed a code of principles for the management and discipline of the insane; and those principles have gradually so far commended themselves to the legal mind, that we are now obliged to carry them out in all our asylums under heavy penalties. We don't punish our cleptomaniacs, we search their pockets, and tell them not to do it again;

we don't give a patient thirty days for breaking glass and destroying furniture; or if he assault an officer in the discharge of his duty, we don't resent the act; even if he attempts the life of any one, he is only restricted so far in his liberty, or subjected to such surveillance as may prevent his doing it again. All such offences are constantly committed by patients, who, if subjected to the legal test of responsibility (knowing right from wrong), would be sentenced to punishment; yet, any official or servant of an asylum who treated them on the same principle would subject himself to prosecution under the statute, and be punished with fine or imprisonment.

Why, then, I repeat, should the criminal lunatic at large be subjected to a different code of laws from those which the legislature requires us to act upon in regard to him when he is placed under our care?

I do not deny that the rules and regulations and discipline of an asylum exercise a beneficial, and even curative, influence upon such persons as those to whom I have referred. I do not urge it as my opinion that they are altogether free from responsibility. But what I do urge is, that the laws should recognise the existence of *disease*, and punish, if it will punish, with a deliberate reference to its existence, and by a sentence adjusted to meet the special circumstances of the case. If, *e.g.*, an intelligent and educated gentleman steals silver spoons, and has had this penchant all his life,—if he steals for no motive of gain, but merely from a morbid craving, and quietly allows his valet to empty his pockets at bedtime and return the stolen goods to their rightful owner,—what I say is, don't hold me up to ridicule in the court, if I, as a medical man, say he committed this act from disease, and laugh me to scorn on the ground that I would screen all the thieves in London on the plea of their being cleptomaniacs. If the gentleman does labour under disease, —if there is such a disease,—it is due to him, and to the law, and to the medical witness, to recognise it as such, and meet it accordingly.

If I make out *disease*, I do not think there is any danger of confounding it with common thieving. If the law, recognising it in the light of science, chooses to apportion a certain punishment to it, I find no fault. The punishment may do good, it may act as a deterring motive for the future, overruling the morbid tendency.

In another case of a similar kind, the act of stealing may be only one symptom, as it often is, of serious and fatal disease of the brain, and it may be found by the medical expert that the accused party has many other symptoms of brain disease affecting his mind,— very probably he steals pebbles and bits of glass, believing them to be precious stones. In that case, I believe the law, even as it is at present, might let him off on his friends undertaking the proper care of him.

Now, in the first case which I have adduced in illustration of my position (and it is not a fictitious one), there is a *degree* of responsibility amenable to deterring motives, and the subject of it may, to a certain extent, be a proper object therefore of a modified punishment suited to his case. In the other case, there is no responsibility, —and no punishment, no motive, and no treatment would mend or change the tendency to steal and hoard.

As in these illustrations, so in all other cases of crimes committed by the insane, there are degrees of responsibility. A man of weak mind, an imbecile, a man of a low type of organization, may commit a murder from motives of revenge of a trivial kind, which would not influence a man of perfectly sound mind. One person labours under an impulse to commit suicide, and resists the impulse; another, under the same morbid impulse, destroys himself. A mother is impelled by a morbid impulse to destroy her offspring, and she shuts them up in her house and walks up and down the road till her husband returns home, and so restrains herself from the act which another labouring under a similar disease perpetrates. A man labours under a horrible impulse to kill his wife whenever he sees an axe or a sharp instrument, and to escape the commission of so horrible an act he seeks protection in an ayslum; another, under the same morbid impulse, destroys his wife, and very likely afterwards attempts his own life.

All these cases, and they are of daily occurrence, show that there are degrees of self-control, and *degrees* therefore of *responsibility*, varying with the amount of disease under which the individual labours.

Why should not the *law* recognise this fact? Why should it not legislate with a view to it? Why should a verdict of manslaughter or *culpable homicide* not be received in any case of murder where the existence of mental disease, even in a partial degree, was distinctly proved? If such a verdict were accepted, it would, I think, put an end to the difficulties which attend such cases, and the present uncertainty of the law regarding them. I do not see how any judge could find more difficulty in apportioning and adjusting the sentence than in any ordinary case of culpable homicide, where the punishment may vary, in the discretion of the judge, from a fine of a shilling to transportation for life. The whole facts of the case would be presented to the consideration of the judge, and the sentence might vary from confinement in a criminal asylum to penal servitude for life or a term of years. Surely it would be better that the difficulty were fairly met and adjudicated upon in this way than that the prisoner should be solemnly condemned, and the mistake be afterwards rectified, and, in a certain number of cases only, by public opinion and the Royal mercy.

In practice, our courts have acted on this principle to a certain extent already in reference to some crimes, such, *e. g.*, as infanticide, by restricting the verdict asked for to one only of culpable homicide. If such a verdict were accepted in cases of murder, where insanity was proved, I do think many of the present unpleasant differences and disputes regarding this plea would disappear. I do not, of course, mean to say that the present verdict of not guilty, on the ground of insanity, should not still be accepted, or that insanity should not still be received as a plea in bar of trial.

The recommendation of the recent Royal Commission on capital punishments, that a verdict of murder in the second degree should be accepted in our courts, might meet the difficulty, if it were not that in that report murders of the first degree are defined to be those "committed deliberately, and with express malice aforethought." Now, this would apply to some of the murders committed by lunatics. They are not unfrequently premeditated, and perpetrated from revenge. If a verdict of murder in the second degree was open to the jury, in cases where they were satisfied that a certain amount of insanity was proved, although not coming up to the legal test of irresponsibility, and the sentence left to the discretion of the judges, there would, I believe, be an end to all the evils the present practice engenders.

The same principle, that of recognising degrees of irresponsibility on the ground of imbecility or insanity, should be extended to the treatment of minor offences.

No one can read the Reports of the Chaplain of Newgate, the Records of Prison Life, recently published by a Prison Matron, the article by Dr Thomson on the Epileptics of our General Prison, and similar records, without being satisfied that a very considerable portion of our permanent criminal population labours under mental infirmity and disease. There seems to be a feeling on the part of some pseudo-philanthropists, that asylum physicians have a tendency to seclude too many persons in asylums, on account of slight peculiarities or eccentricities. I can answer for it, that certainly none of them would desire to receive, if they could avoid it, any of the insane members of our criminal population. They certainly are a curse in any well-regulated asylum. Not the less, however, do I consider that they are proper objects of protection, and segregation, and treatment.

If the public looked at the subject from an economic point of view, I think they would rather congratulate themselves that such persons were permanently placed under proper care, than allowed, after repeated sentences of thirty and sixty days, three months, and so forth, to live at large, perpetuating a diseased and degenerated race, which might otherwise gradually die out.

Another great improvement in our mode of conducting trials where insanity is pleaded, would, I think, undoubtedly result, if the system followed in France were adopted, of employing *experts* to report on the case under examination. These reports, made on oath, after a careful and repeated examination of the prisoner and of witnesses, and made by two or three experienced physicians appointed by the court, or, in part, it might be, by the prisoner's counsel, would, I have no doubt, contrast favourably with our present medical evidence, and convey sound psychological advice and evidence free from the influence of partisanship. These witnesses might be afterwards subjected to an examination in court, if necessary. No one can read the admirable reports in reference to such cases by the medical experts employed, which appear from time to time in the Annales Médico-Psychologiques, without being satisfied with the truth and justice of this statement.

Dr Bucknill very justly says, that in this country we offer a sad contrast to that system in our courts, "where an array of medical men are marshalled by the attorneys on each side, according to their preconceived opinions of the case. These medical witnesses may usually be divided into two classes, those who know something of the prisoner, and nothing of insanity, and those who know something about insanity, and nothing of the prisoner. They generally succeed in neutralizing each other's evidence, and in bringing medical evidence into contempt, at least among lawyers."

The system of written medico-legal reports by experienced men chosen by the court, would, I doubt not, put an end to this obloquy against the profession, and greatly promote the ends of justice.

Lastly, I may be permitted, before I conclude, to say one thing more, and that is, that if this College would make the study of mental diseases imperative upon its licentiates—a subject quite as important, if not much more so, than several of those subjects included in the medical curriculum, that would tend, among other things, to bring medical testimony into more esteem in public opinion, and in our courts of law, in cases of insanity, than it at present enjoys.

The cure or alleviation of diseases of the mind appears so obviously to transcend in dignity, interest, and importance, the treatment of any bodily disorder, that you may well wonder that the study of this subject has been so much ignored by our Colleges and Universities; and perhaps you may be inclined to say that I have finished my address where I ought to have begun it, and that, if I had told you that our College did not require their students to study mental diseases, there was no further explanation required for the differences of opinion among medical witnesses in questions about insanity.

ARTICLE V.—*On the Use of Chlorine in the Treatment of Asiatic Cholera and Choleraic Diarrhœa.* By WILLIAM M. DOBIE, M.D. Edin., F.R.A.S., late Senior President of the Royal Medical Society.

CHOLERA made its appearance in Chester early in the month of September last year. One of its earliest victims was the late Alderman Trevor, who had recently held the office of Mayor of this city for two successive years. It was not, however, till near the end of the month that cholera cases became frequent, and the municipal authorities found it necessary to open a small hospital for their reception, in the park recently presented to the city by the Marquess of Westminster. Three ladies, resident in the city, undertook the nursing of the patients, and remained in the hospital, working day and night, during the two months in which the epidemic prevailed. The hospital was finally closed on the 25th of November, though a few sporadic cases were afterwards reported, and one unmistakable case was seen and treated by myself as late as the 12th of January of this year. The exact number of cases of cholera which occurred in Chester I am unable to ascertain, but I have reason to believe that there were at least 130.

The object in this communication is, not so much to give an account of the epidemic as it occurred in Chester, as to describe the general result of the trial of chlorine as a remedy; having seen no account in any of the medical journals of its internal use in Asiatic cholera.

At the commencement of the visitation, the cases were of a very malignant type. Premonitory diarrhœa was often absent, and the patient passed into a state of collapse after a couple of hours of violent vomiting and purging: death took place in from ten to twenty hours. The first six cases died; they were treated in different ways: some with calomel and small doses of opium, others with mineral acids, etc.

Feeling disheartened by the results of ordinary methods of treatment, and remembering that my friend, Mr Moreton, of Tarvin, had told me that in a sporadic case of Asiatic cholera, occurring in his district, he had used chlorine water with apparently very good effects, I determined to make a trial of it. Though the first patient on whom it was tried died, the improvement observed following its use induced me to administer it in other cases.

Encouraged by the results of further trials, I asked Mr Brittain, the senior surgeon to the Chester Infirmary, to make a trial of chlorine at the cholera hospital, of which he had the sole charge. He most kindly acceded to the request, and further allowed me to pay a daily visit to the hospital. I was thus enabled to form a more correct and satisfactory opinion as to the usefulness of the medicine.

As was to be expected, the results in the cholera hospital were more satisfactory than in the cases treated in the town. In the hospital the air was pure, the nursing perfect, and every possible remedial appliance was brought to the aid of the sufferers. I will now give short accounts of those cases in the town which were attended by myself or my assistant. Most of these were amongst persons living in squalid, unventilated habitations. Many were moribund before they were seen; and nearly all were imperfectly nursed. The chlorine treatment was commenced at the beginning of the visitation of cholera, when the disease was in its worst form. Had the good effects of chlorine been manifested only in the later period of the epidemic, I should have felt more doubtful as to its remedial power.

The following cases are given in the order in which they occurred. That of Alderman Trevor is not included in the list.

1. A. D., girl, aged 9. Cramps; vomiting. Rice-water evacuations; died in three days. The mother of this child was seized a few days later, and died. Treated by acids.

2. R. W., a middle-aged labourer. Moribund. Died in two hours after being seen.

3. J. W., a mechanic, aged 45; of dissipated habits. Rice-water diarrhœa and violent vomiting. In two hours was in a state of collapse. Sulphuric acid was tried in this case without any good effect. Chloroform relieved the cramps. He was taken to the Cholera Hospital, and died about ten hours after seizure.

4. J. A., aged 55. Rather delicate-looking. His eldest son lying dead in the house without having been seen by any medical man. Nervous depression very great; sank in eight or ten hours. Calomel, with small doses of opium; chloroform frictions, etc. He had been a great drinker.

5. J. A., aged 56, wife of the above. Same symptoms; sank in eighteen hours. Treatment the same.

6. M. J., aged 25. Recently confined; treated by acids. Was removed to the Cholera Hospital, where she sank in a few hours.

In the following cases, chlorine was tried; in some of them the collapse was so intense as to render any plan of treatment next to hopeless:—

1. J. H., boy of 5. When first seen, had been purging and vomiting from the previous day. The skin blue; eyes sunken, more than half open, and turned upwards. Pulse could not be felt, and I was told that he had been frequently convulsed. A teaspoonful of the chlorine mixture was administered in an hour after I first saw him, and afterwards given regularly. Warm flannels were applied to the surface, and iced milk and water and chicken-tea were given. I called in a few hours hardly expecting to find him alive. Warmth, however, was restored to the surface, and the pulse was distinctly perceptible at the wrist. His colour was not so blue, and he was able to swallow. It would be tedious to relate the whole particulars of this case; suffice to say, that though he had repeated convulsions, he lingered on for three days, and died comatose. Had it not been for the chlorine, I hardly think the little fellow could have lasted three hours after the time I first saw him.

2. R. H., a railway guard, was seized on his way to Holyhead, and brought back to Chester. He was carried to his own house, where I found him in a state of deep collapse. Chlorine was administered. He made no rally, and died in a few hours.

3. H. J., aged 55; labourer. When first seen, had been ill three or four hours. Vomiting and rice-water purging had been very severe. Everything was now passed into the bed. Eyes

sunken, skin blue, and cramps intense. Mustard-plasters, warm flannels, friction, were assiduously employed, and the chlorine mixture, with ice and iced drinks, given regularly. Reaction set in, and in three days the evacuations were assuming a natural colour, and he was altogether going on well. He did not quite recover his strength for some weeks.

4. S. S., married woman, aged 45. In a low, damp, unventilated house. Violent cholera and cramps. She rallied under the use of chlorine, but afterwards sank. The nursing in this case was very bad. She steadily refused to be removed to the Cholera Hospital.

5. H. K., aged 60. Mechanic; of very dissipated habits. Had been suffering from diarrhœa for a week. Cholera had set in twenty-four hours before I saw him. I found him in a dying state. Chlorine was tried, but he did not rally, and died in six or eight hours.

6. A. R., aged 55. Steady man. Had been subject to diarrhœa. Took a dose of castor-oil on the morning of the day of seizure. Collapsed when first seen, and died in twelve hours. A few doses of chlorine were administered. Vomiting and purging did not occur after he was first seen. He had exposed himself to cold during the occurrence of the purging.

7. J. G., aged 35. A married woman. There was scarcely room to walk between the two beds, which nearly filled the whole of her wretched, ill ventilated bedroom. Chlorine was given with considerable benefit. She rallied, but afterwards sank. I attribute her death to inefficient nursing.

8. A. G., labourer; middle-aged. Rice-water purging, vomiting, and cramps. Improved on the second day under chlorine, and good nursing. Made a good recovery in about a week.

9. J. P., aged 27. Choleraic symptoms: cramps, purging, and great depression. Chlorine was given regularly for three days. He made an excellent recovery. Nursing was very good.

10. J. W., had been purged more than twenty times before he was seen. Quite blue, and dreadfully cramped. Felt that he was dying. Made a good recovery. The first dose of chlorine stopped the rice-water evacuations, and ice greatly relieved the sickness.

For the following memoranda as to the results of treatment in the Cholera Hospital, I am indebted to the notes of Miss Frances M. Wilbraham, who, with Miss Ayckbowm and Miss Barker, so kindly and skilfully nursed these poor sick people :—

Of the first 29 cases treated without chlorine, 21 died, and 8 recovered. After the 20th of October, the cases were all treated with chlorine: of these 6 recovered, and 2 died. Of the 8 recoveries without the use of chlorine, 5 had secondary fever, and 1 suffered from very severe head symptoms, continuing for several days. The following list contains only those patients treated by chlorine in the hospital :—

1. E. T., a girl, aged 9. Violent rice-water diarrhœa; thirst,

cramps, and restlessness; took chlorine regularly, with ice after each dose. Recovered.

2. W. C., a boy, aged 5. Violent sickness; chlorine given frequently, seizing the moments when the stomach was quiet; no after-fever. Recovered.

3. T. C., a man, aged 27. Rice-water purging. Recovered, without consecutive fever.

4. E. H., a woman, aged 23. Lay neglected in the town for many hours after seizure. Had been half-starved. Brought in a dying state to the hospital. Chlorine seemed to rouse vitality for several hours. Died.

5. M. R., aged 60. Husband had died three days before. She did not rally under treatment. Died.

6. C. L. Had been confined ten days before her seizure. She had violent cramps, sickness, and rice-water purging. The thirst and restlessness were excessive. She several times appeared to be dying. Blueness and clamminess of the skin, sunken eyes, and great exhaustion. She was for some time picking at the bedclothes. Chlorine was administered regularly from Sunday till Wednesday, when Mr Brittain recommended the dose should be given at longer intervals than usual. She had flushings and excitement, but no after-fever. Recovered.

7. E. W. Very weakly woman. Chlorine seemed to revive and warm her. Recovered without subsequent fever.

8. E. R., an intemperate elderly woman, very weak. Recovered.

Miss F. M. Wilbraham observes, "We steadily refused to let the patients drink anything for full ten minutes after each dose, as drinking instantaneously produced sickness. We thought that this medicine decidedly checked the secondary fever."

Including the case of Alderman Trevor, and taking into account that two of the cases in my first list died in the Cholera Hospital, it appears that of 34 cases of cholera in the town and in the Hospital, treated on the ordinary plans, 8 recovered, and 26 died; and of 18 treated by chlorine, 10 recovered, and 8 died. If this result of the observation of 52 cases of cholera be of any value, it shows a striking advantage in the mode of treatment by means of chlorine.

I may here observe that the chlorinated mixture is apt to be imperfectly prepared by the druggists. When the chlorate of potash is dissolved in a considerable quantity of water, and the pure hydrochloric acid added, scarcely any chlorine is set free.

If prepared in the following way, the result is very satisfactory:—

Take a clear dry wine or brandy bottle, place it for a few minutes upon a hot brick, or upon a stove, until it becomes thoroughly warm, then take eight grains of chlorate of potash in coarse powder, and add to it one fluid drachm, by measure, of chemically pure hydrochloric acid; agitate the chlorate in the acid until all effervescence ceases, and the chlorate is completely dissolved. Then add an ounce of water, shaking the bottle briskly. Continue thus to add sixteen ounces of water, ounce by ounce, shaking the bottle between each addition. If two fluid drachms of chloric ether be now added, the mixture is complete.

To this mixture I should be much inclined to add a moderate dose of the liquor morphiæ hydrochloratis, following out a suggestion of Dr Alderson, of which I shall say a few words presently.

The chlorine mixture, as thus prepared, is about one-third saturated with chlorine, it can be readily taken by patients direct from the bottle; but it was generally administered with an equal bulk of water. Three drachms of the mixture are about equal in strength to a drachm of the liquor chlori of the British Pharmacopœia.

The dose of the chlorinated mixture was half-an-ounce every hour, or every two hours, to an adult; if the first dose was vomited, another was given after a shorter interval.

Mr Brittain chiefly used the liquor chlori of the British Pharmacopœia in drachm doses, diluted with a tablespoonful of water.

In the very numerous cases of severe choleraic diarrhœa which occurred in Chester during the prevalence of the epidemic, no remedy appeared to me at all equal to the chlorine mixture. Many patients, grateful for the speedy relief, spontaneously said, "That is a wonderful medicine—I was better after the first dose." There cannot be a doubt that many of these cases would have passed into true cholera had the disease not been checked in the early stage. This leads me to remark, that in houses where cholera had broken out, I generally ordered the whole family to take a teaspoonful of the chlorine mixture two or three times a-day, and in no case, when this plan was adopted in addition to thorough ventilation and disinfection, did the cholera extend to other members of the household.[1]

Chlorine seems to have a decided effect in restraining the excessive exudation from the mucous membrane of the bowels. The slight acidity of the mixture probably adds to its usefulness, as it has been demonstrated that the osmotic current travels from the acid to the alkaline side of a membrane. The chlorine renders the retained secretions innocuous, and may possibly exert a special action on the blood itself if the osmotic current be reversed, even supposing that all active absorption from the bowels be in abeyance.

Dr Alderson, in a very interesting lecture, recently published in the *Lancet*, has called attention to the fact discovered by Poiseuille, that the hydrochlorate of morphia has a remarkable power in checking osmotic action. He goes on to observe,—"It was ascertained by him that when that substance was added to saline solutions the osmotic action tested between them and serum is immediately affected. The current is at first stayed, and after a time is actually reversed. This was confirmed by Bacchati, and it at once reveals the mode of action of morphia in checking diarrhœa, and in promoting constipation." Further on in the lecture, Dr Alderson remarks,—"Checking the flow of albuminous fluid from the bowels, on the principles displayed by Poiseuille and Bacchati, has reason in it; and I should advise for that purpose the use of morphia in the form of hydrochlorate; the hydrochlorate having shown a specific power not only to stay the current, but to reverse it."

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[1] Mr Shepheard, chemist, of this city, tells me that the late Dr Sawyer of Guildford Street, London, fourteen years ago, was in the habit of ordering chlorine in cases of choleraic diarrhœa.

I should therefore be strongly inclined, in the future treatment of cholera, to add moderate doses of the hydrochlorate of morphia to the chlorine mixture during the continuance of the diarrhœa, and after its cessation, or when collapse had fairly set in, to give the chlorine mixture alone. I noticed that the chlorine mixture had a remarkably sedative action upon the stomach and bowels; the vomiting as well as the diarrhœa being restrained. My impression is, that the chlorine mixture, with the addition of chloric ether, was better tolerated by the irritable stomach than the diluted chlorine water given alone.

The pulse, in cases where it was quite imperceptible at the wrist, became in a short time full and strong. The capillary circulation being restored, the surface became warm, and the purplish blue of the skin changed to a more natural hue. The cramps were subsequently less severe. Reaction was seldom violent, and I do not recollect one instance of secondary fever in the cases treated by chlorine.

The secretions of the liver and kidneys in mild cases were rapidly restored. Another point to be observed is the remarkable comfort it gave to the patients; the excessive restlessness was much mitigated, and one man said, "I could not live without that medicine."

The object of this communication will be gained if the attention of my medical brethren be directed to this remedy, already so useful in bad cases of scarlatina. I feel that its true value in cholera can only be properly tested by trial on a large scale. So many new remedies have been proposed and found worthless that nothing short of the conviction that chlorine will prove valuable in combating this dire malady, could have overcome my reluctance to add another to the long list of anti-choleraic medicines.

Part Second.

REVIEWS.

On Addison's Disease: Clinical Lectures on Addison's Disease, and a Report on Disease of the Supra-Renal Capsules. By EDWARD HEADLAM GREENHOW, M.D., F.R.C.P., Assistant-Physician to the Middlesex Hospital, etc. London: 1866.

WITH the exception of Dr Wilks, no one perhaps has done more to add to our knowledge of "Addison's Disease" than Dr Greenhow, and we are glad that he has now brought together his various communications on the subject. The volume before us consists of two clinical lectures, some cases of the disease, and a report with an elaborate table of cases lately laid before the Pathological Society; the whole constitutes a very complete account of this still somewhat mysterious malady.

Dr Greenhow gives an excellent description of the symptoms of Addison's disease, but as it contains nothing novel, we need not specially allude to it. With regard to the pathological change which occurs in the capsules, there has been some difference of opinion. Dr Addison himself thought that any morbid condition of these organs might give rise to the symptoms which he described; and his cases include several which are not really examples of the disease in question. Dr Wilks was the first to show that the remarkable phenomena of the disease are only associated with one form of degeneration of the capsules, that which Dr Addison simply styled scrofulous, but which Dr Wilks does not consider to be tubercular. Dr Greenhow seems inclined to recur to the opinion that the lesion is of a tubercular character, and expresses himself in the following manner:—

> "By the great majority of reporters this particular organic lesion has been described as tubercular or scrofulous; but Dr Wilks appears to regard it as an idiopathic or peculiar disease; and Dr Burdon Sanderson, in reporting upon a specimen of this disease in the capsules, which he kindly examined for me, says, that no structure was to be found having any resemblance to tubercle. Dr Dickinson also speaks of the disease as something *sui generis*. On the other hand, my friend, Dr Cayley, who has carefully examined specimens from four of the cases I have reported to the Pathological Society, considers the deposit in the capsules to be essentially of a tuberculous nature, as do likewise Dr Child of Oxford, and Dr Hayden of Dublin. Rokitansky, Virchow, Trousseau, and other foreign authorities, also agree in applying the term tubercular to this particular deposit in the supra-renal capsules; and, certainly, its very frequent occurrence in persons affected with tubercle in the lungs, or other

organs, taken together with the rarity of its association with any other disease, cannot fail to indicate some intimate relation between this particular lesion characteristic of Addison's disease, and the tubercular diathesis."

For our own part, on this point, we agree with Dr Wilks; from the histological characters of the deposit it may be impossible to determine whether it is tubercular, or an inflammatory product undergoing degeneration; but in many of the published cases it has been expressly stated that there was no tubercle elsewhere; and we ourselves once met with a case of tuberculosis, where the capsules were almost completely converted into masses of tubercle, but in which there had been no symptoms of the disease.

An interesting point in Dr Greenhow's work, is the importance which seems to attach to disease of adjoining parts, or to external injury in giving rise to this disease. In a considerable number of cases, there were evidences of inflammation in the cellular envelopes of the capsules, such as thickening, and firm adhesions to adjoining organs. In several cases, the disease had certainly extended to the capsules from adjoining lumbar abscess; while, in at least three cases, there was the history of local injury preceding the symptoms of the disease.

With regard to the duration of the disease, it is, as in the case of all chronic diseases, difficult to arrive at a perfectly definite conclusion. The average duration of all the cases collected by Dr Wilks[1] was eighteen months; but, as he himself remarks, this period would in all probability be considerably prolonged if a larger number of observations were accumulated. Dr Greenhow alludes particularly to the well-marked remissions met with in many cases, during which the patients have been enabled to return to their ordinary avocations, although they have eventually died of the disease. During these remissions, Dr Greenhow thinks he has observed the discoloration of the skin become less deep, though when once distinctly marked he has never known it entirely disappear.

Addison's disease, though so far as we know incurable, may yet be benefited by appropriate treatment. Dr Greenhow very properly lays great stress on the importance of prolonged rest, and careful abstinence from bodily or mental exertion. In one of his own cases, the injudicious administration, by the patient's mother, of a drastic purgative, appears to have been the immediate cause of the fatal seizure. The treatment recommended by Dr Greenhow consists in the administration of as nourishing food as the patient's stomach can bear; the use of ice, soda-water and brandy, chloroform, or bismuth, to allay vomiting or nausea; and the employment of tonics, such as cod-liver oil or glycerine, and the tincture of the sesquichloride of iron. We conclude by repeating, that we consider Dr Greenhow's volume a valuable contribution to the history of Addison's disease.

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[1] Guy's Hospital Reports, 1862.

Traité pratique des Maladies de l'Uterus et de ses Annexes. Par A. COURTY, Professeur de Clinique à la Faculté de Médecine de Montpellier. Paris: 1866.

WE have read this volume by M. Courty. It numbers 1088 octavo pages, closely printed; and to peruse it is a long labour, for, though a valuable, elaborate, and useful work, it has not the quality of easy and pleasant readableness which Frenchmen are justly celebrated for communicating to their literary productions. It has, however, substantial good qualities: it is accurate, copious, replete with literary references, and contains many ingenious reflections and proposals by its author. In some parts it is far too copious; and, as usual, these parts are devoted to subjects regarding which little is really known; on which, therefore, almost any amount of flabby talk may be expended by one who has the gift of easily writing. To justify this criticism, we may notice that 312 pages are devoted to general considerations on anatomy, physiology, diagnosis, and treatment; 496 to diseases in which there is no permanent organic change; and only 191 to the organic diseases. Now, it appears to us that these last have too little space given them, or the others have too much. And perusal of the book convinces us that the error is in giving the others too much. For the shorter chapters in the book are decidedly the best. Even in the long part of the work we can name chapters which are comparatively brief, and which are, at the same time, comparatively excellent; for example, that on peri-uterine inflammation occupying 50 pages. To contrast with this, we may name the four chapters on fluxion, 12 pages; on congestion, 10 pages; on engorgement, 12 pages; on metritis, 33 pages. Our readers will no doubt be at some loss to distinguish from one another, fluxion, congestion, and engorgement of the uterus, and all of them from inflammation; and, after studying the subject, we do not care to try to enlighten them. We notice the point to characterize the book; and to do this more completely, we extract a set of definitions by M. Courty, to illustrate briefly the ingenuity which he has applied to the subjects of the long chapters referred to. "Atony," says he, "is want of tone, astheny, want of acting forces, adynamy, want of radical forces."

This book, however, while it is open to criticisms such as those given, deserves much more of praise. M. Courty is a laborious, experienced, and skilful practitioner, and does not fail to give to his work high qualities. We may tell British authors that he uses with advantage the resources of homalographic anatomy, chiefly resorting to the works of Legendre, a valuable source of instruction scarcely ever referred to on this side the Channel. And to the anatomists of St Petersburg, with their unlimited supply of cold, we would give every encouragement to farther developments of this art. For the work of Pirogoff does not nearly exhaust the instructive matter to be expected from it. Especially do we desiderate its application to pregnancy and to the diseases of the pelvis and abdomen.

Practitioners, unacquainted with French medical fashions, will be astonished to find M. Courty recommending that digital examinations of the womb be made with the patient on her feet, and even declaring this attitude to be indispensable. If this last adjective is properly applied by Courty, we fear there are very few proper examinations made in a year in Great Britain.

General medical readers will curiously notice a fault in this book, though one to which we do not attach very high importance, namely, the constant allusion to and recommendation of patent medicines; or, in other words, of medicines whose composition is not known. In this respect, we can claim great purity in British authors, and certainly it is creditable to them. But, perhaps, for at least alluding to such medicines, M. Courty has even greater reason than we have; for the French people may unfortunately be more addicted to faith in empirics and use of their drugs than we are. Anderson's pills are now very little known and used in Edinburgh, where their "inventor" lived. Recently, on the death of an officer in the army, who by inheritance came to possess the secret, Anderson's famous pills came before the public as the subject of an action in our supreme courts. The secret has been purchased by certain parties, who are making great efforts to revive the nostrum, which, dying in its native land, has a vigorous existence in Montpellier. And nearly the same story has to be told of Morison's pills, which, like Anderson's, frequently cause trouble to the doctors of the south of France, though now but little used in the native country of their "inventor."

It is easy to find from this book that, just before publishing it, M. Courty had made a professional tour in Great Britain, and we commend him for his zeal. But, note-book in hand, he has put down a few "crams," and not a few ill-digested pieces of pathology and treatment, which are now embalmed in his large volume. Among other places, we find he visited Edinburgh; and we congratulate him and ourselves on the attention which he has paid to ovariotomy, including the experience of Dr Thomas Keith, the eminent ovariotomist of this metropolis. This great operation, one of the triumphs of modern and of British surgery, has had a chequered history in Edinburgh. It is historically true that this town played a chief part in bringing it, amid all the difficulties attending such an enterprise, through its earliest trials, the first steps in its progress. Lizars not only repeatedly performed it; he also published a large illustrated work on the subject. But he was not successful in his operations as a whole. Had he been more fortunate, his name would have acquired very great lustre from the circumstance. The operation, however, was not forgotten. Clay perseveringly, against every kind of discouragement, recommended and practised it. Then it came into the hands of Spencer Wells, Baker Brown, Tyler Smith, Thomas Keith, and many others, who now regularly perform it with marvellous results, and with general and well-merited acclaim. But, to return to Edinburgh and to M. Courty. We find our author giving statistics of ovariotomy, and the results of Dr Thomas Keith's operations are shown to be the best in the world. We know M. Courty's statistics are not quite accurate, but we believe the conclusion just stated to be very near the truth; and we are pleased to find it so. Dr Keith has published his cases in the pages of this journal, and his last series of operations has presented a mortality of only a fraction more than 1 in 5. As our readers know, his operations are characterized by the greatest boldness, surgical skill, and surgical simplicity. We will only call to mind his first case, the most remarkable and the most encouraging that has been put on record. The tumour weighed much more than a hundred pounds, and was heavier than the convalescent patient. It had old and most extensive adhesions. Patches of cyst-wall were left adherent to the abdominal wall. The belly was laid open from sternum to pubes. The woman made good her complete recovery!!

On Uncontrollable Drunkenness considered as a form of Mental Disorder. By FORBES WINSLOW, M.D., D.C.L. London: Robert Hardwicke: 1866: Pp. 54.

THIS pamphlet, like most of Dr Winslow's writings, is of a semi-popular and semi-medical character. It contains nothing new in reference to drunkenness as a form of mental disorder. The description of the malady is sufficiently striking and graphic, and illustrated by a few well-marked examples.

Although recognising it as a form of mental disease, Dr Winslow says, "It would be the height of folly to treat any but extreme and exceptional cases of this kind as *insane persons*, and confine them as *lunatics* in asylums." For this he gives two reasons, first, that in such cases it would be difficult to get the requisite medical certificates, as the insanity shows itself in the conduct of the individual, and not in his *ideas;* there are no delusions to cite in proof of his insanity. Secondly, he says he could not be kept long enough in an asylum to insure his cure after he had been admitted.

For these reasons, and for a third, namely, that it is desirable in most cases of this kind, for the future welfare and interests of the individual and his family to avoid the imputation of *insanity,* Dr Winslow advocates the erection of *Sanatoria* for inebriates of this kind. He thinks that many such persons would go to such institutions voluntarily, and remain in them till cured; and for those who would not go of their own free will, he recommends that a Court of Inquiry should be established, authorized by law, to seclude such persons in a Sanatorium.

This, we think, is the essence of Dr Winslow's pamphlet. In reference to the several points of importance involved, we may briefly remark, first, that if this is a mental disorder we do not see why it cannot be certified as such by medical men, as well as any other mental disorder where there are no delusions, as happens not unfrequently in cases of suicidal and homicidal mania, melancholia, and even acute mania. Secondly, that such cases are, in point of fact, to be met with in all our large asylums in England, Scotland, and Ireland. Thirdly, that although their detention in asylums may not be long enough to effect a cure on a first attack, out of deference to the opinions of those who consider with the English Commissioners, "that a lunatic asylum is not a place for the *permanent* detention of persons who have recovered the use of their reason, and are not obnoxious to the charge of unsoundness of mind, otherwise than on account of the liability to run into their former excesses when restored to liberty," yet, it may be added, that both the English and Irish Commissioners have stated that they are very slow to recommend the liberation of such persons sent to an asylum for a second or third attack.

In reference to the third objection to asylum treatment, we admit that it would be very desirable, if such cases could be sent to any other places than asylums, both for their own interests, and for the interests and comfort of the asylums, where they are generally regarded as great sources of discontent and discomfort. But where are these other places, these sanatoria, to be found?

Were they established and legalized in the manner advocated by Dr Winslow, we question very much whether the patients or their friends would not prefer the odium of an asylum to that of an *inebriate institution.*

The only places other than asylums which we at present possess for the treatment of such cases, are the houses of clergymen or doctors living in remote districts, far removed, if possible, from public-houses and gin-palaces, in regions where credit is unknown, and hard cash is the only means of procuring the poison. And of all such places, we prefer, like Sancho Panza, an *island,* where our patients not only can get no credit, but where they cannot walk away till their cure is supposed to be completed.

We fear the chances of legislation for this class of patients is small, and at present we must make the best use of the means we have for the sake of rescuing from ruin, if possible, those who cannot save themselves, and many of whom might be the sources of joy, and pride, and comfort, not only to their own friends, but in not a few instances to society at large.

On Abscess and Tumours of the Orbit. Part I. By SPENCER WATSON, F.R.C.S.Eng., Assistant-Surgeon King's College, and the Central London Ophthalmic Hospitals. London: H. K. Lewis: 1866.

THIS pamphlet is a reprint of some papers that appeared in the *Medical Mirror.*

Abscess of the orbit first comes under consideration, and the author details in succession the causes, diagnosis, prognosis, treatment, and pathology, of this affection. It is to be regretted he did not commence by giving the symptoms of the disease, whereby the paper would have been rendered more complete, and the chapter on diagnosis more valuable. A few well-narrated cases are appended. The various forms of cysts that may occur in the orbit is the subject of the second chapter. The remarks on the diagnosis of these growths are worthy of attentive perusal.

The next part of his paper on vasculars and aneurismal growths, contains a very good resumé of what is known concerning these somewhat obscure affections, with a table (somewhat incomplete) of reported cases in which ligature of the carotid, or other similar means of treatment, had been adopted.

The last chapter contains a brief description of effusions of blood, air, and serum in the orbit.

In several places, this paper bears traces of having been somewhat hurriedly written, the author's meaning being here and there rather obscure.

We have much pleasure in welcoming this as a contribution to our literature on these rare affections. In Part II. the author will have a wide scope in treating of the solid growths that may occupy the orbital cavity.

On Inhalation as a Means of Local Treatment of the Organs of Respiration by Atomized Fluids and Gases. By HERMANN BEIGEL, M.D., L.R.C.P.L., Assistant-Physician to the Metropolitan Free Hospital, &c. &c. With Woodcuts. London: Hardwicke: 1866.

AFTER the notoriety which this method of treating diseases of the throat and chest has lately attained, in connexion with the proceedings of "Dr Hunter of New York," there is a risk lest a remedy, which in some suitable cases may be really useful, should be laid aside as having been abused. This work by Dr Beigel may be of some service, by showing how inhalation may be used, and in what cases it is useful. The work is divided into two parts. The first, on inhalation in general, commences with an historical account of the introduction of inhalation, from the days of Hippocrates, who may be called the grandfather of this method, to Sales-Giron, who certainly deserves the credit of being the father of the inhalation of atomized fluids. Experiments and cases are then cited which prove the possibility of the entrance of atomized fluids into the trachea and bronchi. The various apparatus which have been invented are then described by the aid of woodcuts, and the methods, medicaments, and arrangements, for private and dispensary practice, are very fully detailed.

In Part II., after a statistical report on the immense prevalence of diseases of the respiratory organs, a pretty large series of cases is given, including diseases of larynx, trachea, bronchi, and lungs. Some of these are examples of successful, others of unsuccessful, treatment; but all very fairly fulfil the author's intention, which was, as he tells us in a concluding paragraph, "to show that inhalation is a resource by no means to be neglected in diseases of the organs of respiration. He who expects wonders from that mode of treatment will soon be disappointed: he who recommends it as an infallible one, will prove a false prophet; but an unprejudiced application of the atomizer will lead to the conviction that the invention of Sales-Giron has been a most valuable addition to therapeutics."

From some peculiarities in the style and diction, there can be little doubt that our author has learned English as a foreign language. His descriptions, however, are always intelligible. The type is bold and clear, and the general appearance of the work reflects credit on its publisher.

Osteology: A Concise Description of the Human Skeleton, adapted for the use of Students in Medicine; accompanied by an Explanatory Atlas of Plates. By ARTHUR TREHERN NORTON, Assistant Lecturer on and Demonstrator of Anatomy, St Mary's Medical School. London: Robert Hardwicke: 1866.

TWO neat little volumes, one containing 126 pages of letterpress, and the other 20 plates of the bones, with explanatory references facing each page, are intended to "free the subject of osteology of the unattractive character so frequently complained of by the young beginner," and to aid his studies. While Mr Norton, as a teacher of anatomy, must naturally be supposed to know what students want, we really can hardly understand for what class of men the little book is written, and the plates prepared. Those who are merely cramming for a pass, will find all they need in Gray or Quain, described at least as intelligibly, and delineated quite as clearly; while the more diligent ones, who really want to study the subject fully, will find far more in the classic Ward, or in

Professor Humphrey's most valuable monograph. Those who want beautiful plates, a little poetry, and a mild flavouring of comparative anatomy, will find these more luxuriously supplied them in Holden on the Bones.

The descriptive letterpress has certainly the merit of possessing no obtrusive originality, and is fairly clear and concise, but uninteresting from its baldness, and from the absence of the faintest hint of any physiological or surgical relations. The plates are elaborate and highly finished, but boldness of outline is in some places sacrificed to shading. The references are convenient, being placed always on the opposite page to the plate, so as to be seen at a glance, and thus to avoid any trouble in turning over pages. While creditable to the industry of the author and his artist, we fear the sale of this pretty little work is not likely to prove remunerative to its enterprising publisher.

Medical Students of the Period. By R. TEMPLE WRIGHT, M.D., late Scholar of King's College, London. Edinburgh: Blackwood & Sons: 1867.

THAT medical students had at one time a very indifferent reputation is a matter of fact; and that too many of them deserved it, cannot we fear be denied; but a great change for the better has taken place, and students of medicine need not now shrink from a comparison with the aspirants to any other liberal profession. Still, this change has not been fully realized out of the profession, and, misled by amusing but exaggerated accounts of the doings of the youthful sawbones, the public generally believe that medical students as a class are wilder and more dissipated than their neighbours. It is to defend them from this imputation, and to represent them in their true colours that Dr Wright now comes before us. Dr Wright is well qualified for the task he has undertaken; he has a familiar acquaintance with college life at Cambridge, London, and Edinburgh; he has unbounded enthusiasm for his profession; while his style of writing is easy and agreeable, and numerous well-told anecdotes serve to enliven the graver portions of his work. The book is principally intended for the general public, but will, we are sure, be read with interest by our own profession; by the juniors, as describing scenes through which they have lately passed; by the seniors, as showing what changes have taken place, and what improvements have been introduced, since they occupied the benches of the lecture-room, or thronged the hospital ward. Among the general public "Medical Students of the Period" should be at least equally popular; it gives information on many points regarding which the greatest ignorance prevails; while the hearty, genial tone of the writer, cannot but prejudice them in favour of a class of which the author proves himself so good a representative. We cannot follow Dr Wright as he conducts his readers to the lecture-theatre, the dissecting-room, the chemical laboratory, the hospital; neither can we accompany him to the medical society; nor, greatest deprivation of all, can we spend a social evening with him, enlivened by appropriate songs. To all our friends, medical and non-medical, we would say, read Dr Wright's book, and we are sure that any prejudice you entertained towards medical students will be effectually and agreeably dissipated. We conclude with two short extracts as specimens of the style of the volume.

Speaking of the abuse of charity on the part of those who are quite fit to pay for medical advice, Dr Wright says—

"There is one fault that should be noticed common to all out-patients, whether surgical or medical; far too many men and women are for weeks and months out-patients at hospitals who could very well afford to pay for advice and medicine, if we may judge by the glossy cloth coats and the showy silks they wear. This is a most unjustifiable abuse of charity; but it would soon cease if every doctor were as independent and courageous as one eminent physician, who, when *he* had care of out-patients, used to astonish any smart woman whom he saw enter the room by calling her to the front of all the others, and catechising her in this strain:—

"'Pray, ma'am what is your husband's employment?'

"'A mechanic, sir—a skilled mechanic.' (That is the favourite answer.)

"'And may I inquire what his wages are?'

"'Three guineas a-week, sir.' (Sometimes it is even more.)

"'Just what I expected; more than a hundred and sixty pounds a-year, you see! Are you aware that an immense number of clergymen and other gentlemen have less than £160 a-year? Yet what would you say if the clergymen's wives were found coming to the hospital? Poor dear ladies, if they *did* come, I am sure they could not find all those smart silks that *you* come rustling in.' Here they generally beat an ignominious retreat, at which—'Yes, you may well retire; but you must not go to some other hospital.'

"He would then turn to the students present, and remark, 'But she *will* go, gentlemen, for all that, and her husband will consider she was very ill-used because we would not admit her here. Women of this stamp crowd into the maternity hospitals, and fleece those charities to even a greater extent than they do us, not with the connivance only, but also with the approbation of their husbands. And these knaves, gentlemen,' the stanch old Tory would add, 'are the model "working-men" of that pugnacious "friend" of theirs, the vociferous member for Birmingham!'"

The following little scene occurs at the Children's Hospital:—

"'Well, Willie, what are you so disconsolate about this morning? Haven't you had any breakfast?'

"'Oh! 'essir—only full diet take all my apple-tite away! I can 't eat any more,' and this in a most piteous tone.

"'Yes, sir,' says little Edie, backing up her friend Willie; 'he couldn 't eat all his bread and butter when he had full diet brought him this mornin'; but oh! he had such a *drink-etite!* He finished all his own milk, and I gave him some of mine, and yet he was dry!'

"Their little remarks are often very amusing. The sister in charge of the children's ward had a bad headache for a day or two, and wee Robin, a golden-haired little urchin, was quite concerned about her; he came up to her as she lay on a couch, and stroked her hand, then 'Oh! sister,' he said, 'I can 't bear to see you so ill; why don 't you get well? I 'll tell you what you should do. *You ought to have a baby; mother always gets well as soon as she 's had a new baby.*'"

Part Third.

MEETINGS OF SOCIETIES.

PROCEEDINGS OF THE EDINBURGH OBSTETRICAL SOCIETY.

SESSION XXVI.—MEETING III.

12*th December* 1866.—Dr Charles Bell, *Vice-President*, in the Chair.

The following gentlemen were admitted as Fellows of the Society:—Dr Baker, Liverpool, as a corresponding Fellow, and Mr S. Stirling, Edinburgh, as an ordinary Fellow.

I. ON THE COMPARATIVE VALUE OF THE LONG FORCEPS, AND TURNING IN CASES OF CONTRACTED BRIM, WITH CASES. BY DR MILNE.

The paper will be found at p. 798 of this number of the Journal.

Dr Keiller thought the Society was indebted to Dr Milne for the great care he had evidently bestowed on the excellent paper now read. The subject of the communication was well worthy of the Society's studious attention; and the able manner in which the various points referred to had been treated by Dr Milne very much enhanced the value of his interesting and most practical paper. The questions mooted offered ample scope for discussion, but the most obvious one calling for special remark was that involved in the giving a preference to the operation of turning in most cases in which the long forceps are used. He (Dr Keiller) did not hesitate to state, that in the main he concurred in the views entertained by Dr Milne; and having from past experience come to look on the long forceps, when incautiously used, as a dangerous implement, and having on the other hand witnessed the most ample evidence of the comparative advantages and safety of having recourse to version in many cases of labour obstructed from pelvic deficiency, he (Dr K.) had good reason to coincide generally in the reasoning and conclusions adopted in the present paper. There were several grounds, however, on which the occasional use of the long forceps would be well founded, and although he (Dr Keiller) felt strongly impressed with the general soundness of Dr Milne's arguments, still there remained much to be said in favour of the forceps if properly and cautiously applied and used in proper cases. Dr Keiller mentioned the character of cases which he considered proper turning cases, and contrasted them with those suited for the forceps.

Dr Inglis agreed with Dr Milne, that we could deliver by turning where the long forceps could not suit; and yet it would be impossible to give up the use of the long forceps. Dr Inglis mentioned one case—a primipara—where long forceps failed; the child was turned, and both legs and one arm were got down—but there it remained, and the head had to be opened. Dr Inglis referred to an excellent paper recently furnished by Sir James Simpson, where he, Sir James, advocated turning in place of craniotomy.

II. CASE OF ACCIDENTAL HÆMORRHAGE. BY A. MACDONALD, M.D.

Mrs Gow, residing in White Horse Close, Canongate, was seized, on the 12th November 1866, while at dinner about four o'clock in the afternoon, with a sudden pain in the belly. She had not been exposed previously to any extraordinary fatigue, and had no idea of any cause for the pain. The only work she was in the habit of performing, likely to call forth any great amount of physical exertion, was carrying cinders from the adjoining gasworks, which she used for fuel. But she was unable to trace any connexion between the appearance of the pain and this employment. She expected to be confined about this time, and accordingly went to bed, and after waiting about two hours, that is till six o'clock, she sent for medical assistance. A student in connexion with the New Town Dispensary arrived about seven o'clock, when he found her suffering from a constant pain, or, more accurately, excessive tenderness over the lower part of the belly immediately above the pubes. He remained with her for twenty minutes, learnt that it was her fourth child, that her previous labours had been easy, and her general health good, that her pulse, though rather quick and sharp, was otherwise good. He waited for some time for the accession of a regular pain, but none occurring, he proceeded to make a vaginal examination. His finger reached with great difficulty the os uteri, which had barely begun to dilate. The undilated os, the persistent tenderness above the pubes, and the fact that the patient had slept very little for several nights previously, suggested to him the propriety of administering an opiate. He accordingly ordered—℞ Tincturæ opii, m.xx.; Aquæ camphoræ, ℥i.—Sig. The whole to be taken immediately. He visited the patient a second time about nine o'clock, when he found her in a very restless state. The tenderness still continued, and there were no proper uterine pains. But the os uteri had now dilated to about the size of a shilling piece, and on the hand being withdrawn there was found upon it a little arterial blood. As there had been some uterine contraction, as evidenced by the dilatation of the os uteri, such a slight amount of hæmorrhage did not suggest any cause for alarm, and the patient was again left for a couple of hours. Between eleven and twelve o'clock another visit was made, and it was discovered that there had been hæmorrhage to such an amount as to soak through the bed-clothes, and form a large pool of blood upon the floor. The patient's pulse was thready, and the ordinary signs of severe hæmorrhage present. The os uteri was found to have made some progress, but, while there was still constant pain complained of, there were no intermittent pains. The student then sent for me, as I happened to be doing duty for Dr Andrew Inglis during his absence from town. I arrived at Canongate about one o'clock A.M. In the meantime the patient had had two teaspoonfuls of the liquor ergotæ administered to her, and the vagina plugged, with the result of completely arresting all external hæmorrhage. On my arrival I found the patient very restless, and her mind wandering a little. The pulse was quick (110–120), weak, but regular. The surface of the body was pale, and the extremities cold. The abdomen felt larger than usual. On auscultating I failed to hear any fœtal heart sounds. I immediately removed the plug from the vagina, and found there was some, but not great, hæmorrhage from the genital passages. The os uteri was about the size of half-a-crown. The cervix was thick, but soft and dilatable. The uterine action was both regular and powerful. *No portion* of the placenta could be reached by the finger per vaginam. As I was unwilling that the woman should suffer from even the small amount of hæmorrhage that still remained, and since from the absence of fœtal heart sounds, as well as from the large separation of the placenta from the uterus, which had evidently taken place, I believed the child was already dead, I ruptured the membranes, after which the bleeding entirely stopt for the whole of the labour, which was rapid, the child being born dead about four o'clock A.M. After the head had escaped from the vulva, the shoulders were found wedged into the cervix in a peculiar manner, requiring a very unusual degree of force to bring them through the cervix, after I had waited for some time to see if the uterine action would itself conclude the labour. The placenta came away immediately after the expulsion of the fœtus. It seemed perfectly healthy, but the whole of its attached surface was covered with clots, showing that it had been completely separated for some time. After the removal of the placenta, the uterus was found to contain large masses of clots, which were very tenacious, and had evidently been there for some hours. After clearing out these, the uterus contracted well. The patient had another dose of ergot administered to her, and a tight bandage applied. She was ordered to have beef-tea, and small doses of stimulants, with other nourishing food; and if there were no recurrence of hæmorrhage in four hours, to have a grain of opium every four hours. There was no recurrence of the hæmorrhage. The patient made a good, though slow, recovery.

Remarks.—I ought almost to apologize for troubling the Society with his simple case. But if so, I must plead as my excuse, that I am anxious to rekindle a discussion of the subject of uterine hæmorrhage, from overhearing which I derived a great deal of instruction last year, and that the cases of accidental hæmorrhage are comparatively rare. I think, also, that in this case there are one or two points of special interest. It also proves the correctness of the rules of practice usually advocated for meeting this accident.

The first point to which I would advert is the *entire absence* of all known cause for the separation of the placenta. Though carefully interrogated, the patient could remember no extra effort or exertion with which she could connect the pain which supervened while she was quietly seated at dinner, and which after events proved to have been due to separation of the placenta from the uterus. It is true that the patient was a hard-working woman, surrounded with none of the amenities which lessen the troubles of the pregnant state. But in this respect she was no worse off than thousands of women occupying the same station in life. Though she was in the habit of carrying the cinders from the gaswork, she could not recollect having made any special exertion on that day, nor did she even recollect that circumstance at all till closely pressed by cross-questioning. She was, besides, a healthy woman; the child looked healthy, and was well nourished; while the placenta was also perfectly healthy. Is it possible, I would ask, or is it consistent with the experience of any of the members of the Society, that partial or spurious uterine contractions could of themselves effect complete separation of the placenta? The contractions causing the rupture in this case could scarcely have been general, as at the time when the rupture (as evinced by the pain) commenced the os uteri remained unaffected. I should be sorry to think this result a possible one, as it would be a source of anxiety during the latter part of pregnancy, against which it would be impossible to guard.

The next point is the difficulty of arriving at a true diagnosis at first, and the liability of falling into a mistaken diagnosis, which in truth took place in this very case. I confess, however, I cannot well see how it could have been avoided. That it was simply a case of spurious pains was by far the most natural conclusion to come to in the circumstances, and the treatment by an opiate followed as a matter of course, though, as events showed, it produced disastrous results. But how can we guard against the recurrence of such an accident? This I am utterly unable to answer; for, at the time when the rupture evidently took place, there was not a drop of blood externally. It was not possible to tell, from the appearance of the abdomen externally, that any alteration in its shape was due to hæmorrhage, as the student was unable to compare its appearance when he saw it with its appearance previously to the rupture, not having had an opportnnity of seeing it before. Even afterwards, when there was a little external hæmorrhage, it was so trifling, that the attendant could scarcely be blamed for overlooking it. The pain was much more naturally referable to spurious contractions than to placental rupture. One thing it teaches us, however, and that is the necessity of extreme caution in the exhibition of opium to patients in the parturient condition.

Lastly, the case shows the correctness of the ordinary rules of treatment for such cases. The vaginal plug could do no possible good, but might do much mischief by leading to increase of internal hæmorrhage, and I therefore removed it at once. But the ergot of rye and the rupturing of the membranes, whether they act by pressing the child against the bleeding uterine surface, or by contracting the uterine fibres around the placental arteries and veins, evidently saved the woman's life. Having reason to believe the child was dead, there was no necessity for abstaining from rupturing the membranes. Turning, at the time I first saw the patient, was out of the question, from the small amount of dilatation of the os uteri. The same was true of forceps. It is worthy of remark, that in this case, as indeed in almost all similar cases, the dilatation of the os went on very rapidly.

The difficulty of getting the shoulders through the os seems to me to have been occasioned by the child being wedged in or impacted among the tough clots which were found to fill the uterus.

A somewhat desultory conversation followed,

Dr Charles Bell said, it very frequently happened, that some delay occurred after the head was born, and it was necessary to assist the shoulders to make the turn into the outlet; and to use traction by means of the fingers inserted in the axilla. Sometimes Dr Bell had seen twenty minutes elapse between the birth of the head and shoulders, during which the child cried lustily.

Dr Milne considered opium dangerous in such cases as those referred to by Dr Macdonald, as it caused relaxation of the uterus.

Dr Sidey was in the habit of giving opium in place of brandy.

Dr Macdonald urged the use of ergot to cause contraction of the uterus, and opium might be given some hours afterwards.

Dr Wilson approved of opium as a stimulant some hours after hæmorrhage had ceased.

SESSION XXVI.—MEETING IV.

January 9, 1867.—Dr CHARLES BELL, *Vice-President*, in the Chair.

Dr Miller, 48 Albany Street, and Dr Rosa, George Street, were admitted as Ordinary Fellows of the Society.

I. CASE OF CONGENITAL CYSTIC TUMOUR. BY DR CAPPIE.

On the 11th October last, I attended Mrs S. in her first confinement. She commenced to feel unwell in the early part of the afternoon; during the second stage of labour the pains were unusually strong; and, although the outlet of the pelvis appeared rather contracted, delivery was completed in the evening shortly after seven o'clock. When the head of the child was born, I, as usual, tried to make my finger run along under the chin, to make sure that the mouth was free. On the present occasion, however, I found it was impossible to do so. Feeling certain that the face was born, I raised the bedclothes to ascertain what was wrong. From the appearance which then presented itself, the immediate impression made on my mind was that the child about to be born would have no neck. The skin of the face was continued downwards on a level with the chin, and I was prepared to expect some malformation. As the pains continued strongly forcing, the delay in the body being born was not considerable. I then found that the appearance which had puzzled me was occasioned by a tumour, which extended downwards from the chin and left side of the neck, covering the whole region of the neck and upper part of the chest and left shoulder. Its shape was conical. Its circumference at its attachment above the sternum was thirteen inches, and from the tip of the chin to the extremity of the tumour measured five inches. The left side of the face was distorted from the swelling on that side extending from the ear forward on the cheek. All round the base of the tumour it had a doughy feeling, and large varicose veins were seen under the skin. The skin over the body of the tumour was very vascular, tense, and shining like that over a ripe abscess. Towards its extremity the bright red gradually merged into a very dark colour. At the very tip of the conical swelling the skin had a blistered appearance, and showed a tendency to ulcerate. The whole tumour had the appearance of an extremely vascular swelling, or of a bag tensely filled with fluid and threatening to burst. In other respects the child, which was a female, was well formed and appeared healthy. It breathed freely; its cry was natural, and it readily swallowed what fluid was given to it. For the first two days there was no difference on the appearance of the tumour. The ulcerated surface then became sloughy, and every day I expected it would break, though what was likely to come out of it I could not tell. About the eighth day, on the dressing being changed, about twelve ounces of fluid were discharged from the ulcerated surface. The fluid resembled venous blood, but did not coagulate. The tumour then became greatly more flaccid in appearance, but the size was still great. The drawing I now show to the Society, and which was made under the directions of Mr Turner of the University, was made after the discharge had taken place. The child every day after that became weaker, and for two days before its death, which took place on the 29th of the month, the breathing was distressed and laborious.

A post-mortem examination was made by Mr Turner. The tumour and adjoining parts were taken away and carefully dissected by Dr Chiene, Mr Turner's assistant, and the following are his notes of the dissection: "*General Position.*—The tumour occupied the whole of the left side of the neck, from the ear to the clavicle; it crossed the middle line, filling up the anterior triangle on the right side. The skin over the tumour was adherent and natural in appearance, except at the most dependent part—above the left clavicle,—where it was attenuated, and of a bluish purple colour. Here the skin had given way, leaving an ulcerated opening, the size of a shilling, from which a bloody fluid had been discharged during life. The tumour was carefully dissected out, only a small portion which dipped behind the manubrium of the sternum being left. The lower jaw, tongue, and larynx, with the adjacent parts, were removed along with the tumour. *Appearance on Dissection.*—The tumour consisted of numerous cysts infiltrated among the tissues. No appearance of a common investing capsule could be discovered. The thyroid and sub-maxillary glands were overlaid by the tumour, but were not continuous with it. The lymphatic glands could not be detected, and the boundaries of the left parotid could not be defined, as the tumour extended upwards into its region, and seemed to be intimately blended with it. *Cysts.—Size.*—They varied in size from a pin's head to a small orange. The larger ones were situated to the left side, over the ulcerated opening already mentioned. *Walls.*—Thin and membranous, in some instances imperfect, so that the cavities of adjoining cysts communicated freely with each other. *Contents.*—For the most part they contained a bloody fluid; many of the smaller cysts had a serous or oily fluid; in a few instances grumous matter escaped on opening them. *Probable Origin.*—To all appearance this tumour may be considered as having taken its origin in the areolar tissue of the neck."

For these notes, and for the drawing now exhibited, I have to express obligation to Mr Turner and to Dr Chiene.

Dr Cappie said that the child sucked for two days, but only from one breast, in consequence of the tumour.

Dr Keiller said that such cases before the child was born might be mistaken for congenital goitre. He had a case where, after the head was born, a tumour was felt lying on the perinæum impeding the labour, but which, after expulsion, was found to be a large goitre, filling the space between the chin and chest. The tumour gradually disappeared, and there only remains a nodule, hardly recognisable. The boy is now twelve years old. The surface of the skin was not discoloured, and therefore altogether different from the case mentioned by Dr Cappie. Infantile goitre is very rare, but cases such as Dr Cappie's are still more so. Dr K. stated that Dr Alexander Simpson had lately seen in Glasgow a similar case of congenital goitre, which he (Dr Keiller) thought would in the same way ultimately disappear.

Dr Cappie said that Mr Turner had never seen a similar case before.

II. CASE OF IMPERFORATE ANUS. BY DR LITTLE OF SINGAPORE. COMMUNICATED BY DR KEILLER.

Dr Keiller read the following account of a singular case of imperforate anus sent to him by Dr Little of Singapore.—

Singapore, 3d October 1866.

My dear Keiller,—A very remarkable case in your line having come under my observation the other day, tempts me to write you a few lines on the subject. About three weeks ago, a native of India, a Kling from the coast of Coromandel, brought me a child to look at. The child was attenuated to almost a skeleton—the limbs seemed to have no muscular fibre, the face was pinched, only the muscles about the mouth were more developed, which, with other appearances, gave the child a remarkable resemblance to a young ourang-outang reduced by dysentery; the chest had merely a covering of skin, but the abdomen was enormously enlarged, and hard as a stone—quite dull on percussion over its whole extent, and had no part impressible—its circumference round the navel was twenty-four inches, while the chest, loosely measured, was only fourteen inches. The child was fourteen months old, and had never *defecated* from its birth; indeed, it could not; for the most thorough examination could detect no anus. The place where the anus ought to have been was marked by a darkening, and a slight puckering of the skin. The most careful examination by means of a fine platina wire tipped with wax failed to discover the smallest orifice; indeed, the eye could see there was none. The genital organs, at least the external parts, were more developed than usual with a child of that age, and *no* opening like a recto-vaginal fistula could be detected. The parents said the child had never passed any fæcal matter; but I saw the mother wipe a sebaceous-looking secretion from the inside of the vulva, which she said was all the child ever passed, and which she thought was from the bowels; the matter was white, sebaceous-looking, and without smell, and very small in quantity. The child micturated, and seldom or never vomited. It subsisted entirely on its mother's milk, and whenever the child cried I saw her giving it the breast. The alveolar processes were imperfectly developed and showed no appearance of teeth. The rectum, apparently, did not extend down to the anus, or where that should have been, for there was no fulness, no protrusion, but rather an emptiness inside the tuberosities of the ischium. The child was born with a large abdomen, and the mother had several healthy children before. I desired the parents to come to the dispensary that I might show it to my partner, Dr Robertson; they came, but he was not in; but Mr R. James, the manager of the dispensary, an intelligent and acute observer, saw the child with me. I asked the parents to bring the child next day, that I might have it photographed,—for as to making an artificial anus in its state was useless; but they did not return. A fortnight after, the father came with another patient, who had fractured the femur; and I learned, to my regret, that the day after I saw the child it had died. Now, in all your experience, or that of your confreres, have you ever heard of a child living for anything like fourteen months without a fæcal opening of some kind or another? Such a case could not have happened in any civilized country; for shortly after birth an artificial anus would have been made by the practitioner, and the child would either have survived, defecating through the artificial opening, or died. But here all fæcal matter was retained; and how can you reconcile that, with the existence of life for fourteen months. I will not enter the region of speculation, but leave it to the members of the Society, to whom you may, if inclined, communicate the case; and I trust you will be able to attest the veracity of your old friend, R. LITTLE.

Dr Keiller remarked that he had given the case, as reported to him by his old friend and fellow-student, Dr Little, who was evidently under the impression that there had been no fæcal discharge of any kind from the birth to the death of the child. It was an exceedingly interesting case, and must be considered unique in its character, provided the fact of there having been no outlet whatever for the usual excrementitious results of digestion had been established by post-mortem examination. There did not, however, appear any way of accounting for the child's surviving so long without defecation, unless by considering that the sebaceous-looking discharge, mentioned by Dr Little as having been occasionally wiped from the child's vulva by its mother, had come from a cloacal or congenital aperture between the undeveloped rectum and vaginal canal. There did not, at least, seem to be any other outlet; there was no vomiting, which, sooner or later, usually takes place in cases of imperforate anus where no intestinal outlet elsewhere exists, and without which it is difficult to explain the survival of the child apart from the explanation afforded by the occurrence of discharge, such as that referred to by Dr Little. He (Dr K.) had come to the conclusion that there had existed some congenital recto-vaginal outlet, in this case, through which the fluid excrement was expelled, and that (however unlike ordinary fæcal matter the discharge observed to be occasionally wiped from the passages appeared to be) the function of defecation was in all probability in this way carried on. This opinion was entirely corroborated by a somewhat similar case which he (Dr K.) treated many years ago when practising in Dundee, the particulars of which, along with other cases, he had more than once mentioned when the subject of imperforate anus and its treatment had been discussed in this Society. It would be interesting to have a correct history of such cases, and especially of the cases treated by operative procedure, with their results. On the present occasion, the case observed by Dr Little was the one for the Society to theorize on and to discuss; and beyond briefly referring to a few of the cases of imperforate anus which had come under his (Dr K.'s) immediate care, and which had some reference to the case he had just communicated, he would defer a more minute description until some future occasion. Dr K. stated that he had operated successfully, where the rectum was within one inch of the anal surface. The difficulty generally experienced is to keep the artificial anus open. If the bowel can be reached, it must be pulled down, and stitched to the skin. Three cases occurred while he practised in Dundee. In the first, the child had no anus, and passed the fæces through the urethra; an operation was attempted, and for a time the secretions came away from the wound, but it contracted and closed, and the child died. At the post-mortem the bowel was found to terminate in the bladder. The second case was like Dr Little's. The patient was a girl, seven years old, where there was no anus, and where the alvine secretions were passed per vaginam. When Dr K. saw her, he operated in the usual manner, and tried to keep it open. The child wore silver tubes and passed the fæces through them. The wound always contracted and closed up. She is still alive, and now upwards of twenty years of age. Dr K. thought this case similar to the one mentioned by Dr Little, where fluid fæces was passed by the vagina.

Dr Cochrane agreed with Dr Keiller as to the nature of Dr Little's case.

Professor Macdonald, of St Andrews, spoke of the difficulties attending the operation for imperforate anus, especially in securing the end of the bowel, which is generally found to run into the vagina. He (Prof. M'D.) suggested that an opening should be made over the sigmoid flexure, in which opinion Dr Charles Bell coincided. Dr Keiller mentioned a case where the bowel terminated at the back of the bladder after traversing the pelvis twice. This was found on dissection, the child having died from obstruction of the bowels.

Dr A. Macdonald had seen a case which terminated in perforation of the bowel from inflammation.

Dr Charles Bell said that imperforate anus was rare, as it occurred on the Continent only once in 25,000 children, and Collins met with only one case during his mastership, although 16,654 children came under his observation. In regard to Dr Little's patient, he (Dr B.) believed that the fæcal discharge must have come away gradually per vaginam or bladder, otherwise, there would have been distention. There was no case on record of a child living much beyond seven weeks. There were three kinds of imperforate anus. The most common was that where the bowel terminated in the urethra, bladder, or vagina, of which the case just related seemed to be an example. The next in frequency was that in which the rectum terminated high up in a cul de sac, in which Amussat's operation might be adopted with advantage. The most rare is when the anus is occluded by a membrane or cohesion of the walls of the rectum.

Dr Bruce stated he had seen two cases. In one, the child was small and puny, but was successfully treated by operation and by the use of bougies, and lived a year, death occurring from another cause. The second was also operated on, but died soon after.

DUNDEE MEDICAL SOCIETY.

At the monthly meeting of the Society, held on Wednesday, 6th February,

Dr W. Crockatt read a report of a very interesting case of disease of the spinal cord.

Dr J. W. Miller reported a curious case of enteric fever. When first seen (Jan. 25), the patient, a tall, rather unhealthy-looking man, had been feeling weak and unable for his work for some weeks, but had, nevertheless, continued at his employment until that day. All that he complained of was debility and a slight cough. There was dulness of the percussion note, but no moist rales, at the apex of the left lung anteriorly; and beyond this no symptom of disease could be detected anywhere. Four days after this, he became very ill early in the morning, as his wife described it, " he went from one faint into another." In the course of the day, he became feverish, the pulse being rapid and full. The bowels had moved once on the previous day, the stool being dark-coloured. He was ordered ℥ij. of castor oil. Next day (Jan. 30), Dr M. was called to see him at 8 A.M. The patient had had very little sleep during the night, and had been very delirious, but was now quite intelligent; he was bathed in perspiration, his countenance very pallid, the pulse about 160, and scarcely perceptible. The bowels had moved twice on the preceding afternoon, and a third time early that morning, each stool being described as having consisted entirely of coagulated blood. A fourth stool had been passed just as Dr M. entered, which he saw, and which consisted entirely of blood, apparently not much less than a pint. There was no tenderness or other sign of abdominal disease, and no eruption on the skin, except that of croton oil on the chest. The cardiac sounds were healthy. The area of hepatic dulness was normal. He was ordered five grains of plumb. acet., with compound ipec. powder every two hours, until six doses had been taken; also brandy in large doses. The same afternoon he seemed to be dying, but subsequently rallied. Three days later (Feb. 2),—his state in the meantime having been one of feverishness, with much delirium, and frequent attempts to rise from bed,—his pulse was 120, of good volume, but very compressible. He was intelligent when addressed. He had had no stool since the 30th, bloody or otherwise. The tongue was dry and brownish. As the patient had all the look of one advanced in enteric fever, the skin was again examined carefully for eruption, and a few rose-coloured spots were found on the abdomen, but by no means very characteristic in their appearance. Still, considering the history of the case, the sudden and early occurrence of the violent hæmorrhage (five days after the patient ceased work), the absence of all symptoms of pyrexia when first seen, and the absence of diarrhœa and abdominal tenderness, Dr M. had considered such a diagnosis an improbable one. In the evening the patient was much worse, there being extreme prostration, much delirium, and constant floccitatio; and, notwithstanding the free use of stimulants, he died next day, ten days after he had been at work.

On the following day Dr M., with Drs Dewar and Anderson, examined the body. The rectum and sigmoid flexure were full of decomposing blood; their coats being deeply stained by the blood, but otherwise healthy. The lower end of the ileum was much congested, and its mucous surface presented the ulcerations of Peyer's patches characteristic of enteric fever.

Part Fourth.

PERISCOPE.

MEDICINE.

PROFESSOR SKODA ON CHRONIC GASTRIC ULCER.

THE following is an extract from a clinical lecture in the *Allg. Wien. Med. Ztg.*, 1865, Nos. 40, 41, and 43:—

The chronic gastric ulcer, which, on account of its characteristic form and peculiar course is designated as *Ulcus rotundum* or *perforans*, was not known to the older physicians; at least, they had no thorough knowledge of it, but confounded it generally with other morbid processes. The chief seat of it is at the lesser curvature, and specially in the pyloric portion of the stomach, chiefly in the posterior wall, more rarely in the larger curvature and at the cardia. In very rare cases it occurs external to the stomach, *e.g.*, in the duodenum or lowest part of the œsophagus. The characteristic features of the ulcer are, its circular form and its tendency to spread destructively in depth, and to extend to all the strata of the gastric parietes. The process of destruction always commences in the mucous membrane, and is confined to it in a large number of cases. We find accordingly, not unfrequently, in bodies the traces of a previous chronic ulcer, and the healing takes place, as in all other ulcerations, by means of new formation of connective tissue at the base of the ulcer, by which its edges are drawn towards each other and finally unite. In proportion to the amount of loss of substance will be the greatness of the constriction and shortening or deformity of the stomach, and the consequences may be, both a narrowing of the pyloric half, and also a considerable interference with the movements of the organ. But if the ulcer progresses, it then frequently leads to perforation, and by the escape of the contents of the stomach gives rise to general and usually fatal peritonitis. In respect of extent and size of the ulcers, numerous gradations occur, and the form of the stomach is still more irregular when several ulcers become confluent. The causes of chronic gastric ulcer are not sufficiently known, probably several factors concur in their production. We may assume as probable, that a partial disturbance of nutrition, due to disease of the bloodvessels, occasions a circumscribed gangrenous destruction of the mucous membrane. The hypothesis, that an altered condition of the gastric juice gives origin to the ulcer, appears to be unfounded; nevertheless, it cannot be denied that the movements of the stomach and the action of the gastric juice hinder the cicatrization and healing. Without doubt, similar ulcers occur on other mucous membranes, but, on the one hand, they are not followed by the same severe consequences to the organism as in the ulcer of the stomach; and, on the other hand, they heal much more easily. Under unfavourable circumstances, as has been mentioned, the ulcer ends in perforation of the stomach and fatal peritonitis; but this occurrence will not rarely be prevented by the circumstance that the base of the ulcer has adhered to the neighbouring parts. Such adhesions are found corresponding to the seat of the ulcer, more frequently between the stomach and pancreas or duodenum; and also with the left lobes of the liver, the anterior walls of the abdomen and the omentum, the spleen, the diaphragm, the colon, etc. If the loss of substance be small, and the adhesions to the neighbouring organs firm, life may be prolonged for a considerable time. But if the loss of substance be great, the function of the stomach will, in spite of the cicatrization, be much disordered, and the nutrition of the organism may in consequence suffer considerably. Besides, even with firm adhesions subsequent perforation may take place from softening of the false membrane.

The symptoms which accompany the ulcer of the stomach during life are very various. Sometimes for a long interval the symptoms are very insignificant or may be entirely absent; but for the most part, disorders of the functions of the stomach manifest themselves. Generally we observe a very painful sensation in the epigastrium of weight or drawing together; by pressure on the region of the stomach, a fixed, painful spot is detected. But these phenomena are also observable in chronic gastric catarrh, and the diseases are often undistinguishable. The appetite is usually more or less disturbed, occasionally unchanged, now and then increased. Yet the patients complain of slow digestion after meals, of pains, pyrosis, eructation, etc. As the disorder increases, retching and vomiting appear. The pain is generally fixed, but not always confined to the same spot. All the symptoms, it is evident, are not pathognomonic, and the physician is therefore, at an early period of the disease, not in a position to make a positive diagnosis. The hæmatemesis is of greater importance, and it is also one of the most dangerous symptoms, from its decided tendency to relapse. Vomiting of blood occurs with very varied intensity. The vomited matters are either only slightly tinged with blood, or are coloured chocolate brown, or like coffee grounds, the dark colour arising from the action of the gastric juice upon the blood effused into and detained for a long time in the stomach. Should a larger bloodvessel be eroded, the bleeding may be so abundant that death may immediately ensue, and, at all events, the highest degree of anæmia and exhaustion results. A feeling of weight and fulness of the epigastrium often precedes vomiting of blood. The hæmatemesis may take place at any period of the disease. The consequences of profuse hæmatemesis are the same which follow any other hæmorrhages: syncope, pallor, coldness of the extremities, feeble pulse, etc. Sometimes the hæmorrhage takes place without the vomiting, if a patient becomes pale after a sudden feeling of weight and heat in the epigastrium; and, on examination, the region of the stomach yields a hollow (?) percussion sound; if the pulse becomes feeble, and syncope comes on, from these symptoms we may conclude that a hæmorrhage has taken place. Such an internal bleeding may occasion death without vomiting. The bleeding generally occurs during digestion; bodily exertion, mental emotions may induce it, but especially any excitement of the circulation. Emetics also, for which the patient sometimes craves, may likewise occasion it.

In chronic gastric ulcers several stages may be distinguished. In the first the formation of the ulcer takes place, and requires considerable time for its completion. In this period the symptoms are the same with those of chronic catarrh of the stomach, with which, indeed, the formation of the ulcer is frequently accompanied. The pains present nothing characteristic; they may be continuous and fixed or paroxysmal, and may be mistaken for a nervous gastralgia. The occurrence of pains in the spine opposite the epigastrium is also not characteristic, being found in other stomach affections. Hence, in the earliest stage, ulcer of the stomach is very difficult to diagnose. Palpation reveals at most a fixed spot where pain is increased by pressure, and only in the case of persistent adhesions can we sometimes discover a certain induration. In the following stage vomiting of blood comes on, from which we are better able to decide on the nature of the disease, although this symptom also does not belong exclusively to ulcer of the stomach, but may appear in the course of carcinoma of the organ. Even during this stage, in spite of repeated hæmorrhage, the lesion may heal; still hæmatemesis must ever be regarded as a very grave symptom, because the bleeding itself may prove dangerous, and besides the bleeding always indicates a deeper ulceration in progress. Usually all the blood is not vomited, but a part of it passes off in an altered form with the stools, and sometimes the whole of the effused blood is so discharged when no vomiting occurs. In the third stage, perforation of the mucous membrane of the stomach at length ensues. When in consequence the contents of the stomach escape into the cavity of the peritoneum, a fatal peritonitis usually follows. This can only be averted in the case of slowl formed perforation by adhesions to the neighbouring parts, and sometimes these adhesions give way at a later period. If these adhesions are extensive, and give rise to a hardness perceptible to the touch, they may be confounded with carcinoma. Occasionally perforation takes place suddenly, unpreceded by any other considerable symptoms of disease, as, for instance, when the progress of the ulcer is quite latent. Extensive adhesions may occasion long-continued disorders of the stomach and induce ill health; but a small adhesion may remain after cure without harm.

The prognosis of a chronic ulceration is always doubtful, although many cases of cure are known; and although a cure can take place at any stage, still it must always be considered a dangerous disease. Death results either from hæmorrhage or peritonitis, or when the course is very chronic from exhaustion. It is supposed that nearly a third of the known cases prove fatal.

In regard to treatment, the most important rule is, that the patient subject himself to a strict dietetic regimen, and observe the strictest quietude, in order to favour the cicatrization of the ulcer. Besides these, we must endeavour to combat particular distressing symptoms. Milk diet is recommended as the most suitable, such as new cow's or asses' milk; others recommend buttermilk. Generally light fluid nourishment agrees best. For gastralgic symptoms, morphia and similar preparations are employed; for the frequently persistent constipation, elysters; for obstinate vomiting, ice, or small quantities of soda-water; for vomiting of blood, ice, alum, tannin, etc., or ferrum muriat., from ten to twenty drops a dose. The chief difficulty of treatment consists in this, that the chief indications, absolute rest, and abstinence from every thing injurious, cannot be fulfilled. We are compelled, therefore, to confine our efforts to reducing the action of the stomach to the minimum, and administering the most easily digested food; a course of treatment which must be persevered in for a long period. For perforation, of course, little can be done, and treatment of symptoms is alone available. — *Prag. Viert. Jahrschrift für die Pract. Heilk.*, bd. iv., 1866.

SURGERY.

RADICAL CURE OF VARICOCELE.

MAISONNEUVE, for the radical cure of varicocele, recommends the use of coagulating injections of the perchloride of iron by means of a syringe. A single injection of two to twenty-five drops is sufficient to obliterate the whole bundle of the veins of the plexus pampiniformis, without the slightest risk to the life of the patient, or the integrity of his sexual functions. — *Quoted in the Vierteljahrschrift für die praktische Heilkunde* for 1866, *from the Gazette Hebdom.* for 19th January 1866.

EXOSTOSIS OF FRONTAL BONE.

A CASE of sub-periosteal extirpation of an ivory exostosis of the frontal bone is briefly reported by Dr Fried. Scholz. A healthy servant girl, æt. 21, had for three years noticed a hard fixed tumour on the right upper eyelid, near the inner canthus, at first about the size of a pea and painless. When Dr Scholz first saw the patient it was of ivory hardness, hemispherical in shape, and as large as a walnut, fixed with a broad base to the upper border of the orbit. The upper eyelid, which was completely separable from the tumour, was stretched, and in a condition of ectropion. The lids could not be completely closed. Among the subjective symptoms were severe pains in the eyeball, flashes of light, and scintillations. The whole circumstances of the case led Dr Scholz to infer that the tumour probably did not extend very far back, and that its origin was certainly from the orbit. Accordingly, under chloroform, he made an incision from the outer canthus along the edge of the orbit over the convexity of the tumour, through the soft parts to the bone, dissected off the periosteum, and displayed the whole tumour, except its base, which extended backwards into the orbit. With knife and bone-pliers (as there was no room for the saw) he removed the tumour, making an opening into the frontal sinuses (one and a-half centimetres long, and three-quarters broad). The wound healed in four weeks, and the result was good; the eyeball has regained its normal position, and all the symptoms have disappeared.—*Vierteljahrschrift, ut supra.*

FRACTURE OF THE FOREARM.

PROFESSOR B. SCHMIDT, in Leipzig, has interesting observations from preparations and experiments on the dead body, on the subject of the deficient power of supination after fractures of the forearm. He explains this from the tendency which the lower broken fragment of the radius has to become pronated by a rotation on its own axis, and to unite in that position without a corresponding rotation of the lower part of the ulna. As a remedy for this, he recommends that in cases where it is difficult to prevent the displacement, the limb should be put up in the position in which it is least likely to occur, namely, with the forearm supinated.—*Arch. d. Hlk.*, 1866, ii., *quoted in the Vierteljahrschrift für die praktische Heilkunde, Prag*, 1866.

AMPUTATION AT KNEE-JOINT.

DR MAZANOWSKI, a Russian surgeon, supports the operation of amputation at the knee-joint in preference to amputation of the thigh, on the following grounds:—1. The chief advantage is the greater use the patient can make of it in walking; 2. The less amount of the body that is removed; 3. The smaller size of the wound; 4. That no muscles are divided except the upper ends of the gastrocnemius; 5. That the stump is composed of skin; 6. That it is not necessary to divide the thigh-bone with the saw. [Its use must be limited, as it necessitates a healthy condition of the condyles; and Dr M.'s method is bad, being a purely circular amputation.]—*Op. cit., ut supra, etc.*, pp. 71, 72.

ETIOLOGY OF FRACTURE OF CLAVICLE.

FROM a well-reasoned and interesting paper by Dr Packard on Fractures of the Clavicle, we extract the following sentences regarding the mode of production, and rationale of the displacement:—

He points out a relationship between the clavicle and first rib, which has been too much ignored; where, in muscular individuals, the first rib forms a wide and strong arch, and, at the same time, the collar-bone runs somewhat backwards as well as closer to the rib, the two bones may be almost in contact as far nearly as to the middle of the clavicle:—"If this statement as to the possibility of leverage of the clavicle over the first rib be accepted as correct, it will obviously account for some of these accidents, as, for instance, for the occurrence of fracture of the collar-bone by the recoil of a duck-gun. Any blow sustained upon the bone just outside of the point where it bears over the first rib will act upon it as upon a lever of the first order, the rib being the fulcrum, the attachment to the sternum the weight. Here the lever will give way at its weakest point, be this just at the fulcrum, anywhere between this and the force, or just where this latter is applied." After giving some instances of fracture by muscular action, he goes on to ask,—

"It may be asked, however, why does not the first rib give way? simply because the rib is at an advantage, being a strongly stayed arch, pressed upon at its convexity; while the clavicle is at a disadvantage, being subjected to a force at its concavity, tending to increase its curvature. In all the cases quoted of muscular action, and in most of those recorded from direct violence, the seat of fracture, if stated, was at or near and to the inner side of the middle of the clavicle.

"Let me state distinctly that the idea I have endeavoured to set forth is simply, that in some persons (perhaps in but a small proportion, and chiefly in males) the clavicle is in so close neighbourhood with the first rib that, upon the application of any force tending to carry its outer end either downwards or backwards, it may give way, just as a stick is broken over a man's knee. The bone, like the stick, will yield at its weakest point. And I offer this as the true explanation of those cases in which muscular action is the only assignable cause of the fracture.

"Very possibly this leverage over the first rib may have something to do with some fractures by indirect violence, as when a man falls forwards on his outstretched hand, and the scapula being forced backwards by the head of the humerus, carries the acromial end of the clavicle along with it. Where the first rib is thick and forms a large arch, the collar-bone would tend to be bent over it, and thus to be broken."

On the subject of the displacing force, Dr Packard lays justly great stress on the power possessed by the serratus magnus and pectoralis minor, and by them alone, "to rock the scapula forward around the thorax, and thus bring the acromion (pushing the outer fragment of the clavicle before it) nearer the middle line." Under the head of treatment, the principle involved is the same in all displacements, "the true principle being to carry the scapula backwards; this may be effected by acting on the head of the humerus, either by a figure-of-eight bandage, properly applied and bearing upon the sound shoulder, the elbow being carried forward and well supported, or by a cap of muslin or linen, so made as to embrace the upper part of the arm, and fastened in the same way."—*New York Journal of Med. Science.*

Part Fifth.

MEDICAL NEWS.

NAVAL MEDICAL DEPARTMENT.

THE Pamphlet[1] that we briefly noticed in our January number, which is generally considered to have been at least semi-officially inspired, would seem to have foreshadowed the advent of the Admiralty "Naval Medical Cadet" scheme (first announced in our able weekly contemporary, the *British Medical Journal* of the 12th January), with the scope of which many of our readers are doubtless familiar. It was, we presume, the elaboration of Mr Childers' notable suggestion, alluded to in our September number, which was to put the hard-bested board in a position at once to be independent of the schools as heretofore, and to be able to train up a special class of medical officers for the public service—gentlemen endued with "sufficient ability, due humility, and becoming thankfulness for a low scale of pay."

As will be remembered, no sooner was "their Lordships'" project known than the indignant voice of the schools and the profession at large was raised against it; and the leading Service papers also condemned it in unqualified terms.

On the 2d February the same Journal wrote as follows:—

ADMIRALTY REPENTANCE.—We hear with great satisfaction that the ill-devised scheme for creating a class of bonded medical recruits, to be called naval medical cadets, which we announced a few weeks since, has, for the time at least, been laid aside. Had it been produced it must have proved an entire failure, as is apparent from the strong feeling of indignant opposition to it manifested in the medical schools and throughout all branches of the medical profession both within and out of the service. We felt sure that, when all sides of the question were fully placed before Sir John Pakington, he would at once recognise the impolicy of a measure at once so humiliating and insulting to a liberal profession and so much at variance with sound principles of administration. It is well known that in this matter the Admiralty very unwisely listened to the advice of irresponsible persons, who had no means of ascertaining the true state of feeling in the service, and who were almost equally ill-informed as to the opinions of the profession. The Admiralty will find its best advisers, first, in those practical men who are acquainted with the wants of the department to which they belong; and next, in the official heads of colleges and recognised leaders of the medical profession who, when consulted, take means to ascertain the actual state of opinion. We feel satisfied that the naval medical service might be made a thoroughly popular service, and the elements for the changes necessary to produce such popularity are to be found in the evidence before the Committee on the position and pay of army and navy medical officers.

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[1] "Everything about Them," &c.

We trust that our well-informed contemporary spoke with authority; and, at the same time, we would fain disbelieve in the rumour that (failing such needy aspirants) my Lords "are ready to open their arms to anything in the guise of a medical student, and we are prepared to welcome the élèves of German universities for service in the British Fleet!"—*Lancet*, 26th Jan.

The following circular has lately been issued:—

CIRCULAR NO. 4 W, CANCELLING CIRCULAR NO. 22 W.
ADMIRALTY, *February* 8, 1867.

My Lords Commissioners of the Admiralty, having had under their consideration the rank, pay, and position of naval medical officers, are pleased, under the authority of her Majesty's Order in Council of 6th July 1866, to establish the following regulations:—

1. Staff-surgeons to be placed on a separate list, and promotion to that rank to be open to officers for distinguished or special service, although twenty years' on full pay may not have been completed.

2. Surgeons especially promoted to be staff-surgeons before serving twenty years, are to have 16s. 6d. a-day half pay.

3. The whole time served on full pay as an assistant-surgeon, to be allowed to surgeons to qualify for the rank of staff-surgeons, provided the examination for surgeon is passed before the officer completes ten years' service.

4. The pay of surgeons and staff-surgeons to increase by periods of four years instead of five years as at present.

5. In order to put naval medical officers on the same footing as army medical officers, in respect of allowance in hospitals at home and abroad, they are to receive the following money allowances in lieu of provisions for themselves and servants, and for fuel and light:—

	At Home.	Abroad.
Inspectors of hospitals,	£85	£130
Deputy-inspectors,	67	112
Staff-surgeons and surgeons,	53	112
Assistant-surgeons,	39	108

All other allowances for provisions to cease.

In cases where medical officers draw provisions or fuel from public stores, they will be charged for the same at cost price.

6. The scale of travelling allowances, extra pay, lodging money, and compensation for losses, to be fixed for naval medical officers according to their relative rank with other naval officers.

7. In regard to cabins; to meet the requirements of the service it is necessary that the senior executive officer and the staff-commander, or master, should have the cabins placed most advantageously for the performance of their special duties; with these exceptions, medical officers are to have cabins

according to their relative rank in the service; cabins will be allotted to assistant-surgeons.

8. Staff-surgeons to be placed on the same footing as commanders with regard to servants.

9. A staff-surgeon to be appointed to all flagships bearing the flag of a commander-in-chief on a foreign station, with an allowance of 5s. a-day in addition to his established pay.

10. The periods of retirement by age to be fixed as follows:—

Surgeons and assistant-surgeons at - - 55 years of age.
Staff-surgeons - - - - 60 ,,
Inspectors-general and deputy inspectors-general 65 ,,

11. Assistant-surgeons at home, after completing their time for examination for the rank of surgeon, may be granted two months' leave of absence on full pay, on condition of their resuming their studies at a medical school or hospital.

12. To place staff-surgeons on an equality in rank with surgeons-major in the army, they are to rank with commanders according to date of commission.

13. Officers in command of her Majesty's ships must, on all occasions, whether on shore or afloat, take rank and precedence of officers placed under their command. A ship must always be represented by an executive or combatant officer, after whom all officers are to take precedence according to their respective rank.

14. The daily pay of naval medical officers to be increased in accordance with the following scale:—

RANK.	Under 5 Years' Service.	Above 5 Years' Service.	Above 10 Years' Service.	Above 14 Years' Service.	Above 18 Years' Service.	Above 22 Years' Service.	Above 26 Years' Service.	Above 30 Years' Service.
	s. *d.*	*s.* *d.*	*s.* *d.*	*s.* *d.*	*s.* *d.*	*s.* *d.*	*s.* *d.*	*s.* *d.*
Assist.-Surgeons,	10 0	12 6	15 0	17 6*	...	...	...	...
Surgeons, . .	...	...	17 6†	20 0	22 0	...	...	...
Staff-Surgeons, .	...	...	...	...	24 0†	27 0	29 0	...
Deputy-Inspectors-General of Hospitals and Fleets, .	...	...	...	...	30 0†	32 0	35 0	37 0
Inspectors-Gen. of Hospitals and Fleets, .	...	...	...	...	...	45 0†	47 0	50 0

* Provided he passes his Examination before ten years' service.
† Or on Promotion.

15. Naval medical officers to be permitted to retire after twenty years' service on full pay, but the rate of half pay awarded to officers so retiring, not to exceed five-tenths of the full pay to which they may be entitled from length of service.

16. As a special reward to officers of long and good service, who, owing to the comparatively small numbers of the inspectorial ranks, have not been promoted to any higher position than that of staff-surgeon, such officers of the rank of staff-surgeon as have served for twenty-five years on full pay, to be allowed the half pay of £1 a-day, on being compulsorily retired at sixty years of age, or retired on medical survey.

17. My Lords will consider and publish hereafter the manner in which it may be found most advisable to assist naval medical officers in their professional education after examination and admission into the Navy.

The new scale of pay to come into operation from the 1st January 1867, the other arrangements, including the increased allowances in clause 5, from the 12th July 1866, the date of circular No. 22.

By command of their Lordships,
HENRY G. LENNOX.

Our readers will perceive that certain improvements have thus been made,—*e.g.*, the increase, so far, of the home allowances, and the additional 2s. per diem after twenty-six years' service. Other moderate concessions must yet be granted, however, before their Lordships and the profession can be in accord.

We cordially concur in the following well-expressed views :—

"There is no inherent reason why the naval service should be so unattractive as it is in the schools. It would require no very great changes to alter the current of feeling. *Let the retirement for officers of twenty years' service be granted; let the honorary distinctions which are fairly won be liberally granted (as in the Army), and let the privilege of relative rank be strictly accorded in spirit and in letter.* The cost of such concessions would be a bagatelle. But these once made and faithfully adhered to, we feel assured that there would be no difficulty in obtaining candidates for the naval medical service. *They would satisfy existing officers in the service, and when they are satisfied, the medical schools will respond cheerfully to the summons of their country.*"—*British Medical Journal*, 16th February.

May the day soon come when we shall be able to recommend our young medical friends to enter Her Majesty's Navy!

COMMISSIONS IN THE BRITISH MEDICAL SERVICE.

THE Director-General of the Army Medical Department encloses a List of the Candidates of Her Majesty's British Service, who were successful at the Competitive Examination in August last, and who have passed through a Course at the Army Medical School, showing the combined results of the Examination.

Names.	*Studied at*	*Marks.*	*Names.*	*Studied at*	*Marks.*
Collins, M.	Cork,	4980	Richards, C. F.	Dublin,	3475
Harman, W. M.	Dublin,	4753	Clifton, G. H.	London,	3376
Fernandes, A. L.	Edinburgh,	4508	Cornish, H.	Edinburgh,	3363
Moore, S.	Dublin,	4410	Fitzmaurice, J.	Dublin,	3155
Gibson, G. J.	Cork,	4315	Armstrong, C.	Dublin,	3137
Nugent, H.	Dublin,	3963	O'Neill, C. J.	Dublin,	3106
Williamson, J.	Aberdeen,	3892	Macartney, J.	Galway,	3031
Steele, W. H.	Dublin,	3758	Longhead, W.	Dublin,	2945
Archdall, T. G.	Dublin,	3637	Dunn, G. C.	Edinburgh,	2840
Major, N. B.	London,	3585	Gamble, S. B.	Dublin,	2768
Ratigan, R. H.	Dublin,	3550			

PROFESSOR SYME'S TREATMENT OF ANEURISM.

THE following reference to the practice of our distinguished Professor of Clinical Surgery by a French surgeon will be read with interest. It occurs in a review of the third edition of Sedillot's Operative Surgery. Sedillot, in speaking of the old operation for aneurism, has stated, as his opinion, that it is now completely forgotten,—on which the reviewer remarks:—"A treatise on operative surgery ought not, cannot forget that this question is restored to 'the order of the day' of surgical practice by the most brilliant operator, and perhaps the boldest surgeon who honours contemporary surgery. What? Is it after the brilliant successes obtained by Mr Syme by means of opening the sac, after the Edinburgh surgeon has cured by this daring method aneurism of the origin of the carotid, and even of the iliac, at the very moment when his memoir on this subject is occupying the attention of surgeons, that M. Sedillot contents himself with this simple mention,—'As these procedures are almost entirely abandoned, we need not trouble ourselves further about them.'"—*Gazette Hebdomadaire*, January 25, 1867.

OBITUARY.

DEATH OF DR SCORESBY-JACKSON, F.R.S.E.

We record with sorrow the loss which the profession has sustained by the death of our esteemed friend and colleague, Dr Robert Edmond Scoresby-Jackson, which took place at his residence in Queen Street, Edinburgh, on the 1st February. In the discharge of his duties as Physician to the Royal Infirmary, Dr Jackson was necessarily exposed to the contagion of fever; but although at previous periods, and especially during the winter session of 1865-6, he had taken charge of large numbers of typhus patients, he had hitherto escaped an attack of the disease. In the course of the present winter, the fever patients were comparatively few in number; nevertheless, Dr Jackson's health having been somewhat impaired by overwork, he was suddenly struck down by typhus on the evening of Saturday, 19th January. There can be no doubt that he contracted the contagion at the Infirmary, since he was not attending any typhus cases elsewhere. By a singular coincidence, his house-physician, Dr John Moir, a son of the famous "Delta," was seized with typhus three days after, but has since happily recovered. Dr Jackson's attack was at first mild; but at the beginning of the second week, alarming head-symptoms suddenly became developed. After two days of great danger, the symptoms moderated, and hopes were entertained of his recovery. But on the night of the 31st, he again became restless, and early on the morning of 1st February his strength began to give way rapidly, and he expired at half-past eight in the morning, at the early age of thirty-three years, adding another to the valuable lives that have been sacrificed in the discharge of professional duty.

Although his career has been so brief, Dr Jackson had already acquired distinction, and it is sad to reflect that he has been cut off just as he was rising to greater eminence. Born at Whitby, Dr Jackson received his medical education in London, chiefly at St George's Hospital. In 1855 he became a member of the Royal College of Surgeons of England, and licentiate of the Apothecaries' Company. He next prosecuted his studies in Paris and in Edinburgh, where he took the degree of M.D. in 1857. The title of his thesis was "Climate, Health, and Disease," subjects to which he had already devoted his attention. Following out this line of inquiry, he went abroad soon after his graduation, and visited various parts of the continent of Europe, ultimately extending his excursion to Algiers, where he spent some time, and collected a valuable series of notes on the climate, especially in regard to its curative effects. On his return, he determined to settle in Edinburgh. He was elected a Fellow of the Royal College of Surgeons in 1859, of the Royal Society in 1861, and of the Royal College of Physicians in 1862. He joined the Medico-Chirurgical Society; and took active duty as a physician of the New Town and Medical Missionary Dispensaries. In 1862 he was chosen lecturer on Materia Medica and Therapeutics at the Medical School, Surgeons' Hall, succeeding Dr Douglas Maclagan who had been nominated to the chair of Medical Jurisprudence in the University. In 1865, Dr Jackson was appointed physician to the Royal Infirmary, and shortly afterwards became one of the lecturers on Clinical Medicine. In addition to these appointments, Dr Jackson was an active member of the Meteorological Society, of which he was one of the council, and chairman of the Medical Department; and he was likewise a director and zealous supporter of the Edinburgh Medical Missionary Society.

As an author, Dr Jackson first made himself known in 1861, by the excellent memoir which he wrote of his uncle, the Rev. Dr William Scoresby, celebrated for his Arctic voyages and discoveries. It was after his uncle's death that Dr Jackson assumed the name of Scoresby in addition to his own. His next work, on Medical Climatology, published in 1862, embodied the results of much study, as well as personal observation, on his favourite subject; and it was followed next year by a remarkable paper read before the Royal Society of Edinburgh, on "The Influence of Weather upon Disease and Mortality." This paper was published in their Transactions, illustrated by beautifully executed coloured plates. After this period, Dr Jackson's pursuits became more strictly professional, and, besides making various minor contributions to periodical medical literature, he devoted his energies specially to the preparation of the "Note-book on Materia Medica, Pharmacology, and Therapeutics," a valuable manual, which was completed less than a year ago. Of his latest contributions of note, the one, an elaborate account of a remarkable case of Aphasia, coming on after typhus fever, in a young medical friend and pupil of his own,—a case which Dr Jackson had attended with untiring assiduity and kindness,—was published, as our readers remember, illustrated by lithographs of the patient's handwriting, in the January number of this Journal; the other, a case likewise of Aphasia and Right Hemiplegia, but depending on cerebral softening, appeared in our last number. The interesting account of Villemin's curious experiments on the inoculability of tubercle in the same number, published anonymously, as is usual, in the Periscope, was also contributed by Dr Jackson. From these works and papers, the qualities which distinguished Dr Jackson as a physician and an author will readily be perceived. Highly educated and variously informed, he was especially remarkable for his thorough and minute investigation of any subject he undertook, and for his scrupulous accuracy in regard both to observation and statement. Such qualities are essential to the true cultivator of science; they entail a great deal of labour, often unrewarded, but they command the respect of all earnest and truthful minds.

Truthfulness in matters of science was, however, only a part of the genuine truthfulness of his whole character. Dr Jackson was eminently a man of high principle. The earnestness and depth of his religious convictions may be seen from casual remarks in the Memoir of his uncle Dr Scoresby. Though never obtruded, it was evident that religion formed the strong under-current of his life; not showing itself merely in acts of charity and disinterested kindness, nor only in the part he took in medical missionary societies, but influencing and continually sustaining his daily conduct and daily duty. Work is worship when rightly understood.

With genuine excellence of character were united those refined manners and social accomplishments, which, notwithstanding a certain degree of natural reserve, made Dr Jackson's company desired and highly esteemed. No one of his standing could have been more deeply regretted than he has been in Edinburgh, in which, though coming to it only a few years ago as a stranger, he has left a large number of sorrowing friends.

Dr Jackson was married in 1858 to the only child of Sir William Johnston of Kirkhill. His widow, with two daughters, survives to mourn his loss.

DEATH OF DR WILLIAM GILCHRIST, TORQUAY.

WE have received intelligence of the death of Dr William Gilchrist at Torquay, on 12th February. Dr Gilchrist was the son of a medical practitioner at Polmont, and graduated in Medicine, with distinction, at the University of Edinburgh in 1857. He soon after went abroad to pursue his studies, more especially in regard to physiology, which was his favourite subject, and spent some time in Berlin. On returning to Edinburgh he commenced practice there, devoting his leisure to scientific pursuits. He acted for a short time as assistant to Professor Bennett in his class of the Institutes of Medicine, but was interrupted in his duties by a violent attack of hæmoptysis, which confined him for some time, and rendered a change of climate advisable. This led to his settlement at Torquay, where his health for a time considerably improved. Latterly, however, we believe the chest affection again became active. Dr Gilchrist communicated papers and reviews, chiefly on Physiological subjects, to various medical journals; and for some years he contributed a quarterly Report on Physiology to this Journal. His chief original researches bore upon the influence of the vagus nerve on respiration, and were published in the *British and Foreign Medico-Chirurgical Review* for 1858. The profession has lost in him a zealous and enthusiastic worker.

APPOINTMENT TO THE LECTURERSHIP ON MATERIA MEDICA AT THE MEDICAL SCHOOL, SURGEONS' HALL, EDINBURGH.

ON the 16th February, Dr Angus Macdonald, F.R.C.S.E., was unanimously appointed to the Lecturership on Materia Medica and Therapeutics at the Medical School, Surgeons' Hall, vacant by the death of Dr Scoresby-Jackson.

DR CHARLES BELL ON THE PERMANGANATES IN DIPHTHERIA.

(*To the Editor of the Edinburgh Medical Journal.*)

SIR,—I beg leave to say, in reference to the paper on Diphtheria in your Journal for the present month, that the treatment by the permanganate of potash is "not altogether new in this disease," nor has it been less successful in the "experience of others" than in the practice of Dr Campbell; as, from its remarkable effect in removing the false membrane in diphtheria, I was induced, six years ago, to publish a paper on the subject in the London Medical Review, which led to the treatment being adopted by some of our most experienced practitioners here. My paper was reprinted by Mr Condy, the discoverer of the permanganates, and widely circulated among the medical profession. When I first brought this remedy under the notice of my medical brethren there was none of it to be got in Edinburgh. As a further evidence of my having first suggested the use of the permanganates in diphtheria, I shall add an abstract from Mr Muter's interesting account of the permanganates. "The remarkably satisfactory nature of the results obtained from the use of the permanganates in this affection (diphtheria), as well as in scarlatina, by Dr C. Bell, to whom is due the merit of having originated the treatment, are deserving the most serious attention on the part of the profession."[1]—I am, Sir, yours, etc.,

C. BELL.

Edinburgh, February 20, 1867.

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[1] The Alkaline Permanganates and their Uses, by John Muter, late assistant-demonstrator in Chemistry, Andersonian University, Glasgow, p. 40.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Influence of Marriage on the Death-Rates of Men and Women in Scotland.* By JAMES STARK, M.D., F.R.S.E.

(*Read before the Royal Society of Edinburgh*, 17*th Dec.* 1866.)

A NINE years' average of the deaths in Scotland shows that at all ages, excepting from ten to fifteen years, males die in a higher ratio than females. Table I. shows this fact; and when the male and female death-rates at each quinquennial period of life are compared, it is seen that even the additional dangers to which females are liable during the child-bearing period do not cause their death-rate to rise so high as that of the males who are exempt from all such risks.

A more important inquiry, however, and one which does not seem to have been hitherto investigated, in consequence of the difficulty of procuring facts which could be depended on, is, What is the effect of marriage on male and female life? Is its influence, in so far as the death-rate is concerned, for good or for evil? Is its influence limited to the female; or has it also a marked influence on the duration of life in the male?

TABLE I.—*Mean Annual Percentage of Deaths to the Population at different Ages in Scotland during the Nine Years*, 1855-63.

AGES.	Percentage of Deaths to Population.		AGES.	Percentage of Deaths to Population.	
	Males.	Females.		Males.	Females.
0–5	6·238	5·686	50–60	2·099	1·771
5–10	0·924	0·892	60–70	3·933	3·308
10–15	0·501	0·513	70–80	9·410	8·266
15–20	0·731	0·662	80–90	20·011	18·075
20–30	1·006	0·817	90–100	37·700	36·325
30–40	1·077	1·021	100 and above,	39·652	41·979
40–50	1·388	1·186			
			Total, all Ages,	2·216	2·023

So far as known to me, no attempt has been made by any statist to trace the effect of marriage on the sexes. Even Quetelet, in his masterly treatise on Man, has not hinted that marriage exerts any influence on human mortality. In a paper which I published in 1847, "On the Sanitary State of Edinburgh," it was stated as one of the results of the inquiry into the mortality of the married and unmarried in that town, that the mean age at death of the married was ascertained to be 57·54 years, while that of the unmarried from the 22d year of age to the close of life was 42·18 years. The result seemed so extraordinary that I was led to doubt the accuracy of the returns of the ages; but to the present day I have had no opportunity of following out the inquiry, and it does not seem to have been taken up by any other. It is true that a few managers of insurance companies have in their reports alluded to the mortality of the married and unmarried who were insured in their offices; but these partial facts could give no information as to the influence of marriage on mortality in the general population.

In an inquiry like the present, everything depends on the correctness of the registers on which the statistical tables are founded. The tables for the males were prepared from the Registers of Death in Scotland for the year 1863; those for the females from the same Registers for the years 1861 and 1862; and as almost no death escapes registration in Scotland, and the Scottish people are very particular in entering on the death registers the age and condition of their deceased relatives, every confidence may be placed on the tables prepared from these registers. The numbers also are sufficiently large to yield truthful results, being for the whole of Scotland.

I. *Death-Rates of Married and Unmarried Men.*

Table II. shows the total male deaths above 20 years, in Scotland, for the year 1863, distinguishing the married from the unmarried; and grouping the ages into quinquennial periods for the sake of easy reference and accurate comparison.

This table is a very remarkable one, and the results brought out by it quite startling, so great is the difference between the mortality of the married and the unmarried. By that table it appears that at every age from 20 to 85 the death-rate of the married men is very much smaller than that of the unmarried. After the 85th year of life the numbers are too small to afford the means of accurate comparison, and the observations would require to be carried over a great number of years before any definite conclusions could be arrived at with regard to the influence of marriage at these extreme ages.

By that table it appears that, from 20 to 25 years of age, out of every 100,000 unmarried men 1174 died during the year; but out of a like number of married men only 597 died, or just half the number. In other words, between the age of 20 and 25 years, the death-rate of the bachelors was exactly double that of the married men.

As the age increases, the difference between the death-rates of the married and unmarried decreases; but it decreases slowly and regularly, showing a marked difference in favour of the married men at every quinquennial period of life. Thus, at the age of 25 to 30 years, when the number of married and unmarried men in Scotland is pretty nearly equal, of every 100,000 bachelors 1369 died during the year; but, in an equal number of married men, only 865 died. At that period of life, also, the death-rate of the bachelors very greatly exceeded that of the married men.

Between the ages of 30 and 35 years, while in every 100,000 bachelors 1475 died; only 907 died out of an equal number of married men. Between the ages of 40 and 45, while 1689 died out of every 100,000 bachelors, only 1248 died out of an equal number of married men. At the ages 60 to 65 years, while 4330 died during the year out of every 100,000 bachelors, only 3385 died out of a like number of married men. Even at the age of 70 to 75 years, while 10,143 died out of every 100,000 bachelors, only 8055 died out of a like number of married men. And at the

TABLE II.—MARRIED AND UNMARRIED MEN *at each Quinquennial Age in* SCOTLAND *in* 1863; *the Deaths at the same Ages; and the Proportion of Deaths to the Living at each Age.*

AGES.	HUSBANDS AND WIDOWERS.			BACHELORS.		
	Number Living.	Deaths.	Percentage of Deaths.	Number Living.	Deaths.	Percentage of Deaths.
20–25 .	22,946	137	0·597	106,587	1,251	1·174
25–30 .	54,221	469	0·865	48,618	666	1·369
30–35 .	66,153	600	0·907	25,962	383	1·475
35–40 .	63,858	690	1·080	15,857	253	1·595
40–45 .	62,645	782	1·248	12,311	208	1·689
45–50 .	54,505	869	1·594	8,824	179	2·028
50–55 .	49,591	880	1·774	7,636	205	2·684
55–60 .	38,006	929	2·444	5,550	142	2·558
60–65 .	35,920	1,216	3·385	5,242	227	4·330
65–70 .	22,021	1,134	5·149	2,848	156	5·477
70–75 .	16,029	1,291	8·055	2,021	205	10·143
75–80 .	9,716	1,135	11·681	1,081	157	14·542
80–85 .	5,477	953	17·400	513	101	19·688
85–90 .	1,708	488	28·571	151	32	21·192
90–95 .	449	137	30·512	50	21	42·000
95-100 .	103	40	38·835	6	3	50·000
100 and above	28	15	53·571	3	—	—
Not stated	—	4	—	—	5	—
All Ages	503,376	11,769	2·338	243,259	4,194	1·723

extreme age of 80 to 85, while 19,688 died during the year out of every 100,000 bachelors, only 17,400 died out of a like number of married men.

Here then, we have the remarkable fact for the first time proved by trustworthy statistics, applicable to the whole population, that the influence of marriage on the mortality of the male is of the most potent kind; that, in fact, the bachelor life is much more destructive to the male sex than the most unwholesome of trades, or than a residence in a crowded locality where there is not the most distant attempt at sanitary arrangements of any kind.

This conclusion is fully borne out by looking at the table from a different point of view, viz., by calculating the mean age at death of the married and of the unmarried men. If we take the whole married and the whole unmarried, whose deaths are noted in the above table, viz., from 20 years of age to the close of life, we find that the mean age at death of the married men was $59\frac{1}{2}$ years; whereas the mean age at death of the bachelors was only 40 years. Reckoning from the 20th year of life, therefore, married men have a chance of living $19\frac{1}{2}$ years longer than those who live unmarried.

In the general population, however, the number of bachelors and of married men is nearly equal at the 24th year of life. It will afford, then, a more unexceptional average, if we strike from the table all persons under 25 years of age, and only calculate the mean age at death from the 25th year to the close of life. When this is done, it is found that the mean age at death of the married men is $60\frac{2}{10}$ years, that of the unmarried only $47\frac{7}{10}$ years—thus showing $12\frac{1}{2}$ years of life in favour of married men.

This remarkable fact seems clearly to prove, that the married state is the condition of life best fitted for mankind; and that the prolongation of life which attends that state, is a special provision of nature to protect the father of a family in order that he may provide for his offspring, and superintend their rearing. It is quite true, however, that this special prolongation of life is based on fixed laws of nature. For the married man is in general not only more healthy, vigorous, and free from disease than the unmarried, but he is also more regular in his habits, is better housed, better fed, and better attended to. Indeed, the married men may be accounted, in one sense, as selected lives; for the weak, the delicate, those suffering from disease of any kind, the dissipated, the licentious, do not marry, so that all such are found among the unmarried. And as all such die at a much higher rate than the robust and temperate, we have what we may term a natural explanation of the great difference in the death-rates of the married and unmarried men.

This, however, would only account for the difference in the death-rates of the married and unmarried to a very small extent, and that, too, during the earlier years of life,—say from 20 to 40 years; but it quite fails to explain the difference in the death-rates at the higher ages. Thus, all the men between 20 and 30 years who remain unmarried from natural infirmity, or from possessing sickly or diseased constitutions, or from being addicted to intemperate or licentious habits, die before they have attained their 40th year. Scarcely one such survives the critical climacteric age of seven times seven. All men, therefore, who have survived their 40th year, certainly all above 50 years, must be considered, for all practical purposes, as selected lives; and if a difference should be found in the death-rates of the married and unmarried after that age, it can alone be referred to causes connected with the married state. But Table II. proves that at every separate quinquennial age, from 40 years up to extreme old age, married men died in a much smaller ratio than the unmarried. This lower death-rate in the married men can, therefore, be alone attributed to the influence of marriage, and to the habits which attend marriage in this country; and it would be a most interesting subject for inquiry in countries where married habits are different, to ascertain to what extent these different habits caused the death-rates of the married and unmarried to differ from those of the Scottish people.

This new fact in vital statistics seems to throw light on other subjects. For instance, it has been found impossible to reduce the mortality of the soldier, even when in barracks, and not on active service, to the mortality of the male sex in civil life, except by invaliding and paying off all the men likely to die. The above facts account for this. Sanitarians, in their ignorance of the powerful influence of marriage on the male mortality, were comparing the death-rates of the unmarried soldier with that of both married and unmarried men in civil life. Whereas, the mortality of the soldier ought to have been compared with the death-rates of bachelors alone, and not with the bachelors of the whole population, but with those living in towns, who, we know, die at a much higher rate than those living in the country. If, on the other hand, we consider the soldiers to consist of selected lives, which in reality they are, then their mortality should be compared with that of married men, when it would prove their death-rate to be twice greater than at present we imagine it to be.

Let us return to our Table II. We have seen that at each quinquennial period of life the death-rates of the married men were greatly below those of the unmarried. One would, therefore, naturally expect that a comparison of the death-rates at all ages would show the same conclusion. The very reverse, however, is the case. The summary of that table contradicts, and is opposed to the results of every quinquennial period; and were we to trust to that summary, it would lead us to the manifestly false conclusion, that married men die in much higher proportions than bachelors. Thus the summary shows that bachelors at all ages only died at the rate of 1723 annually in every 100,000 bachelors; whereas married men died at the rate of 2338 annually in every 100,000 married men.

Every one can see that this is a paradoxical and false conclusion. Because, if, at every several year of life, bachelors died in higher proportions than the married men, it must be the same for the whole of life. How, then, it may be asked is this paradoxical result produced?

This is at once explained by looking at the respective numbers dying at each age. Very nearly a half of all the bachelors who died had not attained their thirtieth year of life. At that age the mortality, as compared with the population, is very small; so that when the deaths at all ages are compared with the total number of bachelors, the deaths at the earlier ages bulk so largely that they cause the general mortality of the bachelors to appear to be very little higher than it was at the age 25 to 30 years. All this is reversed for the married men. Instead of the greatest mortality among them being at ages under 30 years, by far the largest number of deaths occurs between 60 and 80 years, at which period of life the mortality, as compared with the number living, is very high. The consequence of this is, that when the total deaths are compared with the living, the death-rate seems to be much higher than it was with the bachelors.

Summarizing the columns, therefore, and comparing the total deaths of the bachelors with the total deaths of the married men, necessarily leads to a false conclusion. The comparison is only correct when limited to the mortality at each quinquennial age, or to that of the mean age at death—supposing that our object is to ascertain the comparative healthiness of an occupation, or the value of life in each.

All are aware that the Registrar-General of England has published, in the supplement to his Twenty-Fifth Annual Report, a series of tables giving the first fruits of his inquiry into the death-rates of persons following different occupations or trades. Hitherto these tables have not been turned to any account, indeed have been quite overlooked, from wanting the key to interpret them. Dr Farr, evidently expecting, as we all did, that they would of themselves settle the question as to the comparative healthiness of each occupation, selected fifteen known trades for comment, and calculated the proportion of deaths to the number of persons following each of these trades. The glaringly erroneous conclusions brought out by comparing the deaths at all ages with the total numbers following each trade, and which he could not explain, seem to have so staggered him that he wisely abstained from commenting on them at all; but, in lieu of comment, added a table, in which he arranged these fifteen selected trades, not in their order of least mortality at all ages, but in their order of least mortality between 45 and 55 years!

Thus, by comparing the deaths at all ages with the number of persons following each occupation, the most unhealthy trades were made to indicate a death-rate only half as high as occupations noted for the longevity of those following them. Thus, bakers, butchers, miners, workers in cotton, flax, tin, copper, lead, etc., were, by this mode of interpreting the Tables, made to have a much lower death-rate than agricultural labourers, shepherds, ploughmen, physicians, lawyers, officers of the national government, and clergymen. But the true comparative healthiness or expectation of life in each occupation cannot be ascertained in this way, but by deducing the mean age at death, or by comparing the relative mortality at the several quinquennial periods of life.

Our Table II., therefore, and the explanation attempted to be given as to the discrepancy between the proportional mortality of the married and unmarried at the several ages, as compared with that at all ages, furnish the key wherewith we shall be able to unlock the treasures at present hid from us in the Registrar-General's Tables. The same explanation applies to them as to our Table. The summary at all ages, though a statistical truth, necessarily leads to a false conclusion.

The above facts, then, prove how careful we must be in deducing conclusions from statistics. Tables of figures, provided they are accurately drawn up, and embrace all the necessary elements, are necessarily truths. But they do not necessarily lead us to true conclusions. On the other hand, as in the above instances, though the figures express a truth in themselves, they necessarily lead us to form erroneous conclusions. In fact, it is much more difficult to arrive at the truth of statistical facts than most people imagine, —perhaps more difficult than in most other sciences,—because we fancy we are dealing with facts and with truths which must infallibly lead us to a true conclusion.

II. *Death-Rates of Married and Unmarried Women.*

The deaths of the married and unmarried women of Scotland at each quinquennial period of life, from 15 years upwards, were abstracted for the two years 1861 and 1862, and the results are given in Tables III. and IV.

In Table III. are given the number of married and unmarried women living in Scotland at each quinquennial period of life, at the middle of the year 1862, the deaths during the same year, and the proportion of deaths to the living. For the year 1861, however, only two columns are given, showing the proportion of deaths in the married and unmarried women during that year, for the sake of affording the means of easy comparison with the results of 1862. By that Table it appears that, though the proportion of deaths at each age differed very slightly during each of these years, there was almost no difference in the results of the comparison between the death-rates of the married and the unmarried at each age; for, excepting at the extreme ages 80 to 85 years, and again from 90 to 95, when the numbers are too small to afford constant quantities, the results brought out by the Table of both years are absolutely identical.

This almost unexpected result confirms the general correctness of the Tables prepared for each year, and shows that the facts brought out by these Tables may be depended on, and that they demonstrate what may be called the law of nature on the subject. Our remarks may, therefore, be limited to Table IV., which gives the mean result of the two years.

When the mean annual percentage of deaths in the married and unmarried women at each quinquennial period of life is compared, it is found that the married died in a higher proportion during the three quinquennial periods of life, 15 to 20, 20 to 25, and 25 to 30 years, but that during the next two quinquennial periods of life, viz., from 30 to 35, and from 35 to 40 years, during which nearly half of their children are born, the married died at a lower rate than the unmarried. Thus, in every 100,000 married, and in a like number of unmarried women, between the ages of 15 and 20 years, 860 married died for every 692 unmarried. Between 20 and 25 years, 911 married women died for every 783 unmarried; while between 25 and 30 years, 940 married women died for every 866 unmarried. From 15 to 30 years, therefore, the mortality of the married exceeded

TABLE III.—*Number of* MARRIED *and* UNMARRIED WOMEN *at different Ages in* SCOTLAND, *in* 1862; *Deaths at the same Ages; and the Percentage of Deaths to the Population at each Age. Also, the Proportions for* 1861.

AGES. 15 Years and Upwards.	Married and Widowed Women in 1862.			Married and Widowed in 1861. Percentage of Deaths.	Unmarried Women in 1862.			Percentage of Deaths in Unmarried Women in 1861.
	Number Living.	Deaths.	Percent. of Deaths.		Number Living.	Deaths.	Percent. of Deaths.	
15–20	3,384	25	0·738	0·983	154,829	1,075	0·694	0·691
20–25	39,924	364	0·912	0·910	114,180	895	0·783	0·783
25–30	71,649	683	0·953	0·928	61,044	506	0·829	0·903
30–35	77,503	764	0·985	0·927	34,954	367	1·049	0·941
35–40	71,962	806	1·120	1·116	24,368	300	1·231	1·181
40–45	69,660	806	1·171	1·116	19,690	225	1·142	1·109
45–50	58,934	785	1·332	1·270	14,659	222	1·507	1·455
50–55	54,479	826	1·516	1·449	13,973	269	1·926	1·576
55–60	41,462	886	2·136	1·989	10,216	244	2·389	2·077
60–65	42,390	1,200	2·830	2·649	11,405	370	3·244	2·715
65–70	26,242	1,242	4·732	4·269	6,875	350	5·090	4·489
70–75	20,337	1,342	6·549	6·627	5,544	434	7·828	6·672
75–80	12,190	1,424	11·681	10·448	3,230	359	11·114	9·741
80–85	7,490	1,278	17·062	15·105	1,986	332	16·717	16·236
85–90	2,646	708	26·756	23·179	582	161	27·663	25·597
90–95	763	282	36·972	32·147	181	55	30·386	39·778
95–100	203	94	46·305	36·138	48	20	41·666	35·416
100 & ab.	34	17	50·000	50·000	23	3	13·042	26·087
Not known	—	11	—	—	—	3	—	—
All Ages	601,252	13,543	2·252	2·109	477,787	6,190	1·295	1·240

that of the unmarried. During the next ten years this is reversed, for in every 100,000 of each class, between the ages of 30 and 35, only 956 married women died, but 995 unmarried; and between 35 and 40 years, only 1118 married women died, but 1206 unmarried.

At the age when the usual "change of life" occurs, viz., between 40 and 45 years, the mortality of the married slightly exceeded that of the unmarried, seeing that in every 100,000 of each class 1143 married died for every 1125 unmarried. This result might have been expected, seeing that the fatigues of child-bearing and nursing, and the harder labour connected with the rearing of her family, somewhat weakens the system of the married woman, and renders that critical period of life more trying to her than to the unmarried woman.

From the 45th year of life to old age, or 75 years, married women die in smaller proportions than the unmarried; and though the numbers above that age are too small to afford steady results, the table shows that from 80 to 95 the chances are still in favour of the married woman.

It is thus seen, that at every quinquennial period of life, the difference between the death-rates of the married and unmarried women is very much less than that between the married and unmarried men. It is thus for the first time demonstrated, that marriage exerts a much more powerful influence on the male than it does on the female; for whereas the influence of marriage on the death-rate of the female is comparatively trifling, it is of the most marked and potent kind on that of the male. The common belief has always been the reverse of this. The common belief has always been that marriage, by adding to the female the additional dangers of child-bearing, would be found greatly to increase her mortality; but it was never even once suspected that it would

TABLE IV.—*Mean Annual Percentage of Deaths among the* MARRIED *and* UNMARRIED WOMEN *of* SCOTLAND, *at different Ages, during the two Years,* 1861 *and* 1862.

AGES.	Percentage of Deaths to the Number Living.		AGES.	Percentage of Deaths to the Number Living.	
	Married and Widowed.	Unmarried Women.		Married and Widowed.	Unmarried Women.
15–20	0·860	0·692	65–70	4·500	4·789
20–25	0·911	0·783	70–75	6·588	7·250
25–30	0·940	0·866	75–80	11·064	10·427
30–35	0·956	0·995	80–85	16·083	16·476
35–40	1·118	1·206	85–90	24·968	26·630
40–45	1·143	1·125	90–95	34·559	35·082
45–50	1·301	1·481	95–100	41·221	38·541
50–55	1·482	1·751	100 and above	50·000	19·064
55–60	2·062	2·233			
60–65	2·739	2·979	All Ages	2·180	1·267

make any difference in the mortality of the male. These facts, however, whose correctness there is no denying, disprove all this; and prove that marriage exerts a much more powerful influence on the death-rate of the male, than all imagined sanitary improvements could ever hope to effect.

Let us, however, return to our tables, and see what additional light they throw on the death-rate of the married women. It was seen that from 15 to 30 years married women died in a higher ratio than the unmarried, but that from 30 to 40 years they died at a lower rate. As nearly half of the children who are born are produced by mothers 30 years of age and upwards, it is worth while to inquire what is the cause of this proportional difference in the death-rates. The fact that the death-rate of the married women from 30 to 40 years of age is lower than that of the unmarried at the same ages, of itself proves that it is not mere child-bearing which increases the mortality of the married woman who is under 30 years of age. Yet it must be some cause connected with child-bearing, else the mortality of the married women from 15 to 30 years of age would have remained as low as it is in the unmarried at these ages. For this cause we have not far to seek.

Every medical man knows that the risk to the mother is far greater at the birth of her first child than at any subsequent delivery; and it is extremely probable that the whole extra mortality of the married female under 30 years of age, may be caused by the greater dangers which attend the birth of her first child. Indeed, this may be considered to be demonstrated by the following facts and considerations.

In the Second Annual Detailed Report of the Registrar-General for Scotland,—viz., for the year 1856,—a table was given showing the ages of all the women in Edinburgh and Glasgow who gave birth to children in 1855, and also the number of children to which each woman had given birth. Table V. is prepared from that table, and shows the proportion of mothers at each quinquennial period of life who gave birth to their *first* child at these respective ages. That table at once demonstrates that the high mortality of the married woman, as compared with that of the unmarried, at ages between 15 and 30, is solely due to the super-

TABLE V.—*Number of Mothers in Edinburgh and Glasgow in* 1855,—*the Number and Proportion at each Age who bare their first child.*

Ages of Mothers.	Total Number of Mothers.	Number bearing their First Child.	Percentage bearing their First Child.
15–20 .	403	354	87·8
20–25 .	3814	1921	50·3
25–30 .	5057	1019	20·1
30–35 .	3943	331	8·3
35–40 .	2395	124	5·1
40 and above	961	32	3·3

added dangers which attend the birth of the first child. Thus, between 15 and 20 years of age, 87 per cent. of the mothers gave birth to their *first* child. From 20 to 25 years of age, 50 per cent. of the mothers gave birth to their *first* child. While from 25 to 30 years, 20 per cent. of the mothers bore their *first* child. Above that age, the proportion of women giving birth to their first child was quite trifling, gradually diminishing from 8 to 3 per cent.

It was, therefore, only at the ages when a very large proportion of the married women were giving birth to their first child that the death-rate of the married women rose higher than that of the unmarried. But the moment that age was attained, when the great majority of the married women had got over the birth of their first child,—viz., above 30 years,—the mortality of the married women fell below that of the unmarried. This fact seems clearly to demonstrate that the higher death-rate of married women between the ages of 15 and 30 is induced by causes connected with the birth of their first child. In fact, the Table seems to prove that after the birth of her first child the married woman, even from 15 to 30, has an equal chance of life with her unmarried sister; and that, after she has passed her 30th year, and during the whole of the remainder of the child-bearing period (30-40), she has a better chance of life than the unmarried woman.

Seeing things are so, there seems to be nothing to prevent the higher death-rate of the married woman under 30 years of age to be quite arrested. Once we know the cause, the remedy lies in our own hands. Medical men all know whence the dangers attending the birth of the first child chiefly arise. Most of them are removable if taken in time. They are causes almost entirely due to our civilisation, and to our faulty habits, which produce an over-excitable, over-stimulated, nervous, worn-out frame, where health and vigour ought alone to exist.

ARTICLE II.—*On the Pathology of Tubercular Meningitis.* By H. CHARLTON BASTIAN, M.A., M.D. Lond., F.L.S.; Lecturer on Pathological Anatomy and Assistant-Physician to St Mary's Hospital.

MOST pathologists are now pretty well agreed as to the relative importance, and precedence in point of time, of the two principal morbid changes characterizing the disease which, in the present day, is usually styled Tubercular Meningitis. Some few, however, still retain the old name "Acute Hydrocephalus," which seems to imply that they regard the effusion into the ventricles of the brain, and the softening of their walls, as the main and primary elements of the disease, instead of looking upon these as secondary, and even not invariable, results of certain more fundamental changes having their seat in the meninges of the organ.

We do not mean to deny the existence of such a disease as may be properly named Acute Hydrocephalus. Such may exist, and may rightfully deserve the name which we are unwilling to accord to the morbid condition now in question.

It is not my object to go into details concerning the history of Tubercular Meningitis, more especially since this has been so fully dwelt upon in the exhaustive *Traité des Maladies des Enfants* of MM. Rilliet and Barthez, and I will only call attention to a few facts. Robert Whytt[1] gave the first important account of the disease; though it seems not to have escaped the attention of Hippocrates, and was also mentioned, more or less incidentally, by several intervening writers. Whytt, as well as his immediate successors, Fothergill,[2] Ludwig,[3] and Odier,[4] looked upon the ventricular effusion as constituting the essence of the malady. In the next year, however, Quin[5] endeavoured to show that the effusion into the ventricles was a result of inflammation; and this same idea, as to determining cause, was held by many subsequent writers, who differed nevertheless widely as to the seat of the inflammation. Senn was the last of those who considered the disease as purely inflammatory in its nature. His monograph appeared in 1825, and he showed that the inflammation occupied the meshes of the pia mater. Whilst looking upon the inflammation as of an ordinary kind, he and others before him had recognised the scrofulous diathesis as a predisposing cause, and he had, moreover, described the phlegmasia as "ayant une forme *granuleuse*." Guersant,[6] immediately following him in 1827, adopted the name of "*meningite granuleuse;*" and though recording, at the same time, the frequent coincidence of tubercle in other organs in the bodies of those dying from this disease, it apparently did not suggest itself to him that the "granulations" might be real tubercles. It did suggest itself, however, to Papavoine,[7] and he has usually received the credit of finally divining the real nature of the disease. His views were expanded and generalized about five years afterwards by Rufz and Gerhard, and in the opinion of Rilliet and Barthez, in an especially able manner also by MM. Fabre and Constant, in an unpublished prize memoir.[8] The labours of these physicians terminated in the following general conclusions, which were for a long time looked upon as incontestable:—1. That the meningeal granulations are of a tubercular nature; 2. That they are identical with the granulations of the serous membranes; 3. That they are met with only in subjects whose other organs also contain tubercle;

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[1] On the Dropsy of the Brain, 1768.

[2] Medic. Obs. and Inquiries, vol. iv., 1771.

[3] Dissert. de Hydr. Cereb. Puer. Leipsic, 1774.

[4] Mém. de la Soc. Roy. de Méd., 1779.

[5] Treatise on the Dropsy of the Brain. Dublin, 1780.

[6] Dict. de Méd., p. 392.

[7] Journal Hebdom., t. vi. p. 113.

[8] *Mémoir e sur la Méningite tuberculeuse.* A manuscript of 258 pages deposited at the Institute on March 30, 1835, to compete for the Monthyon prize, and *couronné* during the following year.

and, 4. That the disease known by the name of acute hydrocephalus is a tubercular malady. These conclusions have been, and still are, received by a large number of pathologists, but the first has been rejected by a certain number, and more especially by many of the modern French school; whilst, amongst other things, in the present communication, I shall hope to be able to prove that the second conclusion is quite incorrect.

With reference to the dispute concerning the first conclusion, it turns upon an old question that has always been considered more or less debatable ground, but which has of late years again received an increase of attention. It is as to the nature of the so-called *grey granulations*, which were first described by Bayle, and regarded by him as non-tubercular, although the same structures were, on the other hand, looked upon by Laennec as the invariable first stage of what afterwards became yellow tubercle. The most frequent situations in which these bodies are met with are the serous membranes, but they are also encountered in the lungs, and more rarely in the parenchymatous organs. The difference of opinion concerning their real nature is most radical at the present day: thus, the majority of modern French pathologists, amongst whom may be mentioned Robin, Bouchut, Luys, and Empis, siding with Bayle, regard them as granulations of a fibro-plastic and non-tubercular nature. The latter indeed looks upon them as typical of a constitutional state, allied to, though different from, tuberculosis, which he has lately treated of under the name of "La Granulie."[1] Some few English pathologists, however, and Virchow, the chief of the Berlin school, look upon these grey granulations, whether single or aggregated, as the only undoubted form of tubercle, and regard with a suspicious eye the yellow and cheesy varieties, believing them to be, when not mere after-stages or degenerations of the true grey tubercle, non-specific in nature, and rather the occasional results of chronic inflammation. Doubtless much time and labour have been wasted in minute descriptions and discussions as to the particular varieties of tubercle, their external size, shape, and colour, and what we want in the present day is to come to some agreement as to the mode in which morbid products arise, as well as to ascertain what are the precise relationships and differences between nearly allied diseased states. Does tubercle, for instance, become developed from a previously effused blastema, or is it the result of a continuous development of preexisting cells; and, if so, of what cells? How does it differ from an ordinary product of inflammation; and what is the mode of genesis of a so-called false membrane? Such, however, are still questions of much dispute amongst pathologists. Those of the humoral school still cling to the old traditions, and talk of effusions of lymph, and specific blastemata, whilst others, casting aside these traditions, and leaning rather in an unbiassed manner upon the teachings of the microscope, speak to us of continuous growth, proliferations of cells, and gradual differentiation of tissue.

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[1] Empis: Paris, 1865. Although the author in this work attempts to show as above stated, that the granulations are something altogether distinct from tubercle, still he admits (p. 49) that the granulations may sometimes tubercularize. But why, if his position be correct, because it can be shown that the granulations sometimes undergo changes, whereby they lose their original semi-transparent appearance, become opaque, and then yellowish in colour; why should these, which were just held to be specifically distinct from tubercle, be said to have been converted into tubercle, simply because they have undergone an alteration in colour, due to a retrograde fatty metamorphosis? Surely this is like denying the existence of a giant with whom he is wrestling, and amounts pretty much to saying that he himself might become the giant, if he could change the colour of his skin through some morbid process of nutrition. It makes colour and fatty degeneration the essence of tubercle.

With reference to the second of the before-mentioned conclusions, to the effect that the granulations of the meninges of the brain are identical with those of the serous membranes generally, I am unable to assent to this for two reasons: firstly, because I hope to show that the granulations of the pia mater are something altogether peculiar, having intimate relations with the arteries in this situation and with certain perivascular sheaths which surround them, but which do not surround those of the other serous membranes. So that, whereas, in the pia mater the granulations are always immediately connected with the arteries, and, moreover, present histological pecularities, in the serous membranes such as the pleura or peritoneum, they have been shown to be local aggregations of cells, mostly of a fibro-plastic nature, with an amorphous intercellular substance; the masses themselves having no necessary relations to the vessels of the part, and being probably formed, as Virchow and Vulpian[1] maintain, by a local excessive growth and proliferation of connective tissue cells. My second doubt as to the identity of the granulations of the pia mater, and those of the serous membranes, is of a more general description, and refers rather to the membranes themselves. For although it is true that granulations are never situated actually in the substance of the arachnoid, but always in the tissue beneath it, and it may be replied that the granulations of the other serous membranes are as frequently situated in the subserous tissue as in the very substance of the serous tissue itself, still it seems to be the opinion now of many anatomists that the arachnoid cannot be considered as an ordinary serous membrane. What was formerly held to be its parietal layer is really nothing more than a simple internal epithelial layer of the dura mater; and its relations to the pia mater are such that the part really existing, the so-called visceral arachnoid, may, for the sake of simplicity, be regarded as only the external condensed portion of the pia mater. It may perhaps be desirable still to retain the name "arachnoid" for this outer portion, though it should always be recollected that it cannot be considered as an ordinary serous membrane.

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[1] Bullet. de la Soc. Méd. des Hôpitaux, t. v. p. 35. An excellent paper to consult concerning the seat and nature of these bodies.

Before proceeding with the narration of the cases on which this paper is founded, and an account of their pathology, it seems desirable that I should give some particulars concerning the anatomy of the bloodvessels of the central nervous system, and of the above-mentioned perivascular sheaths, which are of great interest, though perhaps not widely known.

Wedl[1] noticed that, in certain morbid states of the brain, some of its small vessels showed on their outer wall "a hyaline, imperfect layer of connective tissue, studded with oblong and rounded nuclei." Rokitansky made somewhat similar observations, and these have been confirmed by Dr Sankey,[2] in his investigations on the state of the brain in general paralysis of the insane. He says:—"Another appearance which my preparations show, is what is called a hyaline deposit around the capillaries, fitting, as it were, more or less closely to the vessel, in greater or less degree of transparency and extent;" and further on he adds—"To me it appears highly probable that the hypothesis of Rokitansky[3] is correct, as well as that of Wedl, relative to the formation of connective tissue from a material thrown out by the capillaries; and that in the first stage this material is hyaline; that it afterwards contracts; that in contracting it throws the capillaries into bends and kinks, as in fig. 3, *a.*; that as it goes on contracting, it becomes less hyaline, more fibrous, and at length like a sheath." The present state of our knowledge, as will be seen hereafter, renders it improbable that these kinks and bendings should have been produced by the sheaths of the bloodvessels, and shows, moreover, that what has been considered by the preceding observers to be an evidence of a morbid condition of the cerebral vessels, is, on the contrary, so far as these sheaths are concerned, a perfectly normal accompaniment.

Cruvelhier[4] makes some interesting observations concerning the connective tissue surrounding the arteries of the brain, which may be considered, perhaps, to contain the germs of what has been elaborated by later research. He speaks of the space at the base of the brain bounded posteriorly by the pons and the cerebral peduncles, and anteriorly by lines from the inner extremity of the fissure of Sylvius, on each side, to the anterior extremity of the corpus callosum, and calls it "l'excavation médiane du cerveau." This, he says, contains loose filamentous cellular tissue, separating the arachnoid from the pia mater, and also fluid in communication with the sub-arachnoid fluid of the cord, and he looks upon it as the centre of the cerebral cellular tissue, adding, "Elle se prolonge le long des gros vaisseaux qui constituent le trapèze artériel de la base du cerveau, et le long des divisions qui en partent; ces prolongéments suivent toutes les ramifications vasculaires auxquelles ils forment *une gaîne* proportionnée à leur calibre, s'enfoncent dans l'épaisseur du cerveau avec les capillaires, et constituent la charpente ou trame celluleuse de ce viscère." From this description, it would seem that Cruvelhier must have recognised what subsequent observers have seen and more correctly interpreted. Thus, in 1855, Robin [1] read a communication to the Academy of Medicine in Paris, concerning certain hyaline sheaths surrounding some of the capillaries and smaller arteries of the cerebrum, and he wrote another paper on the same subject a few years later.[2] He considered that these sheaths were not universal, and that they ceased to be apparent on vessels of a certain size. The sheath itself had a much larger diameter than the vessel, its walls were delicate, almost structureless and undulating, whilst between it and the vessel were situated, more or less abundantly, cells of a pretty uniform character, and resembling strongly those met with in the lymphatic system. There can be little doubt that Robin was mistaken in supposing that these vascular sheaths disappeared on vessels about one-third of a millimetre in diameter, by amalgamating with the external coat of the vessels. Professor His [3] has recently made known the existence of certain perivascular spaces around the bloodvessels of the brain and spinal cord. These he has succeeded in injecting; and, moreover, he has found the injected fluid pass outwards from the perivascular canals, into large epi-cerebral and epi-spinal lacunæ situated between the surface and the meninges of these parts. If pushed still further, the injection fills a system of lymphatic canals in the pia mater itself, similar to those which were described in the beginning of this century by Arnold and Fohmann. (Fig. 10.) These perivascular canals, as described by His, are bounded by a layer of condensed tissue, but since his investigations were principally directed to ascertaining the distribution and extent of this system, his work was done with injections, and he did not examine or, indeed, particularly enforce the fact that there was a system of perivascular sheaths, though he so fully ascertained the presence of perivascular spaces, which seemed to him to be extensions inwards of the lymphatic system—in fact, veritable lymphatic canals, each of which contained a bloodvessel in its interior.[4] Lockhart Clarke [5] confirms the account

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[1] Beitrage sur Pathologie der Blutgefasse. Wien, 1859.

[2] Journal of Ment. Sc. Jan., 1864.

[3] Ueber Bindegewebs-Wucherung im Nervensystem. Wien.

[4] Anat. Patholog., vol. i. livr. vi. Paris, 1829–35.

[1] Compt. Rend. de la Soc. de Biolog., p. 142. Paris, 1855.

[2] Brown-Séquard's Journal, 1859.

[3] Zeitschrift fur Wissen. Zoologie, band xv., 1865. A translation of this important paper will appear in the next (May) number of Camb. Jrnl. of Anat. and Physiol.

[4] However peculiar this arrangement may seem, it does not appear to be confined to the brain. According to Ludwig and Tomsa, a similar arrangement exists in the testicles; whilst MacGillavry (Sitzungber. der k.k. Akadem. der Wissench. in Wien, bd. l.) says it is also present in the liver, and His reports the same of the retina. Amongst the lower animals, it has been asserted by Fohmann and Stannius (Siebold and Stannius, Man. d'Anat. Compar., vol. ii. pp. 121, 243), that the bloodvessels are contained within the lymphatic canals in some fish, and in many reptiles; whilst, quite recently (Unters. über die Lymph-drüsen, Leipzig, 1861), Frey has maintained that a similar disposition exists in the pancreas Asellii of the rabbit.

[5] Lancet, September 1, 1866.

given by Robin of the presence of sheaths around *many* of the bloodvessels of the central nervous system. I have frequently examined structures of the same kind, and my observations fully tend to confirm those of His, since I have met with these perivascular sheaths universally, surrounding the cerebral and spinal bloodvessels, and also surrounding most of those in the pia mater. I have frequently seen unmistakable sheaths around arteries of a much larger calibre than those mentioned by Robin, and the diameter of the sheaths in different cases is extremely variable; I have occasionally seen it more than three times as great as that of the contained vessel.[1] The structure of the sheath-wall (which is readily separable from the brain-tissue, and is almost always detached with the vessels) in a healthy organ is almost homogeneous or hyaline, with perhaps faint traces of fibrillation in its substance, and a variable quantity of round or oval nuclei on its inner surface, as well as others, of a different character, on its outer surface. These sheaths, however, and the perivascular spaces are the seats of several distinct pathological changes, concerning one of which I purpose to speak in detail. From what I have seen, I have little doubt that all the perivascular spaces described by His are bounded by membranous sheaths such as were first described by Robin, although he imagined their distribution to be so much more limited.

Having made these introductory remarks, I shall now proceed briefly to give the details of the two cases on which my observations have been made.

Case I.—Sarah D., æt. fifteen months, was seen by to me as an out-patient at St Mary's Hospital, on Nov. 27, 1866. Younger brother died when ten months old, with head and lung symptoms; said to have had "water on the brain." Present child strong when born, but began to lose flesh when six months old; then had four teeth. When brought to hospital, the child was very low and feverish, suffering much from cough, and frequent sickness. 1*st Dec.*—Had slight convulsions of a general character in the evening, for the first time, which recurred on the next evening, and continued with great severity during the night and most of the next day. 4*th Dec.*—Still vomits occasionally. Lies quite insensible in mother's arms; takes no notice of objects brought immediately before its eyes. Pupils medium-sized, but very sluggish. Continually moves right hand and arm, putting its hand up to face and head. Does not move the left arm or leg at all, and has not done so since the cessation of the convulsions on 3d inst.; they seem partially paralyzed. Body extremely thin and wasted. Mother says extreme heat of scalp has existed at times, and that the child often moans and cries most piteously. Scalp now not notably warmer than other parts of body; fontanelle full and slightly prominent. Temperature in axilla, 101½° F. Pulse 160, small and weak. Cough very troublesome. Resonance over chest not much impaired, but distinct semi-metallic râles were audible over the greater part of both lungs, though they were more abundant on right side. *8th Dec.*—Is rather weaker, coughs with less power. Now takes beef-tea, and a little sherry greedily. Motions very offensive, black, and almost putrid. Four grains of calomel, with sugar, to be dropped upon tongue. Cries and moans for hours together. Convulsions frequent, and sometimes lasting more or less continuously for two or three hours; right side of body only being affected. Both eyes turned to right. *10th Dec.*—Much the same. Convulsions rather more frequent, though of shorter duration; generally cries before onset of convulsions. Left side of body still paralyzed. Eyes now not fixed in any direction; pupils still sluggish. Temperature in axilla on paralyzed side, 100⅚° F.; and on non-paralyzed side, 101⅚° F. *11th Dec.*—Convulsions commenced again yesterday evening, and continued almost without intermission till the child's death at 10 A.M. this morning. The abdomen became much swollen and tympanitic several hours before death.

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[1] This large size of the perivascular sheaths occurs more particularly in the corpus striatum and optic thalamus, and not unfrequently several vessels are contained within dilated portions of a single sheath in these situations.

On examination of the head, twelve hours after death, when the dura mater was stripped off, the surface of the arachnoid was somewhat sticky, and the membrane was duller than natural. The veins of the pia mater were full of blood. The right hemisphere of the brain seemed to have been much compressed, since the convolutions were notably flattened and the sulci between them almost obliterated. On the left side the pressure did not seem to have been so great, as the convolutions were scarcely at all flattened, and there was even a small quantity of sub-arachnoid fluid over the anterior part of the hemisphere. The difference between the two sides was very striking. On removing the brain from the skull, some yellowish white lymph was found at the base between the cerebral peduncles and the commissure of the optic nerves. This matter was not very firm or very abundant in quantity, but it extended laterally into the Sylvian fissures, the edges of which were glued together. Near the edges of the commencement of the Sylvian fissure, many small opalescent granulations were seen, though only a very few others scattered over different parts of the brain could be detected on its external surface. When the Sylvian fissures, however, were forcibly separated, a multitude of these granulations were seen, some as large as a mustard-seed, and opaque white in colour, together with lymph on and along the middle cerebral arteries and their ramifications. In these latter situations, the membranes could not be stripped off without tearing the surface of the convolutions, though in most other parts they could be removed without injury to the grey matter. In nearly all parts of the brain, also, the portions of pia mater dipping down between the convolutions were found to bear granulations on the vessels contained therein. On section of the hemispheres, the grey matter was found to be dark in colour, and the white substance showed many blood-points on the surface. The ventricles were both extremely distended, but the amount of fluid contained in them could not be ascertained, since this unfortunately escaped through a rupture at the base of the brain. The fluid did not seem to be very turbid; and, as far as could be estimated, there was about four ounces contained in the lateral ventricles. The walls of these and the fornix were much softened, the latter being almost diffluent; and the walls of the ventricles were in some places eroded on the surface. The superficial veins of the ventricular surface were all distended and full of dark blood. Projecting from the surface of the anterior portion of the right corpus striatum were two or three small greyish nodules about the size of a large mustard-seed, and when a tranverse cut was made through the corpus striatum in this situation, a mass was found, about the size of an ordinary filbert, evidently tubercular in nature, and composed of a close aggregation of granulations, similar to those which were found in the Sylvian fissures, so as to resemble a dense bunch of grapes in miniature. It was two or three of the most superficial granulations of this mass which projected from the surface of the corpus striatum. The mass was surrounded by a small quantity of serous-looking fluid, and the immediately adjacent brain tissue was slightly softened. It seemed to be in direct connexion with one of the branches of the middle cerebral artery lying in the Sylvian fissure beneath. On removing the fornix, the velum interpositum was found to be much thickened, and of almost leathery consistence, showing granulations throughout its substance. The thickened tissue at the base of the brain was in direct continuity with, and in just the same pathological condition as, the velum interpositum. The pia mater of the cerebellum was much less affected than that of the cerebrum. Unfortunately, I was not permitted to examine the thorax and abdomen.

CASE II.—John C., æt. five years, admitted into St Mary's Hospital under the care of Dr Alderson, Nov. 10, 1866. It was ascertained from his mother, by the nurse, that two years ago the child had had scarlet-fever, which was followed by otitis, and a purulent discharge from the left ear. The otorrhœa continued till two months before admission; then the discharge ceased, and at the same time the child's general health began to fail very much. Three weeks ago, when trying to speak to mother, the boy was unable to articulate. He became insensible that night, and remained in much the same condition till he was brought to the hospital. On admission, the child was almost comatose. Head not unusually hot; pupils both contracted. Pulse 84, feeble and compressible. Respirations deep, 32; breath sounds normal. Bowels confined for two days. A purgative was administered, and a blister applied to nucha.

11*th Nov.*—Being in same state, a leech was applied behind each ear, but without benefit.

12*th Nov.*—Quite comatose. Pupils widely dilated. Pulse imperceptible at wrist. Respirations slow and labouring. Face, and surface generally, dusky. Died at 8.30 P.M. At the *autopsy*, twenty-four hours after death, both pupils were equal and widely dilated. The membrana tympani on the left side was destroyed, and thick purulent matter was found in the tympanic cavity, and at the bottom of the meatus. No necrosis of bone. Projecting from the inner surface of the dura mater, in the left parietal region, were fifty or sixty more or less pendulous growths, varying in size between a pin's head and a mustard-seed, having a semi-translucent appearance and histological characters differing from that of tubercle—containing a much larger proportion of fibres and fusiform cells than are met with in formations of this kind. The brain completely filled the cranial cavity, and when the dura mater was removed, the convolutions were found to have been much compressed and flattened, whilst there was a total absence of sub-arachnoid fluid. Some small opaque spots of tubercle were found in the pia mater, over the left hemisphere, about the size of pins' heads, and many others of a smaller size and more translucent were seen on closer inspection. Yellow lymph-like matter was also seen on this side, extending upwards along the great vein, and its branches proceeding from the Sylvian fissure. Only a very few tubercles, and no lymph, to be seen on the outer side of right hemisphere. On removing the brain from the skull, much yellow lymph-like matter was seen at the base, binding down the optic commissure, extending into the Sylvian fissures (principally the right), and backwards as far as as the cerebral peduncles. The sides of the Sylvian fissures were closely adherent. Spots of tubercle not very obvious at the base. Lateral ventricles not distended, though containing about four fluid drachms of clear serum. No diminution in consistence of fornix or of walls of ventricles. No notable thickening of velum interpositum. Arachnoid and pia mater almost universally adherent to grey matter, so that latter was torn in stripping off membranes. Spots of tubercle most abundant in pia mater, dipping down between convolutions. Several of the frontal and parietal convolutions on the left side, when cut through, were found to be of a yellowish colour and of rather increased consistence for a certain depth; which alterations sometimes extended through the whole thickness of the grey matter. Both *lungs* filled with miliary tubercles and grey granulations; especially the upper lobes. Lower lobe of right lung considerably congested. Heart healthy; blood on right side fluid. No tubercle in the liver, spleen, or kidneys, all of which appeared healthy.

These two cases present several differences of much interest. In the first, we have considerable effusion into the ventricles, with softening of their walls, and in the second we have neither of these symptoms, though the most serious implication of the grey matter of the convolutions exists. In both, the disease manifested itself principally in a single hemisphere. In the first case, the right hemisphere was most affected: this was seen by the greater abundance of tubercle on the surface, by the local growth in the corpus striatum, and by the general turgescence of the hemisphere, as evidenced by the flattening of its convolutions, and the obliteration of the intervening sulci. In conjunction with these changes, also, we had the interesting symptom during life, of paralysis on the left side of the body. Was this due to compression of the cortical grey matter on the right side, and consequent impairment of the volitional centre for the left half of the body? or was it to be attributed to the disorganization and pressure caused in the anterior portion of the right corpus striatum by the growth of the mass of tubercle? In the second case, the left hemisphere was the one most notably affected: the superficial spots of tubercle were most abundant on this side; there was also a considerable quantity of firm yellow lymph-like matter extending upwards from the Sylvian fissure, principally along the sides of the great veins; and there was a deep implication of several of the frontal and parietal convolutions, so that on section their cut surfaces presented a yellow colour. Was this increase of disease on the left side of the brain, and the presence of growths from the inner surface of the dura mater, at all determined by the existence of a source of chronic irritation on this side of the skull, due to the otorrhœa and to the disease of the tympanic cavity? The lymph-like matter at the base and by the side of the veins in this case was yellow, and many of the superficial spots of tubercle were large and opaque-white or even yellowish in colour; whilst in the first case, which seemed to have ran even a more acute course, the specks of tubercle were almost translucent, and the lymph at the base was nearly all white, portions of it only having a faint tinge of yellow. So far as it goes, this is in favour of the view we shall have to refer to hereafter, that the translucent, the opaque-white, and the yellow varieties of deposits or growths in the pia mater only represent successive stages of one and the same morbid product. With regard to the flattening of the convolutions, and the obliteration of the sulci, this was present to a marked extent in the second case where there was not the slightest distention of the ventricles; and in the other case, in which there was distention, the compression existed on one side much more than on the other. From this it seems evident that the flattening of the convolutions must not be attributed, as is so often done, to the ventricular distention. It is more likely to be due to an increased bulk of the brain matter itself, owing to a new growth of tissue elements of various kinds, combined with a congested fulness of the vessels, and a dropsical condition of the intervening nerve substance.

It remains for me still to speak somewhat more in detail, concerning the actual seat and nature of the minute spots of tubercle, and their relationship to the more diffuse lymph-like matter which is principally met with at the base; as to the degree of implication of the cortical substance; and concerning the nature and causation of the ventricular effusion and softening.

It has been a matter of ordinary observation by many pathologists, that the tubercular granulations of the pia mater were frequently met with along the course of the vessels. As an instance of this, I may quote Lebert. He says:[1] "On les voit souvent groupées sur le trajet des vaisseaux de la pie mère, plutôt sur celui des veines que sur celui des artères, et nous en avons même rencontré entre les parois de celle-ci; cependant on en voit presque aussi souvent à une certaine distance des vaisseaux. C'est dans ces parties faciles à examiner sous le microscope, qu'on peut se convaincre que quelle que soit la proximité des vaisseaux, ils ne pénètrent point dans l'interieur des granulations." M. Vulpian does not agree with Lebert as to the invariable absence of blood-vessels from the granulations; on the contrary, he says,[2] "Très souvent au milieu d'une granulation grise, on découvre un vaisseau simple ou ramifié;" and adds, that an examination of the pia mater with a low power of the microscope often shows the tubercles "en rapport avec des vaisseaux qui paraissent les traverser." Believing, with Virchow, that these granulations are produced by a multiplication of the elements of connective tissue, M. Vulpian writes: "La presence d'une quantité considerable de tissu conjonctif dans les parois des petits vaisseaux (tunique externe) parait prédisposer ces parois à devenir le lieu d'origine des granulations grises; mais ce n'est pas une prédisposition exclusive. Il suffit d'examiner un lambeau de peritoine ou de pie mère, dans les cas de péritonite ou de méningite tuberculeuse, pour se convaincre de ce fait, à savoir que les granulations peuvent germiner, pour ainsi dire, dans des points privés de vaisseaux, et ou l'on ne trouve que du tissu conjonctif." My own observations are also contrary to those of Lebert, and even more strongly so than those of M. Vulpian, since, after repeated and prolonged investigation, I have fully satisfied myself that not only a few of the granulations of the pia mater have a vessel running through, or in direct connexion with them, but that, save for occasional exceptions, a vessel may be shown to be so related to every one of them. It only requires that one should be familiar with the ordinary appearance and disposition of the perivascular sheaths around the arteries of the brain and pia mater, to convince oneself that these granulations are due to local circumscribed dilatations of the vascular sheath, which is irregularly distended by an excessive growth of the small nuclei or cells, that are usually met with in small quantities, in the space between the sheath and the outer wall of the vessel. (Fig. 2.) In order to ascertain this in the most satisfactory manner, the best plan is to separate any two convolutions and remove a portion of pia mater from between them, when, after washing in water, it may be examined with a pocket-lens, and with successively higher powers of the microscope; or, the Sylvian fissure may be carefully separated, and the pia mater containing the middle cerebral artery and many of its ramifications may be removed. A gentle stream of water should then be allowed to pour on this till all the adhering brain tissue has been washed away, and then the preparation may be placed in water and examined with a powerful lens, when the following appearances may be easily recognised. All the arteries are observed to be thickly beset with granulations, and their external coats, after they have been in water for a few minutes, have an opaque-white aspect. Many of the larger branches are seen to have irregular outlines, owing to the existence of bulgings or swellings of different kinds; some of these being slight and occupying one side of the artery only; whilst others involve the whole circumference of the vessel, and have either an elongated fusiform or a globular outline. (Fig. 2, *a* and *b*.) The smaller twigs proceeding from the main branches are beset with similar granulations and bulgings, so that by this mode of examination one has little difficulty in convincing oneself that nine-tenths at least of the tubercular granulations are situated upon some of the larger or smaller vessels of the pia mater. But if a portion of one of the smaller vessels bearing these well-defined swellings be placed under the microscope, it will be seen that the vessel itself passes through the swelling, retaining the same uniform calibre throughout; that the vessel is invested by a more or less hyaline sheath, which is separated from its walls by a thick intervening layer of small cells or nuclei; and that the lateral bulgings, or swellings involving the whole periphery of the vessel, are due to the local increased accumulations of these very cells, which, in such situations, push the investing sheath still farther away from the wall of the included vessel.

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[1] Traité d'Anat. Patholog.

[2] Bullet. de la Soc. Méd. des Hôpit., t. v. p. 39.

Thus, the granulations of the pia mater not only seem to me to be connected even much more frequently with the vessels than was stated by Vulpian and other observers, but they appear to be always due to a proliferation of cells of a peculiar kind outside the vessels, though within the perivascular canals, and not to a proliferation of the connective tissue elements of the external coats of the vessels themselves, as stated by Vulpian. And this difference is a more important one than it may at first sight appear; since the observations I have already made lead me to believe that in cases of tubercular meningitis, the perivascular canals in almost all parts of the brain contain a greatly increased quantity of these cells. Within the substance of the brain itself the tubercular granulations and local swellings are not commonly met with, but the sheath is very frequently indeed found to be uniformly distended and separated to a considerable distance from the contained artery by a thick envelope of the cells in question. I have removed several vessels from the central ganglia of the brain, which were enclosed in distended sheaths having a diameter three times as great as that of the included artery or vein; and in other parts also I have generally found a notable increase in the amount of cells within the perivascular sheath. In the second case I have reported I found one of the large superficial veins proceeding upwards from the left Sylvian fissure, bordered on each side by a whitish line which was easily recognised by the naked eye, and which was determined by the lens to be due to matter contained within the outer sheath of the vessel. What an important influence this condition must have upon the nutrition of the brain may be imagined, if we consider that the nutritive fluid exuding from the vessels has no longer a ready access to the brain tissue, since at first much of it must be consumed in order to supply material for the growth of the proliferating nuclei, and then, after a thick layer of them has been formed, the nutritive plasma can only get to the nerve tissue after first passing through this intermediate wall of adventitious elements. If the principal function of this system of canals be a mechanical one, as suggested by His, having for its object the protection of the delicate nerve tissue from pressure, at times when the vessels of any portion of the organ are more than ordinarily distended with blood, then must the pathological condition we have described also most effectually put an end to such a function.

The next question to be considered is as to the nature of the nuclei or cells which are found so abundantly within the sheaths of the vessels. They are round or oval in shape, and vary in size between 1-10,000″ and 1-3000″, the average diameter being 1-5000″. They are but slightly affected by dilute acetic acid, and usually present no nucleolus. They do, however, contain a number of minute granules, and their bounding wall is proportionately thick and well defined. (Fig. 3.) It was suggested by Robin that they bore a certain resemblance to lymph corpuscles; and the views entertained by His with regard to the nature of the canals in which they are contained led me at first to think that this might be their real nature. On reflection, however, this view was seen to be untenable; firstly, because even though the canals in which they are contained be lympathic canals, we should not expect to find lymph-corpuscles in them, since these are not met with in the peripheral parts of the lymphatic system, and rarely occur in lymphatic vessels till these have passed through a gland; and, secondly, because their size is smaller than that of lymph-corpuscles, and their external contour is more regular and sharply defined. When the sheaths of arteries from a healthy brain are examined, the membrane itself is found to be structureless, or, at the most, having here and there, either embedded in its substance or lying on its outer surface, an oval or elongated highly refractive nucleus, similar to what is so frequently met with in connective tissue, and on the walls of the capillaries. On its inner surface scattered about irregularly, either singly or in small groups, may generally be detected round or oval nuclei similar in every respect to those which I have just described as existing so abundantly in tubercular meningitis. These will be seen all the more distinctly if the preparation has been previously tinted by a glycerine solution of carmine; and the nuclei in this case do not appear to be free and movable within the perivascular canal, but seem to be distinctly situated on the inner surface of the homogeneous wall of the sheath. It was only, however, after the most careful search, in favourable specimens, that I was able to convince myself that these were really the nuclei of a pavement epithelium lining the walls of the canal. I have by no means been able to recognise the outline of the containing cell in all cases, but I have made it out sufficiently often (Fig. 4.) to convince me of the real nature of the little cells which I now believe to be the nuclei of a pavement epithelium.[1] I am all the more convinced of the truth of this assumption from the fact that these nuclei have almost precisely the same shape and appearance — with the exception of being slightly smaller—as those of the pavement epithelium on the outer surface of the arachnoid; and because, in this situation also, there is often considerable difficulty in detecting the outline of the epithelial scale which contains the nucleus. Also, if the perivascular tubes are real lymphatics, as His imagines them to be, then we should expect to find, as we seem to do, an imperfect layer of epithelium on their inner surface. If a portion of healthy pia mater, moreover, be examined under the microscope, there is no difficulty in detecting in all parts of it nuclei similar to those I have described as existing on the inner surface of the perivascular sheath, and occasionally also the outline of the epithelial scale may be traced around them. These exist in all parts and at different depths of the pia mater, and their presence seems in accordance with the doctrines of His, Arnold, and Fohmann, that the pia mater contains an extensive system of wide lymphatic canals, which, as the former of these observers has shown by means of injections, are continuous with the perivascular canals themselves. For, in this case, the epithelium distributed throughout the pia mater might naturally be supposed to be situated on the inner surface of these hyaline canals, in the same way as they are seen to exist on the sheaths of the arteries and veins.

Often in apparently healthy brains the quantity of epithelium, or at all events of epithelial nuclei is increased, within the sheaths of the arteries, not universally, but here and there. This was noticed by Robin, and he has given an excellent figure[2] of a vessel in this condition. But the especial peculiarity of the vessels of the brain in a case of tubercular meningitis is, that throughout the entire organ, though more especially around the vessels of the pia mater, there are situated an enormous number of these epithelial nuclei, which have been produced by a continuous proliferation from the original single layer lining the inner wall of the perivascular sheath. At different points in the course of the vessels a greatly increased proliferation takes place, so that the cells become tightly pressed together, at the same time that they distend the sheath outwardly and give rise to the local swellings and bulgings that I have previously described. These circumscribed swellings are almost confined to the vessels of the pia mater, where the more unequal distribution of surrounding pressure permits of such growths arising. But for vessels contained within the brain substance itself, the surrounding conditions, as regards pressure, are more uniform, and we consequently do not get the circumscribed bulgings in this situation, but, instead, uniform and cylindrical dilatations of the sheaths, such as I have described in association with some of the arteries of the central ganglia.

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[1] Probably the bounding walls of the epithelial scales would be rendered very much more apparent if the sheaths were first immersed in a very dilute solution of nitrate of silver. This was the method adopted by His, and the examination of such preparations led him also to the opinion that the perivascular sheaths possessed an imperfect epithelial lining.

[2] Brown-Séquard's Journal, 1859.

When the under surface of the pia mater is examined in a situation where the small granulations are very abundant, it may be easily ascertained that most of these are situated within the sheaths of the vessels, but it may also be ascertained, without any difficulty, that in the neighbouring tissue of the pia mater itself, a great increase in the number of nuclei ordinarily occupying this situation is met with. This increase generally takes place in a diffuse manner, but occasionally we do get little circumscribed heaps, or aggregations of nuclei, in every way similar to those met with within the perivascular canals. These look to the naked eye much like the other more ordinary kind of granulation, but when examined under the microscope, they are found to have no membranous bounding wall or direct connexion with a bloodvessel, and they are seen to merge insensibly into the surrounding tissue. Diffuse and more or less extensive areas of proliferation are, however, most common in the meshes of the pia mater; and these local heaps or granulations, not situated within the vascular canals, are much more rare and exceptional.

It must not be supposed, however, that the epithelial elements are the only ones in which this abundant and tumultuous process of growth and multiplication takes place. The contiguous connective tissue elements probably receive a secondary stimulus, of a contagious kind, owing to the mere intensity of the nutritional energy or tendency to growth manifested by their immediate neighbours. This is seen in the pia matter, where, at times, mixed up with the heaps of epithelial nuclei, many connective tissue elements may be frequently recognised, in the form either of simple, oval or spherical, highly refractive nuclei; of bodies of the same kind forming the nuclei of elongated fibre-cells; or of large mother cells, containing from one to five or six nuclei. (Fig. 6.) These last seem to mark great intensity of the morbid process, and to be the products of a most rapid growth, judging from the extreme degree in which they are tinted by the carmine solution. Large numbers of them are seen floating in the field when an obviously affected portion of the pia mater is stripped from the convolutions, and placed under the microscope without previous washing with water. But the same increase in the number of the connective tissue elements may be seen in the walls of the sheaths, as well as in the coats of the arteries which are most affected. In the sheaths a great increase in the number of oval and elongated nuclei are first met with, in and on the surface of their homogeneous tissue, and at a more advanced stage a fibrilation of the membrane takes place, so that in this way, and by a development of the aforesaid nuclei, the whole sheath is converted from its original texture into a thin expansion of fibrous tissue. This change exists, to a variable extent, in all the arteries affected, but I have seen it in its extreme form only in the vessels of the softened ventricular walls. Delicate transverse sections of the velum interpositum, after it had been hardened in chromic acid, seemed to show an increase also in the number of connective tissue nuclei in the outer coats of the vessels, though not to a very marked extent.

It may, perhaps, occur to some, that the nuclei I have described within the perivascular canals, and to which I attribute an epithelial origin, bear a very close resemblance to the round and oval connective tissue nuclei which have been described by Robin, Vulpian, and others, as existing so abundantly in the ordinary granulations of the serous membranes. But, after numerous comparative examinations, I have found that there is little difficulty in discriminating between the two kinds, notwithstanding their agreement as regards size, and their apparent identity of form. It seems to me that this latter apparent resemblance is not a real one; the nuclei of the epithelium being more or less flattened, round or oval discs, whilst those of connective tissue are either spherical or ovoidal in form. As a result of this difference in shape, there follow certain characteristic differences in appearance under the microscope. The epithelial nuclei are not very refractive to light, their granular contents can be easily recognised, and when they are looked at just beyond the proper focal distance, their sharply defined bounding wall becomes even more manifest by being converted into a ring of white light; the contents of the connective tissue nuclei, however, cannot be easily distinguished, and these corpuscles are highly refractive to light, almost in the same way as fat globules, so that when viewed just beyond the focal distance, they appear as little spheres of light, instead of only presenting luminous margins. I have found this a serviceable means of discriminating between these two tissue elements.

In the disease we are considering, we have to deal not only with tubercular granulations of the pia mater, but also with a meningitis of a peculiar kind, limited more especially to the base of the brain, which has usually been looked upon as a direct sequence of the irritation produced by the presence of the above-mentioned granulations. It has, however, been admitted by most writers on this disease, that the amount of inflammation of the pia mater does not vary, *pari passu*, with the amount of tubercular granulations, and, indeed, that large patches of granulations may frequently exist without the presence of lymph around them. Quite recently Empis[1] has maintained that the inflammation is not the result of the presence of the granulations, but that, on the contrary, these are produced by an antecedent inflammation of a peculiar kind. The evidences of inflammation are generally most obvious at the base of the brain, in what Cruveilhier terms "l'excavation médiane du cerveau," and stretching away on either side into the Sylvian fissures. It is marked by the presence of a pretty firm, whitish, or more or less yellow lymph-like matter, such as I have described in the two cases previously reported. In the last of these cases, also, firm yellow lymph extended upwards on the left hemisphere of the cerebrum, principally along the sides of the veins ascending from the Sylvian fissure. Extended patches of lymph were, moreover, seen covering some of the frontal and parietal convolutions, and dipping into the intervening sulci.

Now, are we still to admit that this so-called lymph and the tubercular granulations are products of a different nature, or are they identical in kind but different only in form? I feel convinced from what I have seen, and from the reports of other observers, that the various differences in colour met with in each of these morbid products are only to be looked upon as indications of age, and of subsequent changes taking place in the products after they have been formed; and that, just as we can only regard the translucent, the milk-white, and the yellow granulations as successive stages in the evolution and decay of one and the same morbid growth, so is it also with the different kinds of lymph met with in the pia mater. In the early translucent stage of the granulation the nuclei are not closely packed, but afterwards, when by their continued multiplication they become more tightly pressed together, opacity commences; this increases and gradually merges into a yellow colour as the nuclei become more granular and undergo a fatty degeneration. The differences in colour of the lymph at the base and elsewhere seem to depend upon the same influences; and I think, in answer to the query recently put, there is every reason to believe that the so-called lymph is identical in nature with the material of which the granulations are composed, and that both are more or less simultaneous results of one morbid process: that neither of these is, therefore, to be looked upon as causative of the other. I have been led to form this opinion from the following considerations. As I have mentioned before, when the pia mater is examined in a part where the granulations are abundant, we see in its substance, with the aid of the microscope, a vast increase in number of epithelial nuclei, precisely similar in kind to those existing within the sheaths of the arteries. This increase is usually seen over a more or less considerable area, so as at first to form slightly opaque patches in the pia mater; but, as I have before stated, occasionally little circumscribed heaps of cells exist, differing in no respect from those which are met with at certain points within the perivascular sheaths, except that they are not situated around a vessel, and are not bounded by a membrane. And if a portion of the lymph-like matter which has already become yellow be examined, we find it composed of a number of round or oval corpuscles closely packed together, rather larger than the original nuclei, and in a state of fatty and granular degeneration. (Fig. 7.) They are, in fact, small granule corpuscles, whose size is even less than that of a pus corpuscle, and which, moreover, do not show nuclei as the latter do after the addition of acetic acid. Seeing that the nuclei within the perivascular canals may be ascertained to have undergone precisely the same changes in the older and most opaque granulations, viz., to have increased in size and become distended with granules, after the manner of cells generally in the process of degeneration, there can be little doubt that in both situations alike, these minute granule corpuscles are produced by the modifications which the original nuclei have undergone. So that, whilst I regard the collections of nuclei within the sheaths of the arteries as produced by the proliferation of the epithelial nuclei lining the walls of these canals, I look upon the lymph-like patches in the pia mater as produced by the proliferation of similar nuclei, in all probability lining the wide lymphatic spaces of which the pia mater is in great part composed (Fig. 10), and which, at all events, exist, normally, diffused throughout its substance. But the rapid and abnormal processes of growth are not confined to the nuclei of the lining epithelium, since the adjacent connective tissue elements of the fibrous partitions and trabeculæ also, to a certain extent, share in the same process, so that the old bounding walls are strengthened, and new fibres grow in all directions amongst the nuclei, so as to form a system of trabeculæ reminding one, when transverse sections are examined, of what is met with in the spleen. The vessels included in this mass of new tissue are always found to have their circumferential canals distended with nuclei. (Figs. 8 and 9.) It is owing to the increase of the fibrous elements simultaneously with the rapid development of nuclei, that the firmness and great consistence of this lymph-like matter is due; but still, occasionally, in the midst of this tissue patches of variable extent may be recognised wholly cellular, and composed of a tight-pressed mass of corpuscles, such as are represented in Fig. 7. Nowhere is there to be seen anything like homogeneous blastema or amorphous lymph, but, on the contrary, everywhere there seems evidence of direct and continuous growth and multiplication of pre-existing elements.

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[1] *Loc. cit.*

Sometimes the convolutions immediately subjacent to these patches of lymph in the pia mater are also most gravely implicated, as was seen to be the case in the second instance of this disease that I have reported. On section, the grey matter of different convolutions was found, at times, to be only superficially affected, whilst at others, it was changed throughout its whole depth, and had a distinctly yellowish tinge similar to that of the lymph-like matter above. The consistence of this altered portion seemed to be rather increased than diminished, but beyond this I am unable to go into details, as unfortunately I did not discover this change in the tissue of the convolutions when the brain was in the fresh state, and when it was seen, after having been in dilute spirit for nearly a week, it was not in a favourable state for microscopical examination. In my first case, there was another kind of implication of the brain matter in the form of a tubercular or scrofulous growth springing up in the substance of the right corpus striatum, which was probably increasing rapidly, and hence had caused disintegration and softening of the immediately surrounding grey matter. Growths of this kind are liable to occur in almost every part of the brain, and are occasionally met with, especially in or between some of the convolutions, in cases of meningitis. There seems every reason to believe that they are of precisely the same nature as the more chronic scrofulous growths, which are met with in various parts of the brain, and unassociated with an inflammatory affection. The growth in this case was particularly interesting, because of its association with paralysis during life, and because of the almost direct continuity which was ascertained to exist between it and the arteries in the subjacent Sylvian fissure, on and around which were situated a multitude of granulations and much lymph-like matter. The branches proceeding to the corpus striatum are given off directly from the middle cerebral artery; and it was in connexion with some of the anterior of these that this mass about the size of a marble was found. It had a compound structure, and was made up of a dense aggregation of little granulations, each being about the size of a mustard-seed or slightly larger. Many vessels were contained in and between the granulations, and these seemed to be made up of two kinds of elements, variously intermixed. There were heaps of nuclei here and there, closely pressed together, in their last or granule-corpuscle stage, and also a large proportion of the highly refractive round and oval nuclei with fusiform cells, such as are met with in rapidly proliferating connective tissue. In the layer of softened brain tissue around there was a small quantity of moderately clear serum, which flowed away when the corpus striatum was cut across in the situation of the growth. The softened tissue itself presented the usual characters, and, in addition, many of the large nerve cells immediately surrounding the growth appeared to have undergone a calcareous degeneration or petrifaction. The same change seemed to have extended a short way into the fibres issuing from the cells—into as much of them, in fact, as remained in contact with the cell.

There yet remains another aspect of the disease to be considered, of a most important nature, and which was at one time looked upon as constituting the very essence of the disease; I allude to the effusion of fluid into the ventricles, and to the softening of their walls. Even up to the present time the most adverse opinions are maintained by different physicians concerning the pathology of these changes. Thus, whilst Dr Hughes Bennett looks upon the softening as a post-mortem effect due to the mere maceration of the brain tissue by the intraventricular fluid,[1] Dr Wilks says[2]:—"Much controversy formerly existed as to the cause of this softening, whether it be inflammatory or due merely to the ventricular effusion; but there is no doubt as to the former being the correct explanation, both because a mere soddening of the cerebral tissue will fail to produce a like effect, and also that the microscope reveals inflammatory products. This instrument does not actually show those large granule masses which are so characteristic of ramollissement, and which I shall presently mention; but it shows the elements of these, or the fatty granules collected along the course of the capillary vessels, and which is sufficient to contra-indicate the idea of mechanical softening." Rilliet and Barthez,[3] on the other hand, adopt an intermediate opinion when they say, "Il ne nous semble pas possible d'admettre qu'il y ait la un ramollissement inflammatoire des parois ventriculaires; c'est un simple imbibition." Rokitansky,[4] again, says,—"It is, in fact, nothing more than an acute œdema of the highly delicate and easily injured texture of the brain, and the equally delicate lining membrane of the ventricles; but so rapid occasionally is its progress, and to such degrees does it advance, that it gives rise for the most part to countless lesions of continuity; and this, in the form of softening, disorganizes the brain and destroys life; if it should advance more slowly or to a less degree, it may very often continue a long time without marked symptoms." It certainly is remarkable that so much discrepancy of opinion should still prevail on this subject; but before making any remarks myself, I would again call attention to the fact that this effusion into and affection of the ventricular walls, although most commonly met with, is by no means an essential and invariable accompaniment of tubercular inflammation of the membranes. It was absent, indeed, in the second case I have reported; and when it does exist, it seems to me to be owing to the fact of the direct continuity of tissue existing between the pia mater at the base of the brain, where the morbid processes are being carried on with the greatest intensity, and the velum interpositum or intra-cerebral prolongation of the same, which is continuous with the external pia mater through the great transverse fissure of the cerebrum. Of course, where there is this continuity of tissue, and where one portion of it, namely, that at the base of the brain, is invariably affected, we should naturally expect that, in the large majority of cases, the inflammation would extend more or less to the intra-cerebral portion of the membrane, so that if we can show that such an affection of this part is capable of adequately explaining the effusion into and the softening of the ventricular walls, then it may be considered that we have obtained a more satisfactory view of the pathology of the disease, and of the coherence of its two principal anatomical characters.[1]

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[1] Lectures on Clinical Medicine.

[2] Lect. on Patholog. Anat., p. 142.

[3] Maladies des Enfants, t. iii. p. 467.

[4] Manual of Path. Anat., vol. iii. p. 352.

In the first of my cases there was effusion into the ventricles and softening of their walls, and there was also a complete extension of the inflammation from the pia mater at the base of the brain to the whole tissue of the velum interpositum, so that this was thickened and quite coriaceous in texture. I was able, with care, to remove it entire, in connexion with the similarly thickened pia mater of the base and its lateral prolongations into the Sylvian fissures, so that there existed, in one continuous piece, the inflamed membrane which had occupied the three principal sites of the disease, namely, the Sylvian fissure, the middle of the base of the brain, and the ventricles. Portions of this were put into a chromic-acid solution, and allowed to remain about a week, when the tissue of the velum interpositum was found to be sufficiently hardened to permit of very fine transverse sections being made with a sharp razor. In these sections the perivascular sheaths around all the vessels were found to be symmetrically distended with nuclei (Fig. 8), and the vessels with their sheaths were directly continuous with the surrounding tissue, which was made up of the natural connective tissue framework of the velum interpositum, every available space and interval of which was closely packed with proliferating nuclei. Whilst some few of the vessels contained coagulated blood in their interior—evidently of recent and most likely of post-mortem formation, since the separate blood corpuscles could be distinguished—some of the others, and more especially the *vena magna Galeni* of the right side, were distended with old and granular clots, in which most of the red corpuscles were amorphous or disintegrated, whilst the white were enlarging and assuming new forms, as is their wont in old clots. The left *vena magna Galeni* also contained a clot, which seemed to have been formed during life, though it was more recent and not so perfect as that on the right side. Many of the smaller vessels also, such as that represented in Fig. 8, *d*, contained old clots, by which they were completely obliterated. In sections through the lymph-like matter which was removed from the Sylvian fissures, I also found several small vessels completely obliterated. From this we must conclude, I suppose, that the activity of the inflammatory processes taking place within the sheaths of the vessels and around them, is favourable to the coagulation of blood within the vessels, or, in other words, to the production of thromboses.

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[1] Rokitansky called attention to the importance of this continuity of tissue of which I have just spoken, in accounting for that form of acute hydrocephalus which is associated with inflammation of the meninges. The nature of this form of hydrocephalus, he says, is perfectly clear:—"It is either an extension of meningitis of the base to the ependyma of the ventricles, ... or it consists of a supplemental, and, for the most part, serous exudation accompanying an acute deposition of tubercle in the pia mater at the base of the brain. This form of acute hydrocephalus, therefore, is either actual inflammation or an exudative process having a general connexion with it."—P. 354.

Now, the fact of the existence of clots in the *venæ magnæ Galeni*, which has thus been demonstrated, seems to me to be of immense importance in explaining the mode of origin of the effusion into the ventricles and of the softening of their walls, seeing that all the deep veins of the cerebrum, proceeding from parts around the parietes of the ventricles empty themselves as tributaries into Galen's veins, whilst these terminate in the straight sinus. An obstruction to the return of blood through these channels would, consequently, lead to engorgement and congestion of all the parts from which the tributary veins proceed, and hence would follow an anasarca of this portion of the brain tissue, and an exudation of serum from the superficial veins and from those of the choroid plexuses into the ventricles. In addition to the anasarcous condition of the central brain tissue, due to the pressure of blood in the overloaded veins and capillaries, there would follow an actual softening and degeneration of the tissue, owing to the cessation of the proper flow of nutrient fluid through these parts. The completeness of this process of softening would, of course, depend upon the degree of impediment existing to the return of blood from the deep parts of the brain to the straight sinus. If this were absolute, we should, of course, expect the anasarca to be extreme, and the process of softening to proceed rapidly; whilst, if the impediment were only partial or unilateral, we should expect the changes to be less marked and to proceed more slowly. There seems abundant evidence now to show that actual pathological softenings, exhibiting an abundance of granule corpuscles and aggregations of granules along the vessels, may result from obstruction to the circulation through the veins of a part, where this is complete, just as they may undoubtedly result from the obliteration of the arteries of a part, such as the middle cerebral, where it is impossible that a free and adequate collateral circulation can be established. In both cases, the change is a degenerative one, resulting from an arrest of the circulation through the part, and a consequent cutting off of the supplies of nutritive pabulum; and it has been shown that all the products, such as granule corpuscles and disintegrated nerve tubes, which occur in such cases—and whose presence was formerly looked upon (in accordance with the opinions of Gluge and others) as sufficient evidence of the previous existence of inflammation—may be produced by changes of a purely degenerative kind, and that they are, indeed, invariably met with in secondary atrophies of portions of the spinal cord, where at no time could there have been the slightest suspicion of the existence of an inflammatory process.[1] In the case of which I have been speaking, well-marked granule corpuscles and disintegrated portions of nerve tubes were met with in all parts of the softened ventricular walls, and the same are recorded by Dr J. W. Ogle[1] to have been met with in many other cases of tubercular meningitis. These observations seem directly opposed to the opinion of Dr Hughes Bennett, that the softening is due to a post-mortem maceration, and also to that of MM. Rilliet and Barthez, that it is to be attributed to the effects of a mere imbibition of serum during life; and whilst my observations are in accordance with those of Dr Wilks, we differ only as regards the interpretation to be put upon the phenomena, since what he regards as evidence of the pre-existence of inflammation I am disposed to consider may be accounted for by the progress of mere non-inflammatory degenerative changes. I have found, after repeated trials, that where a real pathological softening exists, containing an abundance of granule corpuscles, the specific gravity of this altered tissue is lowered by about eight degrees of the ordinary hydrometer scale; whereas, in cases of mere post-mortem maceration—in the one or two cases that I have estimated the specific gravity of such altered cerebral substance—I have found little or no deviation from that of surrounding non-affected portions of similar tissue.[2] This characteristic, combined with that of the absence of Gluge's granule corpuscles, ought to be sufficient, in all cases, to enable us to discriminate with certainty between a mere post-mortem maceration and a real pathological softening.

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[1] See the works of Prevost and Cotard, and of Proust, "Sur le Ramollissement cérébral," Paris, 1866; and Reynolds's "System of Medicine," art. *Non-Inflammatory Softening*, vol. ii. (about to be published).

The fact of the much more complete obliteration of the right *vena magna Galeni* may, perhaps, also account for the notable excess of turgescence of the right hemisphere of the brain; and that this was in part due to the œdema caused by venous obstruction, seems evidenced by the notable quantity of serous fluid which was collected in the softened tissue immediately surrounding the growth in the corpus striatum. We have seen, however, that venous congestion and the consequent turgescence of the brain, together with the increased growth of cellular elements taking place within the vascular sheaths and in the pia mater, are of themselves, in some cases—as in the second I have reported—adequate to explain that increase of bulk in the whole organ which leads to compression and flattening of the convolutions.

After this account of the precise nature of the changes taking place in the brain and pia mater in cases of tubercular meningitis, what conclusions are to be drawn, and what modifications require to be made in our views concerning the essential nature of the disease? In the first place, as it seems to me, what I have adduced lends strong support to the doctrines of Virchow and other cellular pathologists, and is opposed to those of the humoral school. In every part where new matter existed, there seems to have been evidence that this was derived by direct and continuous, though abnormal, processes of growth out of pre-existing elements. Nowhere did we find a trace of blastemata in different stages of organization in which a production of cells was taking place *de novo.* Secondly, I think I have shown that the granulations of the pia mater are by no means identical with those of the serous membranes, the lungs, and other parenchymatous tissues, as has generally been maintained by pathologists. Almost all microscopists now admit that these granulations of the serous membranes and the viscera are, in fact, minute fibro-plastic growths, produced by a proliferation of connective tissue elements, whilst, as I have shown, the granulations of the pia mater are almost invariably produced by a multiplication of epithelial nuclei within the perivascular sheaths, and so have a most intimate and essential relationship to the vessels: they are not to be considered, therefore, as the very types of tubercle granulations, or as identical with those of the serous membranes, but rather as structures altogether peculiar and *sui generis.* Whilst there is agreement, however, as to the histological structure of the granulations of the serous membranes, there is anything but agreement amongst pathologists as to the name that should be applied to them: thus, whilst Robin, Bouchut, Empis, and others of the French school, regard them as non-tubercular, Virchow, Vulpian, and a few English pathologists, look upon them as the very types of tubercle—the only real kind, in fact—whilst they regard what the others call tubercle as no tubercle at all, and look upon it instead as the degenerating product of chronic inflammation. It will be observed that this controversy as to the nature of tubercle turns upon the supposition that a real tubercular growth must be produced by the multiplication of one special histological element — at least this is involved in the doctrines of Virchow and others, and perhaps is essential if we are to endeavour to draw lines of demarcation between tubercular products and those of chronic inflammation. But can this distinction be made, or is it always expedient to attempt to maintain it? In both, the essence of the change seems to be due to an irritation and an increased activity of the elements of certain tissues, whereby they exhibit tumultuous and anomalous processes of growth, apparently unrestrained by the ordinary guiding and subordinating rein of healthy nutrition, and such as appear to take place, where, what we are in the habit of calling "vital power" is defective. And the particular form which the process takes seems to be dependent, in great part, upon the constitution of the individual in whom the change occurs. Would it be always reasonable, in fact, that the name of tubercle should be restricted to growths produced by a proliferation of connective tissue elements, whilst that of chronic inflammation should be applied to those masses resulting almost exclusively from a multiplication of epithelial elements—more especially when we see that both changes are mostly coincident in a single organ, and are evidently produced by one and the same morbid state of the constitution? In the lungs, for instance, how frequently, and almost invariably, are miliary tubercles mixed up with accumulated epithelial products in the air tubes; and in tubercular meningitis do we not find, co-existing with the epithelial aggregations in the vascular sheaths and in the pia mater, fibro-plastic granulations of the serous membranes and of the viscera so similar in external appearance to these as to have been hitherto looked upon as identically the same. But the evidence afforded by the same disease as to the identity of the two processes is even closer than this: for, as I have before stated, in the pia mater itself, in the neighbourhood of epithelial elements which were undergoing an active process of growth, the connective tissue cells could also be seen, apparently, undergoing precisely the same changes. The irritative stimulus, in all these cases, acts upon both kinds of elements, and the changes thus resulting seem to be identical in kind, however much the products may differ in different instances, owing to the varying proportions of the different tissue elements involved.

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[1] "On Morbid Growths of the Brain and Spinal Cord."—*British and Foreign Review*, October 1864.

[2] In making this trial with parts of the ventricular wall, we must be attentive to the situation from which the brain tissue is selected, since different parts have, normally, different specific gravities. Thus, the average specific gravity of the white matter is 1·040; of the surface of the corpora striata, 1·035; of the same portion of the optic thalami, 1·042; and of the fornix, only 1·032. (See *Jr. of Ment. Sc.*, Jan. 1866, pp. 500–523.)

From these considerations—because the disease is usually met with in individuals having an undoubtedly tubercular diathesis, and from the almost invariable presence, in fact, of tubercle in other organs of the body—we have no hesitation, in spite of the histological characters of the granulations of the pia mater, in looking upon the name Tubercular Meningitis as in every way most suitable for this disease. If it be thoroughly proved hereafter that the opinion of Professor His is correct, and that the perivascular canals and the network of passages in the substance of the pia mater are in reality lymphatics, then might this disease, undoubtedly, be considered as an inflammation of the lymphatics of the brain and pia mater. But should this view of Professor His be substantiated or not, as to their nature, there cannot be a question as to the existence of this system of canals; so that, whether they form part of the lymphatic system, or whether they constitute only an apparatus for the distribution of the cerebro-spinal fluid throughout the structure of the brain—in order that the same protective influence may be exercised over each individual portion of its structure, which is exercised over the whole organ by that portion of the fluid situated in the sub-arachnoid spaces—whichever of these alternatives prove correct, what has been said concerning the pathological changes taking place within this system of canals will remain unaffected.

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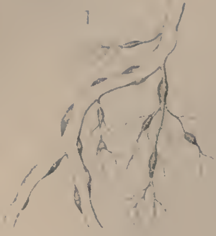

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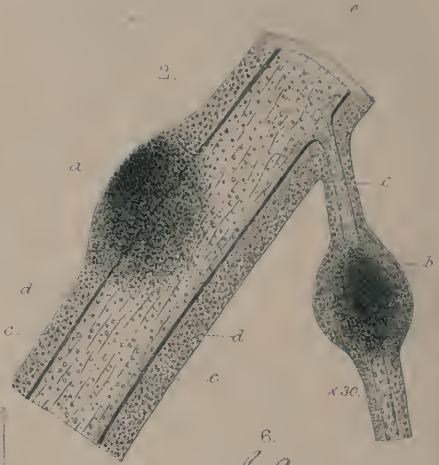

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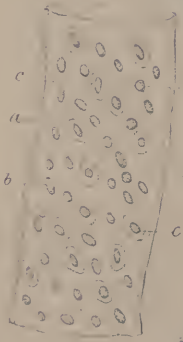

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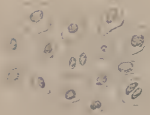

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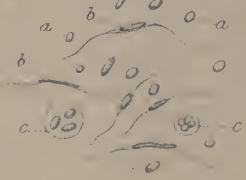

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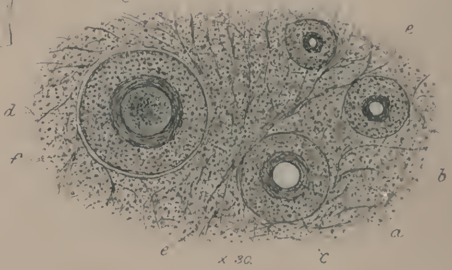

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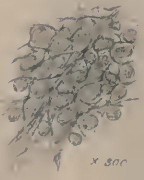

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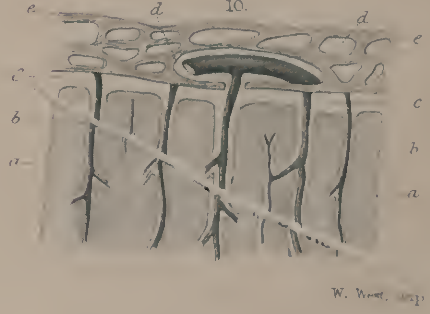

H. C. Bastian. del. Tuffen West. sc.

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Explanation of Plate.

Fig. 1.—Vessels of pia mater slightly enlarged, enclosed within their sheaths, presenting the appearance of opaque bulgings at intervals, and "granulations" formed around smaller offsets.

Fig. 2.—Representation of portion of artery 1-45″ in diameter, with its enveloping sheath, together with a smaller branch from the same. *a*, Local, unilateral bulging of sheath of artery; *b*, complete peripheral bulging of sheath of offset (tubercular granulation of pia mater); *c*, homogeneous sheath wall; *d*, abnormal increase of epithelial nuclei in perivascular space, between sheath and outer wall of vessel; *e*, enclosed artery.

Fig. 3.—Round and oval nuclei, such as are seen within and on the inner surface of vascular sheath.

Fig. 4.—Portion of vascular sheath in healthy condition, as seen without its contained artery. *a*, Homogeneous wall of sheath; *b*, *b*, epithelial nuclei on its inner surface; *c*, *c*, nuclei of same kind, around which the outline of the epithelial scale can be detected.

Fig. 5.—Round and oval nuclei of the same kind, with the epithelial scales to which they belong, from the interstices of the pia mater. The size of the scale bears no definite proportion to the size of the nucleus, and in some scales a division of the nucleus has taken place.

Fig. 6.—Fibro-plastic elements from proliferating connective tissue of pia mater. *a*, *a*, Simple, round or oval, highly refractive nuclei; *b*, *b*, caudate fibre cells; *c*, *c*, mother cells containing nuclei.

Fig. 7.—The granulation corpuscle stage of the epithelial nuclei, as they are found, when closely packed together, in yellow-coloured granulations or lymph-like matter.

Fig. 8.—Transverse section through a portion of thickened *velum interpositum*, with its vessels. The vessels are situated in the midst of a fibrous stroma, all the interstices of which are densely packed with granule corpuscles. The vessels themselves are surrounded by a thick coating of epithelial nuclei occupying the distended perivascular space. *a*, Wall of one of vessels; *b*, nuclei in perivascular space; *c*, wall of perivascular sheath; *d*, old clot, occluding one of vessels; *e*, *e*, *e*, dense fibro-cellular stroma; *f*, a minute space is here represented for the purpose of indicating that the sheath of the vessel is perfectly distinct from, and unconnected with, the surrounding stroma.

Fig. 9.—Much more highly magnified view of a portion of this stroma, showing granule corpuscles, similar to these seen in fig. 7, enclosed within the meshes of fine fibres.

Fig. 10.—Perpendicular section through the surface of the brain and its investing membrane, after His. *a*, Brain substance; *b*, *b*, perivascular canals; *c*, epicerebral space, with which the perivascular spaces communicate; *d*, *d*, sections of lymphatic canals in the substance of pia mater which freely communicate with one another, and with the epicerebral space, and some of which contain bloodvessels within them; *e*, arachnoid (condensed outer layer of pia mater).

ARTICLE III.—*Case of Amputation at Knee-Joint, by Dr Johnston, Montrose.* By WILLIAM M. WATSON, M.D., Montrose.

THE great fatality which prevails in cases of amputation of the thigh, both primary and secondary, leads us to welcome any procedure which will lessen this fatality, either directly, by improving the operation where it is unavoidable, or indirectly, by substituting another operation for it (where this can be done), in cases which have hitherto been treated in the old and much dreaded manner. That this great fatality may be lessened in the latter way, now seems probable. A mode of amputation at the knee-joint has been lately recommended by Mr Syme in this Journal; and in that paper Mr Syme states that the credit of devising this operation is due to Mr Carden of Worcester. As it is only by recording every case in which a new plan of operating has been tried, that a correct estimate of the value of that plan can be formed, this case is published. It speaks very favourably for the operation at the knee-joint; and, as far as I know, was, at the time it was performed, if it is not still, the first case in which it has been tried north of Edinburgh.

Mrs C. Taylor, aged 43, residing in the parish of Kinneff, near the town of Bervie, came to Montrose, in the middle of December 1865, to consult Dr Johnston, on account of a painful swelling on her right leg. She stated that this swelling began eleven years ago, and referred it to a blow by a stone received then. It had not increased much, or troubled her much until three or four months since, when it began to get larger, and to pain her a great deal. On examination, a tumour about the size of an orange was seen four inches above the ankle-joint, over the tibia, and apparently springing from that bone. As fluctuation was distinct, an opening was made into it, when only a small quantity of thin bloody serum came away. The mass of the tumour was found to be composed of soft cerebriform matter, having all the external and microscopic characters of medullary disease. As the patient was very anxious that her leg might be saved, if possible, and as the case was not pressing, notwithstanding the nature of the disease, Dr Johnston agreed to try what could be done in the way of removing the diseased mass alone. Accordingly successive poultices of chloride of zinc and flour were applied, so as to produce sloughing of the mass in layers. After the application of the third poultice, a thick slough came away, laying bare the origin of the disease, a small round tumour about the size of a walnut, attached to, and springing from the shaft of the tibia. As it was evident this could not be removed satisfactorily by caustics, in the middle of January last, the patient being put under the influence of chloroform, the entire mass was removed by the knife; and, immediately after this was done, the actual cautery was freely applied to the wound. The wound so made was dressed with water dressing, and got a chance to heal. The healing process went on well, leading to hopes that the disease had been eradicated, as it skinned entirely over. But it remained in this state only a short time. In the end of April it again became painful, and began to swell, and it was soon evident that nothing but amputation could lead to a successful issue. Taking into consideration the unfavourable results which generally follow amputation below the knee, not only where the operation is performed by the old plan of making a long flap from the muscles of the calf of the leg, but also where the circular method is adopted; taking also into account the fact that we had been provided by Mr Carden with an amputation at the knee-joint, which operation had the high authority of Mr Syme to recommend it, Dr Johnston determined to adopt the new plan of procedure in this case. Accordingly, on the 19th of May, the patient being at her own home, Dr Johnston, assisted by myself, performed the operation. A long semilunar anterior flap was cut—the incision commencing on one side of the joint, running round the joint one and a half or two inches below the lower margin of the patella, and terminating at the other side. The flap so made was dissected from the patella, and turned up as far as the ends of the incision on each side. The large tendon of the extensor muscles was then divided, and the joint opened into. The knife was then carried perpendicularly through the structures behind the joint, and the lateral and other ligaments divided, so completing the separation. The tissues were then separated from the femur for a short distance up, so as to allow the application of a saw, and a slice of the femur removed, taking away the uneven surface caused by the condyles. The bleeding vessels were secured by ligatures, the anterior flap laid over the end of the cut bone, and the wound united posteriorly by silver wire sutures. After this, recovery was very rapid. The patient was out walking, with the assistance of crutches, within three weeks after the operation, and the wound was entirely healed, excepting a small piece at one end, where the last ligature remained. After this came away, in a few days the wound healed entirely, and has remained sound ever since. Although she lived at a distance of sixteen miles, it was necessary that she should be visited—only three times after the operation. Five weeks after the operation, she had an attack of acute rheumatism, which somewhat retarded convalescence.

The entire length of the wound after it was healed was four and a half inches.

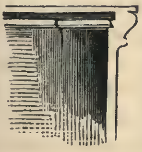
The principal recommendations of this new operation, so far as it has been tried, seem to be,—1*st*, The greater simplicity and ease of the immediate after-treatment. The change of the lint is all that is necessary, the cut surfaces keeping in apposition of themselves, so that any one can dress the stump. 2*d*, A smaller mortality than is secured in cases of amputation of the thigh. This, I believe, will be found to be the case, and is owing to the less mutilation which takes place under the new plan—the smaller extent of the cut surfaces—and the fact that the medullary cavity of the femur is not opened into, so avoiding danger from that fruitful source. *3d,* The preservation of a longer limb than is obtained by the old amputation of the thigh. *4th,* The quality of the stump resulting. The preservation of a long limb after an amputation is not always to be desired; but in this case I think it is, as, along with greater length, we have also a stump as good and as useful as under the old method.

The advantages which this amputation has over amputation below the knee also are apparent. Stumps resulting from amputation below the knee are rarely good, and even when they are good, we have in them three or four inches of limb, which portion is of no use, and too often is a source of great discomfort, from being in the way, being liable to ulceration of the integument covering it, etc.; and an artificial limb, when fitted to it, has to rest, not on the end of the stump, but on the patella, or against the femur, or groin. The result of the amputation at the knee-joint is a stump which seems to be well suited for an artificial limb. In cases of primary amputation, of course each case must be decided on it own merits, and the plan adapted to each put in force; but this operation seems peculiarly adapted to secondary amputations, including particularly those cases of disease of the joint in which only the surfaces of the bones composing the joint are at fault, for by removing the diseased articulating surface of the femur in this amputation all the benefit is got which could be obtained by removing a larger portion of that bone.

I have no doubt that additional experience will speak more strongly in its favour, and that it will come, in all cases where both are applicable, to supplant the old and feared amputation of the thigh.

ARTICLE IV.—*Note on Preparation Jars.* By LAWSON TAIT, Esq.

IN addition to the possession of a thorough knowledge of morbid appearances, it is of some importance that the pathologist should have the knack of being able to display them well and permanently for the benefit of others. In many museums there are sad instances, still in existence, of beautiful and rare preparations rendered worthless by the manner in which they are *put up*, and the most common cause is a faulty jar. It is bad economy to have a small jar; for if a preparation is worth preserving at all, it is worth being well seen, and for this purpose it must have plenty of room. The smaller the jar, the less evenly made is it, and the more dim; so that, for all the difference the extra spirit makes it is much better to have the jar three or four inches larger in every measurement than the preparation requires. Every preparation that already hangs in the hideous, old-fashioned, bladder-covered jar, it is worth while replacing in the more modern jar with the plate-cover. Not only for the reason, that frequently every view of a preparation is of importance (very often that from above is the best), or that the additional light gained is an advantage, but because it really would be a saving, as no amount of string, tin-foil, or paint, will prevent the spirit from evaporating. Some years ago, Mr P. Stevenson improved matters by designing the plate-covered jar with the brass ring, so that a preparation might be viewed from above; but this jar allows of great evaporation. Recently, jars with ground flanges, and covers which are cemented on, have come into use, and these obviate all the objections alluded to. The slip of whalebone, however, which is generally used for suspending the object, gets sodden and loses its *spring* after a very short immersion; so that if the jar should get a jerk, the preparation is very apt to fall to the bottom, necessitating that the jar be opened and the object rearranged. To prevent this annoying accident, I have got Kemp & Co. to make me a jar with a slender glass rod resting on narrow shoulders ground out of the flange, as represented in the woodcut. About the sixteenth of an inch must be allowed for play during variations of temperature. Should any points of the preparation require suspension which cannot be included in the axis of the rod, small glass hooks may be fastened to the cover in the required positions; the best cement to use for this purpose, and for fixing the covers, is that sold as Fowlke's, which is simply a solution of gelatine or isinglass, or both, in glacial acetic acid. This method of putting up a preparation has a neatness and airiness which at once recommend it to the eye. I have preparations which have been in jars closed in the method mentioned, for six years, and yet have not lost a drop of spirit by evaporation. Many interesting preparations lose their value by being *bleached* by the spirit. I have put up many preparations displaying hyperæmia which still retain their original fresh appearances—especially two showing rupture of the biceps from muscular action, and the congestion of the trachea and larynx after death by drowning, which still show, after four years, their original vivid colour. This is accomplished by warming the object very slowly before a fire. It must be done very gently, so as to warm the preparation through, and yet not to harden the surface which must be dry only. The preparation is then to be suspended, while warm, in a mixture of eight parts of the strongest spirit, and one part of chloroform. The jar should not be closed for twenty-four hours.



In putting up *dry preparations* the jars must be perfectly dry. Such objects, more especially bones, should not be varnished with shellac varnish, but with an amber varnish; that used by photographers, and known as Soehnne's, is the best, as it leaves no traces of its application and yet renders the preparation quite hard. It seems to resist fungi and acari completely.

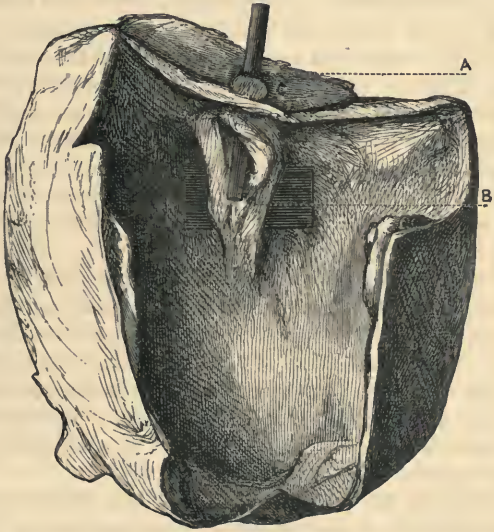
ARTICLE V.—*Encephalocele successfully Removed.* By THOMAS ANNANDALE, F.R.S.E., Lecturer on Surgery and Assistant-Surgeon, Royal Infirmary, Edinburgh.

IN May 1866, I was asked by Dr Mackenzie of Kelso to see with him an infant, æt. 7 weeks, who had been born with a large tumour springing from the back of his head At birth the tumour was nearly as large as the child's own head, and shortly afterwards was tapped by the mother's medical attendant, and a large quantity of clear fluid drawn off. The sac speedily re-filled, and was attacked with inflammation, which was followed by sloughing of the most dependant portion of the tumour, and an escape of purulent fluid, which has continued ever since. On examination I found a pedunculated tumour, the size of a large pear, hanging from the occiput. At the lower extremity of the tumour there was a ragged opening (large enough to admit the points of three fingers) which passed into a granulating cavity, nearly the size of a hen's egg. The remainder of the tumour was composed of solid textures, principally skin and areolar tissue, thickened and œdematous. The pedicle or neck was fully an inch in length, and almost as thick as an adult thumb. It felt solid in consistence, and was closely connected with an opening, which could be felt in the occipital bone immediately below its occipital protuberance, although no actual communication could be detected between it and the interior of the cranium. The opening in the occipital bone was about the size of a shilling. The scalp round the pedicle and the skin of the upper part of the neck hung loose,—a result of the original large size of the tumour. The cavity already referred to was suppurating freely, and the child's strength was beginning to fail very much from this cause.

On considering the circumstances of the case, both Dr Mackenzie and myself agreed that it would be right to remove the tumour, and thus get rid of the local irritation which was affecting the child's health so seriously. I therefore proceeded to do this by passing a needle with a double strong thread through the neck of the tumour about half an inch from the bone, and each half of the pedicle was firmly ligatured by means of this thread in the same way as an internal pile is secured. The pedicle was then divided immediately external to the ligatures. No bleeding took place from the divided surface, and the child apparently suffered no inconvenience from the operation. The ligatures gradually separated, and the child made a good recovery in spite of a sharp attack of measles, which came on a few days after the operation. Four months after the operation the child was perfectly well, and thriving, and presented no trace of the formidable disease from which he had suffered, except a small process of skin corresponding to the divided end of the pedicle.

Examination of Tumour Removed.

The tumour, as before stated, was the size of a large pear, and presented at its dependant aspect an irregular shaped opening, which passed into a granulating cavity. The cavity was completely closed, except at this opening. The transverse section of the pedicle showed it to be composed of skin and condensed areolar tissue. At the centre of the section, and nearer to the posterior wall of the tumour, there was a solid process about the thickness of a large quill, round in form, and of a whitish colour. The section of this process, although a distinct structure, was firmly adherent to the surrounding areolar tissue, and could not be separated from it. (See C.) Immediately at the side of the process, and running parallel with it, was a canal having the diameter of a crow quill. (See A.) This canal was pervious, and allowed a probe introduced into it to pass down towards the posterior wall of the tumour. Further dissection showed that the canal terminated in the apex of the process already mentioned.



Having stuffed the cavity, a careful dissection of the posterior wall of the tumour was made. The skin was first dissected off, but this was done with difficulty, owing to the adhesion which had taken place between it and the areolar tissue. Two or three layers of areolar tissue, all more or less matted together, were then removed, and the appearances which are illustrated in the engraving were met with.[1] In the centre of the posterior wall, at a point corresponding to the centre of the pedicle, there was a distinct pyramidal process (see B.) (a piece of whalebone has been passed under the process to show that it was unattached, except at its base and apex), the section of whose base has already been described in connexion with the pedicle. The base of the pyramid corresponded to the section of the process in the pedicle, its apex became merged in the posterior wall of the tumour. The process was about one inch and a half in length, was quite distinct from surrounding structures, and was only separated from the cavity of the tumour by a thin layer of tissue principally composed of granulations. The canal passed into the substance of the process, and terminated there. The structure of the process was soft and pulpy, and of a dirty white colour.

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[1] In the engraving the pedicle is made to appear much shorter than it was in the preparation. This is owing to the cut surface having been presented, in order to show the opening of the canal, and the section of the process. The process has also been laid freely open to exhibit the termination of the canal in it.

Microscopic examination (using a power of 200°) showed numerous minute granules, lying amongst which were multitudes of circular, or slightly oval, corpuscles (somewhat smaller in size than the red corpuscles of the blood), which had a bright appearance, and well-marked outlines. Not feeling sure of the nature of these corpuscles, I asked my friend Mr Turner to examine them. He reported that they reminded him very much of the small corpuscles found in the delicate connective tissue of the cerebrum. When a little water was added, these corpuscles became much paler, and their outlines less distinct. There were also some flattened band-like structures which branched and formed an irregular net-work. They presented no definite markings, and it was difficult to refer them to any particular structure. Mr Turner felt sure they were not nerve fibres.

Remarks.—Experience has shown that surgical interference in cases of encephalocele and spina bifida is very rarely advisable. The case just related illustrates a condition of the disease, which perhaps is the most favourable for an operation. An encephalocele or spina bifida occasionally undergoes a spontaneous cure by suppuration of the sac, and gradual obliteration of its cavity; and, no doubt, the great success of the operation in the present case was owing to the natural cure having commenced.

The pathology of the tumour is interesting, as showing,—

First, That although the canal or communication between the sac or cavity, and the interior of the cranium, may be closed, it may still remain pervious in the pedicle or neck.

Second, That a distinct process not composed of nervous elements may pass down from the interior of the cranium into the walls of an encephalocele.

It is, however, possible that this process was originally composed of nervous tissue, which had undergone some change in the progress of the case.

ARTICLE VI.—*Cases of Excision of the Knee-Joint, Illustrative of a New Method of after Treatment.* By PATRICK HERON WATSON, M.D., F.R.S.E., F.R.C.S.E., Lecturer on Surgery, Surgeon to the Royal Infirmary, and to the Chalmers' Hospital for Sick and Hurt.

(*Continued from page* 793.)

CASE I.—W. F., aged 24. A farm-servant from Ayrshire. Admitted to Chalmers' Hospital on the 24th May 1864, under Dr Watson's care.

This lad has suffered for four years from a chronic affection of the left knee-joint, consequent upon an injury received in the stable. From the account given by the patient, the joint appears to have been violently wrenched, then to have inflamed, and to have been greatly swollen. After a brief period of quiescence, which had permitted the first effects of the accident to subside, he had once and again attempted work, and as frequently been laid aside. This alternation of repose and work continued for some months, till he was obliged to give up his place, and, from want of means and friends, he had no resource but to become an inmate of the poorhouse. While there, after severe and long-continued suffering, an abscess formed in connexion with the joint, and discharged itself by lateral and posterior openings. This occurred about a year since.

On admission; *generally*, the patient is extremely feeble and anæmic. His appetite is voracious. He has lately suffered from diarrhœa, and is subject to copious night-sweats. He cannot sleep without opiates. *Locally*, the knee is bent at nearly a right angle. There are several openings from which purulent matter is copiously discharged, especially from a large aperture on the outer side. A probe introduced at this sinus comes in contact with the posterior surface of the condyles of the femur in a carious condition. The slightest effort to move the limb occasions excruciating pain.

After a few days' repose to recover from the exhausting effects of his journey, excision of the knee-joint was performed by the long semilunar flap operation. After laying bare the condyles of the femur, the patella was detached from between the condyles of the femur, by means of the bone-pliers, the flap of soft parts, including the patella, now turned upwards, was released from its attachments to the soft parts by a few sweeps of the knife. The articulating surface of the femur and tibia were then found partially anchylosed, and all traces of the crucial ligaments gone. The leg was accordingly forcibly flexed upon the thigh till the heel was brought in contact with the buttock. The extremity of the femur thus exposed, projected from the wound, and was seen to be riddled with worm-eaten cavities. The whole extent of disease, limited by a line passing through the spongy texture of the bone, just above the inner condyloid pit, was removed by a few strokes of a saw, provided with movable back. Space was now obtained to clear the posterior aspect of the head of the tibia, the knife being carried close to the bone, so as to avoid the popliteal artery. A thin slice of the head of the tibia was removed by the saw, applied from behind. The extent of bone removed was close upon an inch and a half, but rather less. The rough torn surface of the patella was smoothed by means of the gouge. Five vessels were secured. The limb was temporarily placed upon a common gooch splint, well padded, and secured in its place by roller bandages. The patient was carried to his bed, which had been meanwhile carefully arranged with a water-pillow and draw-sheet. To maintain repose two thick round sand-bags were placed on either side of the limb, which was kept in a state of rigid repose by broad pieces of flannel bandage. These were placed over the thigh and leg on four different levels, so that when carried beneath and around the sand-bags, and pinned in front, the weight of the bags secured the immobility of the limb. A large opiate was administered, so soon as the patient had sufficiently emerged from the chloroform as to be able to swallow. During the ensuing week he suffered severely from sickness and entire loss of appetite, only partially relieved by ice and bismuth. The retching and restlessness consequent on this unfortunate state of stomach, produced so much disquietude of the limb, and such a tendency to displacement of the osseous surfaces that the sand-bags were removed, lateral splints adjusted, and the limb and apparatus laid in the Salter's swinging-cradle. This afforded very great relief, permitting any degree of movement which was wanted, but was found inconvenient, from the necessity of taking off the side supports each time the dressings were changed. To obviate this, the movable gypsum apparatus, already fully described, was applied.

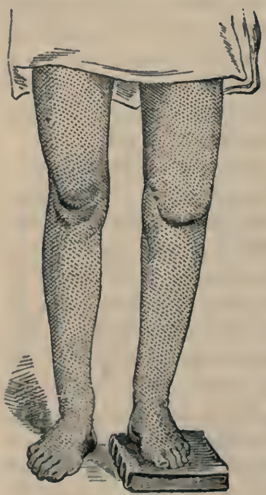


FIG. 5.—CASE 1.

8th June.—The sutures were removed, the entire surface wound having soundly healed. The only openings which remained being those of the old sinus.

The sickness has completely gone. His appetite is re-established—he is free from pain, and is usually sitting up all day long, with the sound limb hanging over the side of the bed. In the middle of July, after all discharge had ceased, except a drop or two in twenty-four hours, he was attacked with rigors, followed by some degree of swelling about the joint, and an increased amount of discharge from the sinus, which had nearly closed. This exacerbation was attributed to his catching cold, as he spent most of his time smoking in the lavatory of his ward. The abscess which resulted required careful management, numerous counter-openings having to be made. This unfortunate intercurrent inflammatory attack, although delaying the ultimate cure, did not in the least interfere with the satisfactory progress of the consolidation of the osseous surfaces, which in August was so complete as to enable him to move about without the employment of any support to the limb.

He was retained in hospital, however, till the 25th of January, till, in fact, he was fit for work, the sinus having entirely healed, and the limb having so regained its strength and muscular energy as to enable him to walk about with ease, without further auxiliary aid than a cork heel in the inside of his boot, corresponding in thickness to the amount of bone removed in the operation, and amounting to nearly an inch.

Case II.—A. M., æt. 30, a servant, was admitted into Chalmers' Hospital on 3d February 1865.

Patient states that more than a year ago she sustained a severe bruise of the right knee-joint, in consequence of a fall upon the stair in carrying coals. This occasioned considerable pain and swelling at the time, but with repose, and some degree of relaxation of work, she continued in her place till the ensuing term. The pain has, however, never altogether subsided, and she walked very lame all that time. She states that the pain has always been increased by exercise, change of weather, or alternations of temperature, and that she has frequently been obliged to spend whole nights sitting up in bed through sheer pain. After leaving her place, finding herself incapable of work of any kind, and being destitute of means or friends, she was obliged to seek parochial relief. She was then admitted to the workhouse, and was transferred to the sick ward of the establishment. There she had remained ever since, with the exception of two occasions, when she had been placed under surgical care elsewhere; but as the only relief then suggested consisted in amputation, she refused to submit, and returned to the parish infirmary.

When admitted to Chalmers' Hospital, she was weak and bloodless, much emaciated, though cheerful and full of hope. Her appetite was good, but her sleep at night was broken in consequence of the pain in the knee. The whole limb was much shrunken, the knee was flexed at nearly a right angle, and firmly anchylosed in that position. The slightest pressure over the head of the tibia, or inner condyle of the femur, gave rise to very great pain.

She was quite ready to submit to any treatment except the removal of the limb.

As her condition was as favourable as it was likely to be at any future period for the operation, and as her sleepless nights demanded some speedy relief to suffering, the knee-joint was excised upon the 8th of February. The operation was practised in the method usually employed by Dr Watson. That is to say—having been fully put under the influence of chloroform, the patient was placed upon the operating table, and the sound limb being confined by means of a towel to the leg of the table, the tourniquet was applied. An incision was then made, commencing above and a little behind the external condyle of the femur, and, with one curvilinear sweep brought down towards the tuberosity of the tibia, then onwards and again upwards, when it terminated at a point exactly opposite to its commencement, and on a level posterior to the inner condyle. This flap having been dissected up as far as the lower margin of the patella, that bone was detached by the bone-pliers from its anchylosis with the condyles of the femur. The adhesions between the posterior part of the articular surface of the femur and head of the tibia were now broken up by an assistant forcibly flexing the leg upon the thigh till the heel touched the buttock. The condyles of the femur now projected from the wound, and were sawn off. The posterior margin of the head of the tibia being cleared for the saw, had a thin slice removed from behind forwards. The bleeding was very copious, but readily arrested by ten needles. No sutures or dressing of any kind applied to the edge of the wound. The limb was immediately adjusted upon the posterior splint, and the iron rod applied with gypsum bandage.

In the evening, as all oozing had ceased, the requisite wire sutures were introduced. The needles were not withdrawn till the next day at the hour of visit.

13*th February.*—Patient keeps well, perfectly free of pain, and which she somewhat hyperbolically expresses, by saying she is in heaven. A little serosity escaping from the inner angle of the incision, amounting to about an ounce in the twenty-four hours.

16*th.*—There is still about a tablespoonful in the day. This fluid has daily been examined by the microscope by my most attentive and painstaking resident surgeon, Dr Coull Mackenzie, and no trace of pus can be detected in it. No dressing.

18*th.*—The quantity of fluid oozing from the wound is much diminished, and to-day (eleventh from the operation) is found to contain a few pus corpuscles.

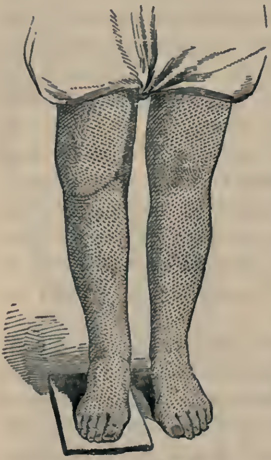


FIG. 6.—CASE 2.

20*th.*—Fluid still scanty. Contains microscopic traces of pus.

22*d.*—Discharge has entirely ceased.

28*th.*—Patient well. No discharge since the 21st instant.

10*th March.*—There is neither pain nor redness about the knee; the soft parts feel firm, and pressure occasions no uneasiness. The patella is firmly fixed to the condyles.

31st March.—The last of the crusts which had formed along the line of incision fell off to-day, leaving the whole line of the incision soundly healed.

10th April.—To-day the whole apparatus was removed; the anchylosis was found perfect. The apparatus has never been touched since its original application to the limb, sixty-one days ago.

The shortening of the limb is less than one inch. This is determined by the thickness of the book required beneath the heel to set her straight upon her limbs. This patient has again returned to service, and I lately saw her walking along the street at a pace of more than three miles an hour.

Case III.—M. M'D., æt 30. Admitted to the Royal Infirmary on the 28th of June 1866, on account of acute disease of the left knee-joint.

This patient states that for some time past she has suffered severely from pain in the knee, from which she has obtained relief only by the administration of large opiates. She is in a state of great nervous excitement, due partly to pain and feverishness, but also in part to taking morphia, and perhaps to other habits of intemperance. The knee-joint has become greatly swollen within the last few days, previously throughout the whole progress of the case there has been very little swelling. When I saw the knee for the first time, upon the 29th of June, I found the patient sitting up in bed clasping the thigh firmly between her hands to prevent the muscular twitchings, which are constant. The slightest effort to move the knee causes frightful pain, and gives rise to a sensation of rough grating of two exposed bony surfaces. The patient was put on antimony and morphia, and the operation of excision arranged for the following day.

30th June.—Excision of the joint performed in the ordinary manner. In turning up the flap of soft parts to expose the bones, a quantity of turbid serum, containing flocculi of purulent lymph, and portions of cartilge, escaped. A large mass of fawn-coloured lymph was contained in the upper part of the articulation. The crucial ligaments were gone, and both osseous surfaces completely exposed and spicular. The extent of bone removed from the femur and tibia did not exceed an inch. Bleeding stopped by five needles. Dr Watson's immovable apparatus applied, and the patient removed to bed. A large opiate given as soon as she could swallow.

In the evening the edges of the incision were approximated by wire sutures. Brandy and ice ordered to check the incessant vomiting, which had induced great depression of the circulation.

1st July.—Patient passed a tolerable night, and had some sleep, in spite of the vomiting, which, when awake, is nearly constant. Pulse 120.

10th July.—The vomiting continued with little abatement for three days, the pulse ranging from 100 to 120. Nothing staid upon the stomach, and her restlessness and exhaustion were so extreme that death was apparently imminent. The vomiting resisted the employment of all remedies, but has now gradually subsided since yesterday, when a blister was applied to the epigastrium, and morphia administered, in the form of one quarter grain pills, repeated so frequently as to maintain a narcotic effect. The gradual subsidence of the swelling of the limb, withdrawing it from close contact with the apparatus, together with the constant restlessness, has produced some degree of displacement of the thigh bone. The whole apparatus was accordingly removed and re-applied.

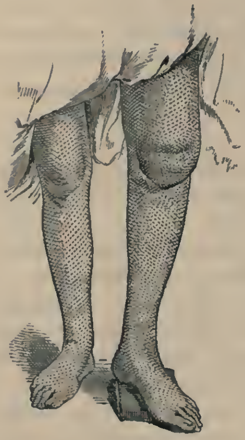


FIG. 7.—CASE 3.

20*th*.—There is still great gastric irritability, which is kept in check by repeated doses of the effervescing citrate of potash and hydrocyanic acid. The knee is, however, progressing rapidly—there being very little discharge in the course of the twenty-four hours. All that there is escapes by the dependent angles of the wound, while the line of incision in front is perfectly and soundly healed.

8*th August.*—Wound all but healed. Patient is now out of bed throughout the whole day.

1*st September.*—Apparatus removed. Wound completely healed. When moving about the ward, to have lateral leather splints applied, and secured with straps. From this period she commenced the use of crutches; and although retained in the hospital till the 25th November, because she had no home to which to go, she might have been dismissed in September, as she was perfectly well, and the limb healed and consolidated. The shortening amounts to a little over an inch.

CASE IV.—C. M., æt. 21. Admitted to Dr Watson's ward in the Royal Infirmary, on account of disease of the left knee-joint, 1st August 1865.

She states that she has suffered from disease of this joint for more than two years. The disease commenced in the soft textures, accompanied with swelling and stiffness of the articulation. These symptoms increased, and pain became superadded. Latterly the pain has been very great, attended with loss of rest, and starting of the limb. The joint is partially flexed, and the ham-string tendons rigid. The slightest attempt to move the joint, or any pressure over the condyles of the femur, gives rise to great agony. She had twice previously been in hospital, when, by repose, the employment of Scott's dressing to the joint, and constitutional treatment, every effort had been made to check the progress of the disease. She is now, so far as her general health is concerned, losing ground daily, in consequence of irritation, loss of rest, and impairment of the appetite. She is extremely anxious that something should be done to relieve her from the disease, and so restore her as to enable her to follow her occupation as a dressmaker.

Excision of the joint was proposed, to which she gladly assented.

8*th August.*—The joint was to-day excised in the manner usually adopted by Dr Watson, by means of a long anterior flap retaining the patella. Fully an inch and three-quarters of bone was removed. These vessels were secured by acupressure. The apparatus applied, and the patient removed to bed. In the evening, as there was no oozing, the acupressure needles were withdrawn, and the flap secured *in situ* by several points of interrupted wire suture. For the next twelve days, as there was some slight redness of the skin, simple cold water dressing was applied. The patient was quiet, and comfortable, free of fever, and able to take nourishment of a light kind freely.

20*th.*—The entire line of incision has healed, except at the ultimate *cornua,* from which some purulent discharge still escapes.

6*th September.*—Lateral apertures reduced to mere points, from which not more than a speck of discharge escapes in the course of twenty-four hours. Dry dressing.

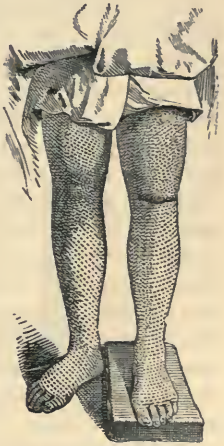


FIG. 8.—CASE 4.

18*th.*—Wound entirely healed. Apparatus removed. Lateral leather splints applied. The patient desired to move about with crutches.

25*th October.*—Can walk with little or no inconvenience without the assistance of crutches, a thick pad being placed beneath the heel.

5*th January.*—Supplied with a boot, the cork heel inside measuring perpendicularly one inch and a half.

CASE V.—G. M., æt. 18, a clerk. Admitted to Chalmers' Hospital, 19th September 1865.

In early life he states that he had suffered from disease of his right knee-joint. He had recovered from this after protracted suppuration. The joint was, however, ever since, more or less tender, and as it was bent to nearly a right angle, he was unable to move about without the assistance of a crutch. As this state of matters forms a serious obstacle to his occupation, preventing his promotion in the government office with which he is connected, he is anxious to have anything done which will secure a useful and a straight limb

His general health is good, his complexion florid, his appetite excellent, and in the meantime he has no pain, and sleeps soundly.

The articulation on examination was found to be firmly anchylosed, the patella fixed to the condyles of the femur, and the hamstring tendons rigidly contracted. There was some degree of increased temperature of the soft parts around the joint, and pressure over the osseous prominences, or jerking of the limb, gave rise to great pain. In these circumstances excision was determined upon as preferable to forcible rupture and extension of the anchylosed limb.

28*th September*.—To-day, Dr Watson excised the articulation in the ordinary way. The soft parts were found greatly condensed, and adherent to the hard tissues. The cartilage of incrustation entirely gone, and the anchylosis so firm that it required to be partially sawn through before the condyles of the femur could be exposed, by forcibly bending the limb for the application of the saw. The extent of bone removed did not exceed an inch and a quarter; the amount of the head of the tibia to be taken away was determined after the condyles of the femur were removed by the tension experienced on attempting to straighten the limb. The bleeding chiefly came from the surface and margins of the flap; the vessels were secured by acupressure. The limb was arranged in the ordinary manner, with splint and rod; the gypsum bandage was immediately applied, and the patient conveyed to bed.

In the evening, as a good deal of oozing had been going on all afternoon, the flap was raised, a large coagulum removed, and the surfaces left exposed for an hour or two. At the end of this time, as all bleeding had ceased, the acupressure needles were withdrawn, and the interrupted metallic sutures introduced.

29*th*.—He has been very sick all night, and is quite unfit to take any food. To have ice and soda-water.

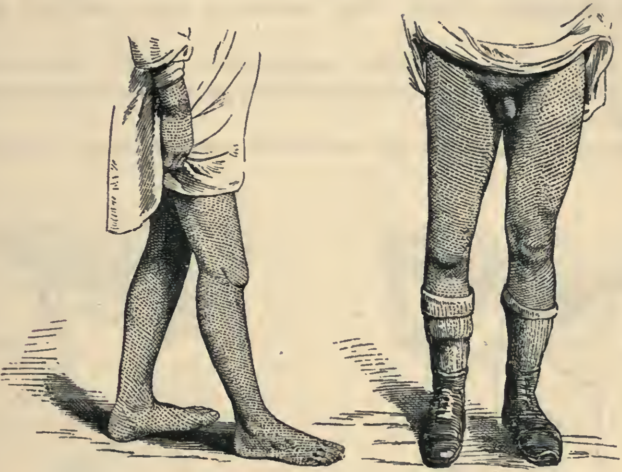
30*th*.—Had a slight rigor, which speedily passed off. The sickness has quite gone, and his tongue is clean and moist. To have five grains of quinine twice a-day.

15*th October*.—He has progressed rapidly since last report. The sutures were removed to-day, the wound having entirely healed, except at the angles, from which a very scanty discharge, scarcely sufficient to stain the dressings, escapes in the course of twenty-four hours.

30*th October*.—The plaster of Paris apparatus removed, and lateral splints substituted. To move about with crutches, as union is tolerably firm.

20*th December*.—He has been perfectly well for more than a month, walking about with the aid of a stick and one crutch, but always complaining of a sharp pricking pain at one spot, at the lower part of the semilunar flap. A few days since, he complained of feeling shivery and feverish; a reddish blush appeared over the front of the knee. This was relieved by a dose of purgative medicine and saline diaphoretics, but there is a small abscess formed in the line of incision, over the head of the tibia. On opening this about a teaspoonful of pus escaped.

21*st.*—He says that on removing the dressing this morning he felt something hard in the opening. On examining this I found the twisted wire, which is attached to the eye-end of an acupressure needle. On laying hold of it with a pair of dressing forceps, and exerting considerable force, I withdrew an acupressure needle, which must have been interposed between the osseous surfaces since the day of the operation.



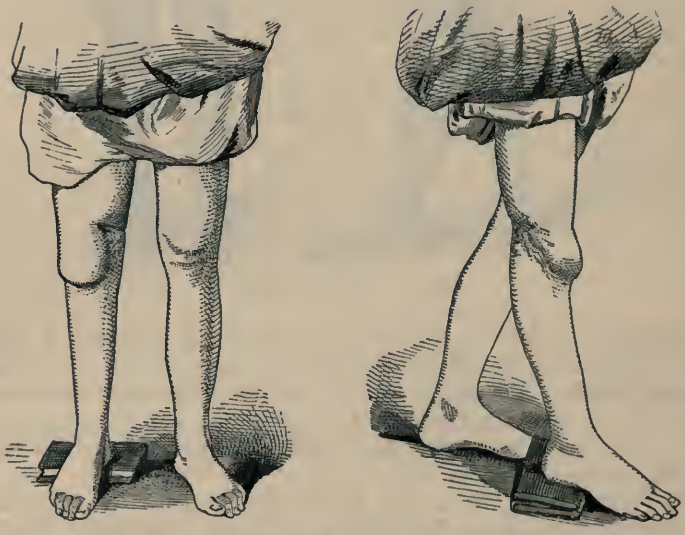
FIGS. 9. and 10.—CASE 5.

30*th January.*—The wound is soundly healed. He can walk with perfect ease, with a high heel in the inside of his boot. To-day he walked more than half a mile to the photographer, to have a portrait taken of his limb.

CASE VI.—F. M‘P., æt. 26, dressmaker. Admitted to Chalmers' Hospital, 25th April 1866.

Patient states that about seven years ago she sustained a severe fall, which produced a dislocation of the knee-joint. This luxation was satisfactorily reduced at the time of the accident, and after a period of rest she was again able to move about. The joint has, however, since that time been stiff, and of late has become increasingly painful, and more and more contracted. About five months ago, abscesses formed around the knee, discharging themselves laterally and posteriorly by three openings. The whole limb is shrunken, and she is incapable of employing it for progression. There is a considerable degree of puffy swelling in the interval between the condyles of the femur and the head of the tibia. The temperature of the soft parts over the joint is increased, and pressure over the knee at any point, or an attempt to move the limb, however gently, gives rise to great pain. There are three openings, two on the outer side, one upon the inner side of the joint, from which matter is copiously discharged. Her general health has failed. She has lost flesh rapidly of late. She sweats at night, and her appetite is gone.

2d May.—The articulation was excised by Dr Watson in the usual manner, the limb placed in the apparatus he employs, and the patient removed to bed.



FIGS. 11 and 12.—CASE 6.

During the ensuing six weeks all went on most satisfactorily, and the wound, healing rapidly, had become reduced to two small apertures, from which scarcely enough discharge came in the interval between the dressing to make a mark on them; while, for a fortnight, the patient had been out of bed all the day long, sitting in a chair with the limb and apparatus supported on a bench.

15th June.—A patient lying in the adjoining bed has had an attack of sloughing phagedæna of the side and flank, following the removal of a fatty tumour from this part, from which she is only now convalescent. To-day, the knee-joint has assumed a swollen puffy appearance, the skin is glazed, and of a dusky red hue around the sinus upon both the outer and inner side of the joint, and a curdy offensive discharge issues from the opening. She complains of feeling cold. Her pulse is quick and feeble, her face flushed, her tongue coated, and she has no appetite. To have an emetic, followed by an opiate, and to commence as soon as the sickness has abated the use of iron in effervescing draughts internally, while the pure Condy's fluid is applied over the knee.

For the next fortnight the phagedænic ulceration spread, opening up the whole line of incision, which gaped widely, disclosing the osseous surfaces covered with a white pulpy substance. The ulceration next spread towards the outer side of the popliteal space, exposing the external hamstring tendons, and threatening the central part of the space. Chloride of zinc solution was applied by means of balls of cotton wool soaked in Sir William Burnet's solution. These were introduced into the gaping wound, and built up one on another till it was entirely occupied with them; a bandage was then applied, to keep the mass in its place.

That evening the patient passed a quiet night, sleeping soundly, and awaking refreshed and hungry for food.

7th September.—Since the separation of the slough caused by the caustic, the wound at once assumed a healthy aspect, and has cicatrized rapidly. The patient's strength is quite restored, the apparatus has been removed, union of the osseous surfaces is complete, and without the aid of crutches she can walk about the ward. Lateral gutta percha splints are still, however, applied to the limb when she moves about, as a precautionary measure. The shortening is very slight, and measures exactly one inch.

Case VII.—H. N., æt. 22. Admitted to the Royal Infirmary on the 22d of December 1863, under Dr Watson's care, on account of gelatinous degeneration of the synovial membrane of the left knee-joint.

The disease had continued for several years in the chronic stage, attended with no pain, but characterized by gradually increasing swelling and stiffness of the articulation. Latterly she has suffered occasional attacks of pain, which has subsided under rest and counter-irritation. The knee is generally enlarged, of an ovoid shape, the thigh and leg being greatly wasted. She complains of pain on the slightest movement, upon pressure, and during the night from spasmodic jerking of the muscles of the limb. Her pulse is excited, her tongue coated behind, and red at the tip. Appetite very capricious, and liable to attacks of diarrhœa. For some time she has suffered from nocturnal diaphoresis.

The limb was placed between lateral bent leather splints, moulded to the outline, and clothed with chamois leather, so as to maintain perfect repose. To have quinine and acid before meals, and morphia at bedtime.

10th January.—She has generally been much relieved since her admission; but the condition of the articulation has changed for the worse, in so far as the swelling is considerably increased by the effusion of fluid into the cavity of the joint. The limb was laid upon a wire cradle splint, and the knee covered with hot lead and opium fomenting cloths.

1st February.—Since last report some improvement manifested itself. The fluid became absorbed, in great part the pain subsided, and the general health began to improve, with coincident gain in flesh.

28th February.—Shortly after last report the joint again became more painful, and the swelling was reproduced. The pain has steadily increased. Now the slightest movement, even coming roughly against the bed, causes severe agony. The ligamentous apparatus is so relaxed that lateral movement, accompanied with a rough grating of the osseous surfaces, can be distinctly felt. A troublesome tickling cough has annoyed her for some nights, and the expectoration is streaked with blood. There are symptoms of softening tubercle in the left apex. The propriety of operative interference, which had previously been rejected by her, was again insisted upon. The operation of excision of the articulation was finally determined upon.

4th March.—To-day, resection of the knee-joint was performed in the usual manner. The patella was left, its roughened surface having been removed by the gouge. The extent of the articular surfaces removed amounted to rather less than an inch and a quarter. A large abscess, situated on the outer side of the limb beneath the vastus externus, was at the same time freely opened, both in communication with the excision wound, and also by a free and dependent counter-opening at its upper part through the skin. There were five vessels secured. Metallic sutures introduced. A gooch splint was applied behind the knee, extending from the tuberosity of the ischium to the heel, and secured in position by roller bandages, leaving the site of the operation free. After being removed to bed, the limb was kept in position by means of lateral sand-bags.

Vespere.—Constant vomiting; pulse 120. Ordered ice, brandy, and morphia.

5th March.—Sickness not so constant; some sleep; pulse 100; limb easy. To have essence of beef and brandy throughout the day in small quantities. Water dressing applied over the line of incision.

12th.—Patient has progressed favourably till to-day, when the sickness returned. The mouth and throat covered with aphthous spots. She has a sunken look, and quiet rapid pulse. To have bismuth in scruple doses, with hydrocyanic acid. To be fed on milk and lime-water.

13th, *14th*, *15th.*—Sickness continues unabated. To have gr. v. of nitrate of silver in an ounce of distilled water, immediately. To have brandy in ærated lime-water, and essence of beef, as she can take them.

16th.—Sickness entirely checked. Aphthous patches on mouth and throat quite gone. The pulse has fallen to 100. Splint, which has become soiled with discharge, removed, and freshly re-applied. The wound looks healthy, though weak.

18*th*.—Diarrhœa has set in. Pulse 130; very feeble. To have hot fomentations over the bowels, lead and opium pills, and as much brandy and essence of beef as she can take.

19*th*.—Quite collapsed at the hour of visit, and is unable to take nourishment or medicine. Sank gradually, and died at 6 P.M.

CASE VIII.—Miss I. A., æt. 52. I was requested to see this lady on the evening of the 9th September 1864, having come to town to place herself under my care, on account of an affection of the knee-joint.

She states that about eighteen months ago she sustained a fall, and twisted or bruised her knee, and from that time onwards she has suffered from pain, which has been more or less severe. She was at first treated with rest and hot fomentation, then with blistering, and splints with firm bandaging. During this time she was able to move about with crutches, but latterly the pain has been too severe to admit of this, having been almost completely deprived of her night's rest through excruciating agony. The limb is slightly bent and quite rigid, the thigh and leg completely atrophied, the knee apparently enlarged to a great extent, which was, however, considerably greater in appearance than reality; there is, however, some fluid around and beneath the patella. The patient is extremely exhausted, very thin and meagre, with a dry skin, and pulse constantly about 100. Suffering extremely from dyspepsia. To have a mild nutritious diet. Bismuth, with charcoal throughout the day; mindererus and morphia at bed-time. The limb to be confined in splints, and the recumbent posture to be maintained.

3*d October*.—The condition of the patient has greatly improved, so far as her constitutional condition is concerned. She sleeps well. The pulse has fallen to 76. Her appetite is fair, and the dyspepsia is relieved. The state of the knee is much the same.

12*th November*.—Has remained much as before till within the last ten days. She complains of a considerable increase of pain, and there are two swellings situated on either side of the knee, one over the outside of the head of the tibia, the other behind the head of the tibia, which manifestly contain purulent matter. In these circumstances it was determined to perform excision of the joint.

16*th November*.—Excised Miss A.'s knee in the usual way. No vessels required to be secured. Abscess behind the head of the tibia opened where it pointed. The usual apparatus applied, a portion of the splint being cut away behind in the inner side to allow of a free drainage from the abscess.

Vespere.—Free of pain; has slept well all afternoon; free from sickness, but complains of flatulence.

24th December.—The wound is now almost entirely healed; the two cornua alone remain open, but the quantity of discharge does not amount to a quarter of a teaspoonful in the forty-eight hours. There is no pain, and for the last ten days she has been able to be in the drawing-room, sitting on a couch all day, busied writing, reading, or working. The only complaint she has to make is, that the acidity of stomach and flatulence are very troublesome. She is, however, greatly relieved by bismuth and Belloc's charcoal.

5th February.—Miss A. has made steady progress, able to move from room to room with her crutches. Sleeping well at night, and in all respects going on most satisfactorily till to-day, when she was suddenly attacked with a fit of colicky pain in the region of the caput cœcum coli, followed by violent diarrhœa.

Vespere.—This has been checked with hot turpentine stupes and chlorodyne.

8th.—Diarrhœa has again commenced to-day with abated violence, and has assumed a dysenteric type; great pain along the whole of the colon. Lead and opium pills every four hours.

12th.—Diarrhœa excessively fœtid; pain continues. To have morphine pilules and the carbolate of lime in pills.

16th.—Diarrhœa, accompanied with greatly increased pain along the course of the colon, passing blood in every motion. Evidently sinking fast. Died towards evening.

A post-mortem examination showed the remains of old tubercular disease of the lungs, and of the mesenteric glands, a waxy liver, and ulceration of both the large and small intestine. The joint was removed for more careful examination, when union between the osseous surfaces was found to be complete. This consisted of a granulation material undergoing ossification, which was furthest advanced at the margins; all the discharge within the last period must have come from that portion of the cavity where the abscesses had originally formed, and with which the sinuses, which discharged matter alone, communicated.

CASE IX.—I was requested, on the 15th of July 1864, to see R. S., æt. 18, who had long suffered from disease of the left knee-joint.

I found him with the limb confined in a box, and enveloped in a rude many-tailed bandage, in the fashion of Scott's dressing, but in which the medication consisted, as I was told, of Archangel tar. This had been applied on the recommendation of some local quack. There was no pain in the joint so long as the limb was kept at rest and no effort made to move the articulation, or to rest any weight upon the foot. Pressure, however, over the head of the tibia or condyles of the femur, elicited a complaint of very great and intolerable suffering. The joint was generally swollen, and swelling and stiffness constituted the commencement of the disease two years ago. The patient was thin, tall, pallid, and anæmic, in spite of living in the country, and sitting constantly when the weather admitted of it in the open air.

I suggested the propriety of operative interference with as little delay as possible, as suppuration was manifestly impending. To this the friends objected, desiring delay for consideration. In the beginning of August his friends, having considered the subject, consented to the excision of the joint. He was accordingly brought into town, and placed under my care in the Infirmary.

10*th August.*—To-day, the joint was excised in the usual manner, leaving the patella. A large abscess on the outer side of the lower part of the thigh was freely opened, both externally at its most dependent part, and also into the excision wound. Immediately thereafter, the ordinary apparatus employed by Dr Watson was applied. No dressing of any kind was applied to the site of the operation, the wound being left open until evening.

In the evening, the oozing having ceased, the flap was laid in position, and retained by means of sutures. He is very sick and depressed from the chloroform; to have six ounces of brandy during the night.

12*th.*—Sickness still continues, but the depression has quite passed off. Wound adhering throughout its entire superficial extent.

16*th.*—Some redness of edges of flap, and a little pus escaping at the angles posteriorly. Water dressing applied.

5*th September.*—The entire wound is healed, with the exception of the openings at the angles, from which a small quantity of pus is still discharged. He is daily out of bed for some hours.

30*th October.*—Since last report he has had several small limited abscesses forming here and there in the thickened synovial textures, and has suffered from an attack of erythema, followed by a phagedenic state of the sores. The abscesses were evacuated by incisions; the phagedenic surfaces treated by means of strong nitric acid, and the deranged state of system with mindererus, and afterwards with iron and taraxacum.

17*th November.*—The openings corresponding to the abscesses still continue to discharge. A probe passed in at them fails to discover anything else than tracks through the thickened synovial tissue. This has been treated, first, by repeated blisters; and, latterly, by freely laying open all the sinuses.

7*th December.*—He is very much better in general health, the swelling of the knee is nearly gone. There is still, however, some discharge from the sinuses posteriorly. The osseous surfaces have consolidated, and there is neither lateral nor antero-posterior motion. The limb is kept between lateral splints, and he is encouraged to move about with crutches. To return home.

22*d September* 1865.—He was placed under my care in Chalmers' Hospital, the sinuses still continuing unhealed. The osseous textures do not communicate with the track of the sinuses, but these latter all pass towards the patella.

12th October.—Having administered chloroform, Dr. W. laid open the sinus upon the outer side of the limb, which seemed to communicate most directly with the parts beneath the patella, and introduced his finger to explore the condition of parts. He found the patella covered with a granulation membrane, and in no part exposed; Chassaignac's drainage tubing passed along all the sinuous tracks.

1st November.—The swelling is now almost gone, the discharge from the tubes is daily getting less.

16th.—Alarming hæmorrhage suddenly occurred from one of the sinuses, and had gone on to a great extent before its existence was recognised by any one, the patient himself being sound asleep. This was checked by plugging the wound with dry lint and applying a bandage.

20th.—The bleeding has recurred on the 18th, 19th, and again to-day. It is obviously of arterial origin, and comes, Dr Watson believes, from the anterior tibial artery, either before or after it perforates the interosseous membrane. The bleeding has only been staunched by plugging with lint soaked in the solution of the perchloride of iron, and retained in position by a bandage.

21st.—Bleeding still going on. The patient greatly exhausted. Dr Watson, in considering all the circumstances of the case, determined to amputate without further delay. This was executed in the lower third of the thigh. Acupressure employed.

22d.—Patient has rallied wonderfully; he is squeamish from the chloroform. Acupressure needles withdrawn.

Vespere.—Bleeding has taken place, the cavity of the stump is distended, and blood and clots come oozing and rolling out between the sutures. Dr Watson opened up the stump, cleared out the clots, and found the face of the stump perfectly dry, but the oozing coming from the side of the bone. On sponging away the blood, the femur was found to be stripped of its periosteum for more than four inches,[1] and the bleeding seemed to come from the inner surface of the periosteum. To reach the bleeding point, Dr Watson extended the external angle between the flaps upwards, dividing periosteum and muscles in the incision as well as the skin. The bleeding was then found to come from the *nutrient* artery of the bone. This was secured by means of a needle, when all bleeding at once ceased. The denuded portion of the shaft of the femur was then removed with the saw, the flaps brought together by means of sutures, and the stump supported by means of pads of lint and a roller bandage.

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[1] This stripping of the periosteum from the bone is no uncommon occurrence after amputation of the thigh in scrofulous or cachectic patients. I have seen entire denudation of the shaft of the femur follow the excision of its head and neck. Its occurrence after excision of the knee has been put on record by Mr Syme, as occurring in one of his two original cases: "On the 6th January, in order to prevent displacement of the bones, which all our efforts had been insufficient to effect completely, I cut away about two inches of the femur with the pliers, *and then observed*, with much concern, that the bone was denuded beyond the farthest extent to which my finger could reach."—(Syme on Excision of Diseased Joints, p. 139.) So far as I am aware, however, the occurrence of copious bleeding in such cases from the nutrient artery of the bone has not been recorded.

23d.—Patient very weak and exhausted all night and morning, but rallied a little this forenoon. Ordered small quantities of champagne to allay the sickness. Towards evening he began to sink, and died about 9 P.M.

His friends did not permit any examination of the body. But on dissecting the amputated limb, my house-surgeon, Mr Hamilton, found that my surmise as to the anterior tibial artery being the source of the hæmorrhage was correct, the aperture in the vessel exactly corresponding to the point where it penetrates the interosseous membrane. The whole extent of surface of both femur and tibia was united together by granulation substance undergoing ossification from the margin towards the centre.

CASE X.—I was sent for on the evening of the 24th of January 1866 to the country to see Mrs E. S., æt. 32, who, in a fit of temporary insanity, had precipitated herself, from a window some 25 feet above, upon the pavement in front of the house. I found that, in addition to several other injuries, she had sustained a compound comminuted fracture of the right patella, the fragments of which were driven into the cancellated substance of the articulating end of the femur.

Having laid the limb upon a well-padded splint, and supporting her on a mattress, I had Mrs S. brought into town, and placed in Chalmers' Hospital.

As she was in a very collapsed state, and some doubt existed as to the extent of her other injuries, Dr Watson deemed it expedient to do nothing further till he saw her again at the hour of visit on the following day.

25th January.—Reaction has taken place. Her hallucinations have all passed off. She is perfectly calm and collected. The state of the knee-joint having been represented to her, she at once acceded to the proposal to excise the articulation.

This was done by means of two semilunar incisions, including, between them, the patella and the lacerated wound lying over the bone. The extremities of the two incisions met together over the outer and inner condyles, and were then extended backwards as a single incision to the level of the posterior aspect of the joint. The articulating surface of the femur was alone removed, the tibia, being uninjured, was left untouched; the inter-articular cartilages were also left attached to the head of the tibia. The limb was put up in the mode usually adopted by Dr Watson.

Vespere.—The edges of the incisions approximated by means of sutures.

26th.—Patient passed a tolerably easy night, and is now quite free from all pain in the knee. The acupressure needles removed.

30th.—Continues very well indeed. Some purulent discharge escaping from the angles of the wound; the central part entirely healed. Pulse feeble.

31st.—To have wine, four ounces. Brandy and water at bedtime.

10th February.—Everything has gone on most satisfactorily till to-day, when Mrs S. was suddenly seized with rigors, followed by feverish reaction and copious sweating. To have five grains of quinine, twice a-day; opiate at night.

16th.—She has had the rigors and fever with sweating repeated daily since last report. Her breath has a sweet odour, like newly mown hay. She has a sunken look, and no appetite for food. A violent attack of pain seized her to-day, about 11 o'clock. She complains of it chiefly in the lower part of the right side, shooting up to the shoulder, so that she cannot draw a long breath without it catching the breathing. Over the lower part of the right side of the chest there is a distinct friction sound recognised. To have a blister applied over that side, and to have iodide of potassium in five-grain doses, three times a-day. Continue the quinine and opiate.

20th.—The pain was greatly alleviated by the employment of these measures. Mrs S. has become distinctly icteric. She sleeps the greater part of the day, and is more or less delirious every night. Omit the medicine. Continue nothing but nutriment.

24th.—Has lain since last report in a semi-comatose state, passing her urine in the bed. Pulse feeble and rapid.

26th.—Died comatose in the afternoon.

The post-mortem examination revealed recent pleuritic adhesions of both sides, especially of the right, with considerable effusion of serum. There were numerous metastatic abscesses throughout the substance of both lungs, especially of the right.

On the upper surface of the arachnoid there was a collection of a jelly-like substance. The other organs were healthy. The parts engaged in the excision were in a most healthy condition, but quite ununited.

CASE XI.—I was requested, in the beginning of May 1865, to see C. W., æt. 24, clerk. He has suffered from disease of the knee-joint for many years, which obliged him at an early period to relinquish business, and turn his attention to the occupation of a clerk. For a time there was no change for the worse in his symptoms, but within the last six months the joint, in addition to being swollen and stiff, has been painful, so much so, as almost entirely to prevent his moving about, or to admit of sleeping at night.

30th May 1865.—On admission to hospital, the whole limb was found to be atrophied. The knee generally swollen, presenting an ovoid spindle shape between the emaciated thigh and leg. There is severe pain experienced when any attempt is made to move the articulation, or pressure is made over the articular ends of the bone. The patella floats, and the capsule of the joint is distended with fluid.

Blisters were applied on either side of the joint, and as soon as the blistered surface permitted, a bandage was applied around the articulation. The whole limb was confined in bend-leather splints, moulded to the limb and lined with chamois leather.

14th June.—All pain is gone, but the quantity of fluid in the joint remains undiminished.

20th.—The most complete repose has been maintained since last report, while tincture of cantharides and iron have been administered internally. The fluid effusion continues unabated. To-day Dr Watson tapped the joint with a hydrocele trochar, and withdrew about a pint of turbid serosity. Nearly an ounce of tincture of iodine was then injected into the articulation.

21st.—The swelling this day at visit is reproduced to the full extent. The heat of surface over the joint is increased, and the skin is suffused of a reddish colour. To have an evaporating lotion applied.

26th.—Swelling has gradually diminished since last report. The temperature of the skin and the redness have also subsided.

8th July.—The swelling is now nearly gone. What increase in bulk there is, is more apparent than real. There is, however, still some puffy swelling, due to a thickened state of the synovial membrane. The limb strapped with the gum and mercurial plaster, and the side splints again applied. He left the hospital at his own desire.

In the month of January 1866, I was again requested to see him, as of late the knee had occasioned him considerable pain. The swelling anteriorly is rather less than when I had formerly seen him, but there is more emaciation of the leg and thigh, and posteriorly over the head of the tibia; and also above the head of the fibula there are two fluctuating swellings, which on pressure are painful.

Desired to foment the knee, and keep it at rest; while means of a dietetic and medicinal kind were employed with the view of improving his appetite, and strengthening his system.

22d January.—C. W. was admitted into Chalmers' Hospital, as the fluctuating swellings had gradually increased. The pain in the joint was now very severe, and as this was attended with loss of rest, complete absence of all appetite for food, a quickened pulse, night sweats, and general exhaustion, it was determined to excise the knee-joint. Meanwhile, the abscesses were opened, and about half an ounce of thick, adhesive, ropy pus escaped from each.

4th February.—The pain has not abated since the opening of the abscesses, the swelling of the entire joint has been reproduced, and a large fluctuating swelling exists above the patella beneath the extensors of the leg.

6*th.*—To-day, Dr Watson excised the knee-joint in the method he usually adopts, removing the patella, however, on account of the degree of ulceration of its articular surface which was present. In clearing the femur and tibia for the application of the saw, quantities of thick, almost cheesy, masses of purulent substance adhering to the surface of the synovial membrane were observed and wiped away with the sponge, and a large mass of gelatinous lymphy matter was removed from the upper part of the capsule of the joint. The usual gypsum apparatus with splint and suspension-rod was applied, and the patient removed to bed.

In the evening, the flap was laid down in its position, and secured there with sutures.

20*th.*—Ever since the operation he has suffered from more or less sickness and vomiting, everything he took for five days being ejected from the stomach. Along with this an almost constant hiccough tormented him, which prevented rest. Nothing afforded so much relief to these painful symptoms as a heavy weight (eight pounds) applied over the epigastrium, and morphia administered by subcutaneous injection, in pilules, or by the rectum as circumstances admitted. Ether, ice, chloroform, prussic acid, cannabis indica, musk, champagne, were all tried, as well as bismuth and grey powder, but none seemed to afford more than temporary relief. He is now reduced to an extreme state of emaciation from the inability to take food, which till to-day has continued, without anything but a very little essence of beef remaining on the stomach. He has become icteric within the last three days. A bedsore has formed over the sacrum in spite of every care and precaution. To have essence of beef and champagne or sparkling hock every hour.

3*d April.*—The jaundice rapidly subsided under the use of liq. bismuthi. He has commensurately gradually gained appetite and strength. The bedsores are now completely healed. The limb has progressed satisfactorily, the only trouble being occasioned by the tendency in matter to bag in the hollow left by the removal of the patella. To-day, to obviate this, a free opening was made through the skin, in this situation evacuating a quantity of pus.

6*th June.*—With the exception of the sinuses, which continue to discharge purulent matter, the whole original incision is healed. On laying open one of these apertures to examine into the cause of the delayed consolidation, Dr Watson felt in the head of the tibia a large portion of dead cancellated tissue. To effect the removal of this as speedily as possible, and to permit the osseous surfaces to come in contact with each other, Dr W. laid open the whole line of the incision, and forcibly bending the leg upon the thigh, disclosed the cut surface of the head of the tibia, with a portion of dead cancellated tissue about the size of a five-shilling piece situated in the middle. This was partially detached from the surrounding osseous tissue. Its removal was effected by taking away a slice of the tibia, which included its entire thickness, and left a healthy osseous surface beneath. Limb adjusted and apparatus applied as at first.

12th.—He has gone on well till to-day, when rigors, followed by febrile reaction, hiccough, and vomiting, set in.

15th.—This was attendant upon an attack of erysipelas which, seizing the limb, has spread upwards, and is now occupying the abdominal and thoracic regions on the same side. A blister applied over the region of the liver. Effervescing citrate of potash draughts with ten minims of tincture of the muriate of iron every two hours. To have sparkling hock with aerated lemonade.

20th. — The erysipelatous attack has subsided, leaving the limb, however, greatly swollen. A severe attack of diarrhœa has come on, which requires him to have a $\frac{1}{4}$ gr. of morphia every four hours, with ℈j. doses of bismuth to subdue the pain and irritation.

5th July.—Diarrhœa subdued. The appetite is returning in some degree. The swelling of the thigh, which was so great as to require the cutting up of the bandage, is rapidly subsiding. There is still, however, great tympanitic distention of the abdomen in front, and some degree of dropsical effusion in the lateral and posterior part of the cavity.

30th. — The diarrhœa again set in after a brief period of improvement which seemed to promise a rally, and after great abdominal uneasiness, which resisted everything but the employment of large opiates, he has gradually become more anæmic and exhausted, and died in the course of this evening. No sectio permitted.

Case XII.—J. G., æt. 35, admitted 8th October 1866. Patient states that about two years ago, when in India as a private in the 28th Regiment, he received a blow from a cricket ball upon the left knee, which laid him up in hospital for some time. About two months after the receipt of the injury he returned home with his regiment and proceeded with it to Aldershott. While there, he was attacked with pain and swelling and stiffness of the joint, which obliged him to go into hospital; there he continued till discharged from the service some months ago. During his residence in hospital there were two abscesses opened, one over the head of the fibula, the other along the line of the outer hamstring tendons. Since his discharge from the service, he has resided in Stirlingshire with his friends. The gradual deterioration of his general health has led him to come into the infirmary.

On admission, the knee is greatly swollen, the swelling being elastic in its characters, and engaging chiefly the subcrureal pouch of synovial membrane. There is considerable freedom of movement of the joint both passive and active; the patient cannot, however, employ the limb in progression, as bearing his weight even partially upon it occasions severe pain. There is a copious gluey discharge from both openings. A probe introduced through either enters the joint cavity, but comes in contact with no osseous texture. Hot opiate fomentations were applied over the joint, and the limb was laid upon a wire splint.

18*th October*,—Two fresh abscesses formed on the outer side, above and below the level of the patella. These were opened, water-dressing applied, and quinine and sulphuric acid ordered to check the copious night sweats.

22*d.*—Splint removed as the posture is uneasy; warm opiate fomentations continued.

31*st.*—Complains of great pain and starting of the limb at night. To have an opiate.

4*th November.*—Patient's health failing rapidly. He is now anxious that anything short of amputation should be had recourse to, which affords the slightest prospect of relief from the severe pain he constantly suffers in spite of large and repeated opiates.

6*th.*—To-day, Dr Watson excised the knee-joint in the usual manner. With the view of removing as little of the femur as possible, he sawed off the condyles on the level of the condyloid pit by a transverse section, and then removed the upper and posterior portion of the articulating surface by applying the saw at right angles to the original section, *i.e.*, parallel to the long axis of the bone. The amount of the femur and tibia removed amounted exactly to an inch and a quarter. A large abscess beneath the *quadriceps extensor cruris* was then opened, and the whole suppurating surfaces pencilled over with strong tincture of iodine. The patellar surface was perfectly sound. Acupressure was employed to arrest the hæmorrhage, which was unusually profuse, owing to the vascularity of the diseased structures. The limb was immediately put up in the ordinary method employed by Dr Watson, and on removing the patient to bed, the limb and apparatus were attached to the running pully of a Salter's swinging apparatus. There was considerable sickness and depression after the effect of the chloroform passed off, which was relieved by the administration of brandy and ice while he was surrounded by pans of hot water.

7*th.*—Patient had a good night. The bandage round the knee was clipped away, and the margins of the plaster bandage varnished with hot paraffin to prevent soakage of discharge.

8*th.*—Acupressure needles withdrawn. Bismuth and hydrocyanic acid ordered to check an inclination to vomit. To have essence of beef and milk with aerated lime-water for food with brandy. The limb is dressed with water-dressing, and an ice-bag is suspended over the site of operation. As matter seems to bag at one point beneath the flap, an incision was made to-day to evacuate it directly. The whole amount of the discharge in twenty-four hours does not exceed one ounce.

12*th.*—Patient still improving.

From this period until the 20th November he continued to progress favourably, and, although feeble, took nourishment readily. He, however, then began to manifest unmistakably the existence of pyæmia, from which he sank upon the 24th November. The only situations, however, in which there was even a suspicion of any metastatic deposits were the sheath of the *rectus abdominis*, and the temporo-maxillary articulations.

The sectio cadaveris revealed the existence of obsolete tubercle in the apices of the lungs, with œdema of their lower lobes. Old adhesions between the liver and diaphragm, enlargement of the spleen, pericardial effusion, and purulent collections in the right temporo-maxillary articulation and sheath of the rectus.

Remarks.—I have arranged these twelve cases in two sections, placing the successful results before the fatal cases, not that I would give them an undue value, but because I have less now to say about them, for they speak for themselves; while having more to say about the fatal cases, I have detailed them last, that the reader may the more readily follow my remarks.

With the exception of the one case of primary operation the others were all instances of long-standing disease. In four of these five cases the disease which destroyed the joint had undoubtedly originated in the synovial membrane, and the patients' condition gave evidence of an originally enfeebled constitution. In the first of the fatal cases, Case VII., there was hæmoptysis and physical symptoms of phthisis pulmonalis. In Case VIII., dysenteric diarrhœa and pulmonary symptoms with constant dyspepsia had existed for a long period. In Case IX., the lad was tall, overgrown, and emaciated from the very first. In Cases XI. and XII., the patients were anæmic, had suffered previously from hepatic affections of warm climates, and from the whole progress of their cases indicated the existence of some internal mischief. In only two of these cases, IX. and XI., was the operation of excision a matter of choice; in the others it was undertaken at the strongly expressed wish of the patient, that (whatever the result) they selected it in preference to amputation, to which they would not submit.

Furthermore, in Case IX., feeble constitutionally as the lad undoubtedly was, there was nothing decidedly to contra-indicate excision, and the fatal result is assuredly attributable to the accidental occurrence of hæmorrhage,—a complication which no prescience could have foreseen or prevented, any more than after the amputation, the recurrence of hæmorrhage from the separation of the periosteum of the femur, opening the nutrient artery of the bone, could have been anticipated. In Case XI., the violent attack of continued hiccough which immediately succeeded the operation threatened to prove fatal of itself, and had, in my opinion, a very decided influence in determining the necrosis of the cancellated tissue of the head of the tibia. To this last unfortunate complication of the operation must be referred the want of union of the osseous surfaces, and the spongy unhealthy condition of the surrounding soft parts. Had no necrosis followed the operation, had no hiccough disturbed the limb and exhausted the patient, I am convinced the extremity would to-day have been as sound and useful as that of any of the successful results.

In Case VIII. the dysenteric diarrhœa, due to tubercular disease of the intestines, may have been excited during the progress of the post operation period, but a careful inquiry into the antecedent health and history of this lady's case convinces me that it was an old and insidious mischief, long lingering about her, and now called into fresh vigour from the long track of bad health, the unavoidable confinement, and the difficulties of digestion which had all come in upon her during the last few months. Still, with all this, the progress of the knee up to within a week of her death was all that could be desired, and had the intercurrent illness not proved fatal, all seemed to promise a prosperous result.

In Case X. the fatal issue due to pyæmia is a result which may be said in no respect to have any connexion with the operation being one of excision. It is the most frequent cause of death in all surgical procedures, great and small. It has been known to prove as fatal after such trifling operations as tying hæmorrhoids or cutting a squint, as in the greater procedures of surgery.

In Case XI., as in the others, we may reasonably question if any other line of practice would have afforded a better issue. In none of these five fatal cases was the operation undertaken as an *opération de complaisance.* It was necessary that something should be done to relieve each of these patients from a condition of the textures which afforded no prospect of other relief than death if left to follow its own course; and the only question at the time was, whether excision or amputation should be undertaken in the circumstances with what we knew at that period of the patient's condition and antecedents? The answer in favour of excision seemed clear; but in looking back to what influenced me then, and carrying the mind forward with what I know now of the progress and issue of each of those cases, I do not think I could come to any other resolve now than that amputation—the progress of which in regard to these patients and their systems we cannot really know—would have proved in all human probability equally fatal, and possibly much more rapidly so.

Are there, however, any indications from the successful cases as compared with the unsuccessful ones, whereby we can in anticipation regard the prospects of a given case as better or worse in undertaking excision? I think there is. I am inclined, next to the employment of the operation in cases of accident or injury, to regard its success as probably greatest,—1. When the disease has commenced in disease of the hard textures of the joint, or at least when there is but little chronic change in the synovial membrane; 2. When there is no great burrowing of matter beneath the muscles or in the popliteal space; 3. When the acute stage of the disease is on the decline, or has declined, leaving the patient with no manifest evidence of original defect in constitutional vigour; 4. Where no bedsores already exist, in such a situation as to prevent a dorsal decubitus. But because my experience in my own cases, and in those of others, leads me to such a practical conclusion, I do not think myself justified in deciding that all outside this selected number must necessarily be given over to amputation. A like selection to that I have indicated no doubt would have afforded the most successful results when amputation alone was employed; yet the others, because less hopeful, were not permitted to perish simply because they were unfavourable for recovery. If the existing condition which urged the necessity for surgical interference were a fatal one, no man could hesitate to employ what held out never so slender a chance even of recovery because it was a slender one. Should we, therefore, at the present day, withhold the employment of excision in such unsatisfactory cases because they are for the most part unsatisfactory? I believe not, unless one of two things could be proved,—1. That amputation would have succeeded better; or 2. That these cases are so unsatisfactory as to prove uniformly fatal. The ordinary method of determining such questions by a reference to statistics seems to me fallacious in the extreme, for statistics in such circumstances place upon the same platform of dead figures living quantities which differ ofttimes *toto cœlo*. If an operator selects his cases, and refuses his operative aid in instances where a fatal issue is to be feared, he may produce to the wondering gaze of the profession results of the most astonishingly successful kind so far as numbers are concerned, but miserable in the extreme, I am inclined to think, if the greatest amount of relief to suffering humanity is to be accepted as the aim of all surgical interference.

There is only one way in which a surgeon, in regarding the propriety of any course of practice as estimated by the results of another's practice, can really arrive at any useful conclusion, and that is by placing himself mentally alongside of the practitioner who was in charge, and attempting, from the data afforded at a certain point, to decide whether at that time any other procedure was then more suitable, or if anything in the symptoms should have enabled him to prognosticate the unfavourable or favourable result.

No man would ever submit to amputation if he knew that by excision he would get rid of his disease, and at the same time preserve life and limb. No man, again, would ever for an instant submit to excision of the knee could it promise nothing more than certain death. But after these points have been determined, the risks in each case, not as gauged by numbers, but as of a determinate nature, and as specially likely in a certain given condition of constitution and external circumstances to influence unfavourably the one operation or the other, should alone be permitted to weigh in the balance and turn the scale one way or another. A vital element sometimes, however, must come into our calculation in deciding upon our operative procedure, and that is the wish of the patient himself. Holding such views as to the principles which should alone influence a surgeon in his choice of an operation, where a choice does exist and operative interference is unavaidable, I do not think, had I again in those twelve cases to decide upon the line of action to be taken, that I should pursue a different course from that which I took. Nor do I see any reason to think that, had I resorted to amputation instead of excision, the results would have been less fatal than they proved under the treatment adopted.

Before concluding this paper, I have a word to say in regard to several moot points connected with this operation. First, then, with reference to the allegation that incisions freely laying open the whole extent of a suppurating joint, combined with complete repose, effected by splints, gypsum apparatus, starch bandages, or otherwise, may with perfect propriety be substituted in most cases where excision of the articulation could be satisfactorily substituted for amputation. Such a statement can only hold good where the lower extremity is concerned, and where the limb is nearly in the straight position, or where with tenotomy and forcible extension under chloroform a like position could be obtained. I apprehend, in all such cases, there is a very great difference in the after progress and result to be observed from the conditions obtained by excision. Where the bones undergoing more or less diseased action are kept in steady contact, no excision being practised, the tension and counter-pressure of the osseous surfaces thus maintained must tend, in many cases, to determine farther suppurative progress, farther ulceration, it may be, in some cases, actual necrosis of considerable fragments of the cancellated tissue of the opposed bony surfaces. When, on the other hand, excision is practised, complete relief is at once afforded to all the tension, and ill results due to further destruction within the limits of the field of operation rarely occur.

2. It may, perhaps, be asked, when such admirable results are obtained from excision of the elbow-joint, in the shape of a movable articulation, scarcely less useful or seemly than the natural joint, and infinitely more serviceable than an anchylosed one, why should we rest content in excision of the knee with a stiff limb? There are both theoretical and practical objections to this suggestion. The theoretical or analogical reasons are:—1. That the elbow-joint is useful in virtue of its freedom of movement in every direction, so long as the approximated osseous surfaces do not actually pass each other, while in the knee any movement which interferes with the complete rigidity of the limb as a column of

support, capable of sustaining the weight of the whole trunk, renders the limb almost unserviceable in progression. When, for example, the muscles fail to lock the knee-joint in complete extension, walking is almost an impossibility. Again, while an ununited fracture of the arm is more an annoyance than a cause of incapacity in the affected arm, an ununited fracture of the thigh disables the patient till the limb is in some way rendered rigid. It is further easily demonstrated in any case of excision of the elbow-joint where the arm is in every respect most perfectly useful as a prehensive organ, that the patient can rest no weight upon it. I have seen a patient whose elbow-joint has been excised scrubbing a floor most energetically with the affected arm, and resting her weight upon the sound limb; on asking her to transfer the scrubbing-brush to the hand of the sound limb and support herself upon the arm operated upon, in making the attempt she at once fell over upon that side, the limb doubling beneath her weight. There is this manifest difficulty in attempting, in excision of the knee-joint, to obtain a movable articulation which shall be of any use to the patient, that in making the union flexible in one direction it must be flexible in all directions equally. Thus the tendency to bend outwards or inwards would be as great as the extent of the motions of flexion extension, and as we have seen that in the after treatment there is a tendency to bow outwards, this would, in all probability, be the direction in which the flexure of the limb would occur when the weight of the body was borne upon that extremity. 2. The practical difficulties which have occurred in attempting the formation of a useful limb with only a fibrous union at the site of the excision are, I think, sufficient, so far as our knowledge at present goes, to forbid its repetition. It is no new proposal this of a movable joint. Mr Syme, so long ago as 1831, seems, in his early essays, to have been in favour of such an attempt. He says:—"During the cure, it does not seem proper to insure absolute rest in order to obtain a true anchylosis or osseous union, since the very long bone that would thus be formed, besides being extremely inconvenient to the patient, by rendering the limb perfectly rigid, could not fail to expose it to a great risk of fracture, by affording long levers to forces acting at the extremities. A great degree of flexibility, on the other hand, would unfit the limb for support and progressive motion, so that while perfect immobility and free motion ought to be avoided, a slight degree of flexibility ought to be promoted. The chief difficulty of the cure consists in preventing the tendency to bend outwards, which is always strong, and if not counteracted, most injurious to the appearance and usefulness of the limb."[1]

I am also informed that in those cases in which Langenbeck has performed excision of the knee, he has adopted measures calculated to obtain a movable articulation, but that the result has been the necessity of resorting to amputation in every case. Such are the reasons, theoretical and practical, which have prevented me from making any attempt in this direction, and which meanwhile appear to me likely to influence practical surgeons against the adoption of a method of after treatment calculated to obtain such unsatisfactory results.

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[1] Syme, Treatise on the Excision of Diseased Joints, p. 134–5.

3. The method by which I have practised the excision has, in every instance except one, been by means of the semilunar incision raising a long flap from the anterior surface of the articulation. The one exceptional case in which I employed an elliptical incision, crossing the front of the joint, and including the patella within its limits, was one in which the operation was practised on account of compound comminuted fracture of the patella. Here the lacerated condition of the integuments, together with the necessity to effect the removal of the comminuted bone, made this method preferable to any other. In employing the long anterior flap as a means of gaining access to the articulation, I have preferred it for four reasons, chiefly:—1. That while it opens up the parts to be dealt with to both sight and touch more thoroughly than any other, it is easily and rapidly mapped out by a single curvilinear sweep of the knife. 2. That when its extremities are carried behind the line to the condyles they form a complete and efficient drain for the whole extent of the cavity formed by the excision. 3. That its margin corresponds in a less degree than any other to the line of section of the osseous surfaces. 4. That the flap so raised will serve for amputation of the thigh through the condyles, should the state of the joint render that advisable after excision has been completed. The H incision, the simple transverse incision, the lateral incisions, both straight, or one straight and the other curvilinear, I have employed on several occasions in operating upon the dead body, but none of them have seemed to me to possess any such superiority over the long flap as to lead me to employ it in practice. They have certain advantages, it is true, which I readily grant, such as admitting in the case of the H, the transverse, or the elliptical incision, a more easy removal of the patella, if that is thought essential or desirable in any given case; but as I have always attempted to retain the patella where this is possible, incisions specially suited to favour its easy removal have not seemed to me to possess any advantage in this operation. If the articular surface of the patella is affected by disease, the gouge will usually enable its diseased tissue to be easily removed to the desired limits as regards both superficial extent and depth. Besides, it is well to bear in mind the practical fact that it is rare to find any such extent of disease in the patella as to determine the necessity for its removal,—an observation in morbid anatomy which its position as a sesamoid bone might almost lead one *à priori* to anticipate. The lateral forms of incision, straight on each side, or straight on the inside of the knee and curvilinear upon the outside, were devised for the purpose of leaving, not only the patella intact, but the ligamentum

patellæ as well. This may seem at first sight an advantage, and were a useful movable articulation attainable as a result, it would be so without doubt; but as such has not hitherto been known to follow upon this method of operating, the difficulties which it creates in the manual procedure should be sufficient to condemn it.

Perhaps it may be asked, why I have retained the patella? My reply is, that (1) its removal is unnecessary in most cases; (2) its presence in the flap bears up the soft parts from the line of incision, and, without preventing consolidation, helps to keep them away from the cut margin of either osseous surface; (3) that its removal occasions more bleeding; and (4) that the hollow left after its removal from the centre of the long flap leaves a hollow cavity in which matter bags, and requires a separate incision to drain it efficiently.

4. The patients whose cases I have narrated are all adults. I have not performed the operation in any child or adolescent. I have been deterred from doing so by the small prospect which the removal of the epiphysis of the femur and tibia must afford of the after growth of the limb proving commensurate to that of the rest of the body. In children I have hitherto regarded amputation as a preferable operative procedure. I believe I shall continue to do so until I have had satisfactory evidence that the limb grows, or that the short and shrunken limb is capable, with the aid of a wooden pin, to support the superincumbent weight of an adult body more satisfactorily than a thigh stump and a wooden leg.

5. In regard to instruments requisite for the performance of the operation, I need say nothing further than that I have required nothing more than a large bistoury in a firm handle, and a common amputation-saw with a back which yields as the blade cuts its way through the condyles of the femur or broad head of the tibia. Various ingenious devices have, however, been recommended and employed by others in this extremely simple operation for dividing the osseous tissues. Among these, Mr Butcher's saw, Symonovsky's saw modified by Lüer, the old double-toothed saw of Moreau, and the chain saw recommended by the late Dr Jaffray of Glasgow, in his edition of Park and Moreau's works on excision of the joints, specially deserve mention. There can certainly be no objection taken to their employment if the section of the bone were more easily or nicely effected by means of them than by the common amputation-saw. But as one and all of them are less easily manipulated, and there is no evidence in favour of their effecting this work more smoothly, I cannot conceive why instruments, certainly well-suited for the key-hole work or the pattern sawing of the cabinetmaker, should be imported into the armamentarium of surgery, or made of such great moment in the performance of an operation which requires neither sections in curves, in zigzags, nor in dovetail patterns.

I once heard a surgeon desiderating some instrument by which two smooth sections of the tibia and femur might be easily effected in excision of the knee, as he had seen difficulties occur from a want of capacity in the operator to cut two flat surfaces, which, when laid *in situ*, produced a straight limb. It may have been a legitimate wish upon the part of so clumsy an operator, and were the saws we have named capable of affording such facilities, we should strongly recommend them to his attention; but, as they are certainly not fitted to afford such assistance, a simpler expedient would perhaps be more easy of adoption, viz., that until a man acquires the very moderate degree of command over his hands and eyes necessary for the performance of so simple an operation, he had much better totally abstain from engaging in the operative department of his profession.

Part Second.

REVIEWS.

I. *Acupressure, an excellent Method of Arresting Surgical Hæmorrhage, and of Accelerating the Healing of Wounds.* By WILLIAM PIRRIE, C.M., M.D., A.M., F.R.S.E.; and WILLIAM KEITH, M.R.C.S.E., M.D.

II. *Notes on the Progress of Acupressure.* By Sir J. Y. SIMPSON, Bart., M.D., D.C.L., Professor of Medicine and Midwifery in the University of Edinburgh.

SIX years have only elapsed since Sir J. Y. Simpson first suggested to the medical profession a new method of arresting surgical hæmorrhage under the title of Acupressure. In these six years this suggestion has become an accomplished fact in the experience of many surgeons at home and abroad; and as year by year new converts have been made to the expediency of giving a fair trial to this means of permanently arresting arterial bleeding, so year by year has this method gained adherents, and the procedure been removed from the sphere of theory and experiment to that of practice and experience. In extending thus its basis, it has not been its lot to lose its old adherents as it has gained new; the old adherents have become only more satisfied, the new more enthusiastic and confident. If the earlier converts are less demonstrative in their statements, it is probably because matured convictions deal in deeds rather than words,—in practical employment more than written speculation,—it certainly is not that they have forsaken this hæmostatic means as untrustworthy, but that, having proved it, and found it to bear out all that they expected, they have silently and contentedly pursued their course, strong in the strength which theory sustained by the accumulating weight of personal experience is felt to afford.

Every new invention has to force its way to acceptance; to every step of progress hostile criticism is sure to raise determined obstruction. Had acupressure emanated from any source less honoured, less famous, or less powerful than the fertile brain of the successful discoverer of chloroform, no doubt the detraction and opposition with which this invention has been assailed might well have sunk it out of sight, for as long a period as the ligature of Paré languished under the aspersions and practical neglect of the surgeons of Europe, who delighted to rule the surgical progress of that day with the iron sceptre hot from the blazing brazier of their traditionary surgery, derived from the Arabian period of the healing art. The opposition has, indeed, been most formidable; but it nevertheless appears that, in spite of all obstacles, acupressure still exists as a great accomplished fact; it has acquired a secure position in the experience of numerous practical surgeons, and the testimony in its favour is accumulating from many eminent authorities, whose impartiality, as well as their opportunities of forming a judgment based on extensive experience, are undoubted, and demand the fair consideration of all unprejudiced observers.

It might have been a fondness for novelty—a juvenile desire to exceed the boundary-line of well-beaten practice—which led some of our young and enthusiastic friends to slip the knot of the ligature and pin their faith to the skirts of the academic Lucina, who already, "victo dolore," had so charmed the old snake of the Æsculapian rod into deepest slumber that its deadly fang need cause "nor fear nor dread." But no such youthful enthusiasm could ever be alleged in explanation of the approval and daily employment in practice of this "Excellent Method of Arresting Surgical Hæmorrhage" against such leaders in the surgical camp as the renowned and experienced authors of this work upon acupressure, which has just emanated from the Aberdeen Medical School.

Thanks to them for such a work; there is value in the volume of independent practical experience they have just published,—because they write afar from the actual field of strife, between leaders in our own camp,—because they write and speak from the foremost position of teachers in a large public hospital, where their words and deeds are alike open to the scrutiny of the pupils of their school,—because by the profession they are regarded as speaking as masters in their art, and as giving a verdict completely emancipated from the influence of those whose age and power might be supposed to check or encourage the opinions they espoused upon the case in question.

This work consists essentially of two parts: the first and larger is from the pen of Professor Pirrie; the second from that of Dr Keith; while, in a short statement, the cases in which Dr Fiddes has employed acupressure are tabulated by Professor Pirrie. Professor Pirrie gives us, in his first chapter, a brief sketch of the origin and progress of acupressure. In his second, he describes the instruments required for acupressure. In his third, he enumerates the various modes of acupressure. He then describes the methods of healing open incised wounds, that "there may be no misunderstanding as to the nomenclature employed to describe the methods by which healing was believed to be effected in the cases narrated," in his next chapter, which constitutes the great bulk of the work. We may state briefly that the terms employed are those adopted by Mr Paget, and generally recognised in modern works on surgery. The final chapter is an appreciation of the experience of acupressure as a means of checking hæmorrhage, and of securing speedy union in cases of recent wounds. Dr Keith's part of this elegant volume is entirely devoted to a clinical record with remarks embracing his experience of acupressure. To facilitate reference, it seems desirable that we should briefly enumerate the seven different methods of applying acupressure detailed in the text.

The First Method consists in introducing a long needle or glass-headed pin, with bayonet point, through the flap from its cutaneous surface close to the opening of the bleeding vessel; carrying its point across the track of the artery, and then pushing its point into the tissues of the flap, it is made again to emerge upon the cutaneous aspect of the flap.

The Second Method. Here a short needle, threaded with iron wire, to admit of its being withdrawn, is introduced upon the cut surface of the flap. First taking up the tissues on one side of its course, it is made to emerge close to the channel of the bleeding vessel, and bridging over its track, is pushed onwards more or less deeply among the tissues beyond, so as to compress its tube and stanch the bleeding by the strain of the tissues among which either extremity of the needle has been buried.

The Third Method is effected by transfixing the tissues lying around an arterial tube, by means of a needle introduced from the cut surface. A loop of wire is now passed over the exposed point of the needle, and the two ends of the wire together tightened round its shank, till the tissues containing the vessel, and now included between the needle and the wire, are sufficiently compressed to check all flow of blood from the open mouth.

The Fourth Method is precisely the same as the third, except that a steel pin with a glass head is substituted for the needle threaded with twisted wire.

The Fifth Method was originally employed by Sir J. Y. Simpson in a case operated on by Dr P. D. Handyside, but independently introduced by Dr Knowles, a former house-surgeon in the Aberdeen Hospital (and hence called in Aberdeen the Aberdeen method). This is effected by introducing a needle or pin through a modicum of tissues on one side of the artery to be compressed, and then having rotated its shank or head a quarter or half circle horizontal with the surface of the wound, its point is pushed onwards among the tissues, corresponding to the new axis of the needle, so as to maintain the occlusion of the artery by the degree of twisting of texture thus produced.

The Sixth Method, originated by Dr Keith, consists in passing a pin through the tissues beside the open mouth of an artery; the duplicature of iron wire is then looped over the needle-point; the ends of the wire, instead of being bent together round the shank, are separated from each other and made to include the bleeding vessel and the textures in which it lies, so that when the ends of the wire are crossed behind the needle-shank and tightened, the included parts are compressed on both sides of the needle.

The Seventh Method consists in employing a pin to compress the bleeding artery against an osseous surface in contact or in close proximity to which it lies.

But the methods of employing acupressure apparently multiply as experience advances in the use of this hæmostatic agency; for Sir J. Y. Simpson, in a note to his recently-published brochure upon the Progress of Acupressure, which, along with the work of Drs Pirrie and Keith, we have placed at the head of this article, speaks as follows:—

> "In their practice they do not notice one method which I have often employed to close minor vessels, and which I have seen my friend Dr Heron Watson, Surgeon to the Edinburgh Royal Infirmary and to Chalmers' Hospital, use. It consists in transfixing with a pin or needle one or two lines of the soft tissue by the side of the bleeding artery, the pin being laid almost flat in doing so, and with its head towards the bottom of the wound. The head of the pin is then raised up till it is at right angles to the surface of the wound, and then further turned over till the head comes to the outer edge of the wound, and its point is turned to the bottom of the wound. It is easily fixed in this new position by being thrust forward a few lines into the tissues beyond. In its movement the head of the pin is thus turned round half a circle, and in doing so closes the bleeding orifice and overlaps it with a small quantity of neighbouring tissue. A quarter of a circle turn is sometimes quite sufficient. This kind of 'over-twist' is one of the simplest and most expeditious modes of acupressure for smaller vessels. But further practice may point out many other modes of simplifying the application of the needle." (Pp. 6 and 7.)

It would be impossible to transfer to our pages (unless, indeed, we were to reprint them in full) a just idea of the history and progress of the cases narrated by Drs Pirrie and Keith in their treatise upon this important and novel subject. To the work itself, therefore, we must refer our readers for fuller information and all necessary details. The impression produced on our own minds by reading their remarkable statements has been one strongly favourable to the employment of acupressure, and we therefore think it only fair to these distinguished surgeons that we should summarize their results in a form which admits of ready reference and comparison by those who may not have ready access to the work itself.

No.	Age. Yrs.	Nature of Case.	Method of Acupressure	Period at wh. needles removed, in hours.	Result.	Operator.
1.	6	Amputation of thigh for disease.	3d.	48	"Few drops of pus," otherwise primary adhesion.	Prof. Pirrie.
2.	13	Amp. thigh, disease.	5th.	48	"A little pus," otherwise primary union.	Do.
3.	51	Amp. thigh, disease.	5th.	72	Death, bronchitis, 6 weeks after operation.	Do.
4.	66	Amp. thigh, epithelial cancer.	4th.	72	Considerable suppuration.	Do.
5.	14	Amp. thigh, disease.	3d.		Died, 36 hours, shock.	Do.
6.	15	Amp. thigh, medullary tumour.	4th.	72	Not more than a tea-spoonful of pus.	Do.
7.	42	Extirp. of mamma.	1st and 3d.	48	No supp., except where one ligature for after-bleeding.	Do.
8.	35	Extirp. of mamma.	3d.	48	Entirely healed by immediate union.	Do.
9.	22	Excision of elbow.	3d.	48	Healed in 3 weeks.	Do.
10.	young man.	Excision of testis.	1st.	24	Primary union.	Do.
11.	18	Amp. leg, disease.	5th.	48	Sloughing, erysipelas, small-pox, recovery.	Do.
12.	22	Wound of fore-arm.	7th.	24	Immediate union.	Do.
13.	47	Excision of mamma.	1st and 3d.	43	Primary adhesion.	Do.
14.	7	Primary amp. of arm.	3d.	15	Perfect immediate union.	Do.
15.	28	Excision of mamma.	1st and 3d.	24	Immediate union.	Do.
16.	7	Excis. of erectile tumour.	1st.		Immediate union.	Do.
17.	20	Excision of elbow.	4th and 5th.	24	Suppuration, healed in 2 months.	Do.
18.	15	Excision of elbow.	4th.	24	Suppur., 22 days, healed.	Do.
19.	26	Wound of fore-arm.	1st.	15	Immediate union.	Do.
20.	50	Secondary hæmorrhage from thumb.	1st.	16	Granulation.	Do.
21.	37	Wound of hand.	1st.	24	Primary union.	Do.
22.	17	Excision of upper part of fibula.	3d.	24	Granulation.	Do.
23.	44	Excision of testis.	1st and 4th.	2½	Primary adhesion.	Do.
24.	38	Amp. leg, cancer.	4th and 5th.	24; bleeding and needle introduced for 24 more	Died, exhausted, diarrhœa.	Do.
25.	32	Excision of mamma.	1st, 4th, 5th.	3	Primary union, small-pox, a few drops of pus during eruption.	Do.
26.	35	Excision of mamma.	1st, 4th, 5th.	48	Immediate union.	Do.
27.	46	Excision of mamma.	4th and 5th.	22	Primary adhesion.	Do.
28.	45	Excision of mamma.	1st and 4th.	18	Primary union, except at one spot.	Do.
29.	10	Amp. leg, disease.	4th.	48	Suppuration, 4 weeks.	Do.
30.	15	Amp. leg, disease.	4th and 5th.	72	Erysipelas, supposed granulation,	Do.
31.	62	Epithelioma of lip.	1st.	48	Immediate union.	Do.
32.	6	Amp. thigh, disease.	4th and 5th.	44	Immediate union.	Do.
33.	45	Primary amp. of arm.	1st, 3d, 5th.	96	Suppuration.	Dr Keith.
34.	54	Amp. fore-arm, tumour.	1st and 3d.	52	Erysipelas, suppuration.	Do.
35.	14	Amp. leg, disease.	3d.	48 and 72	Union, first intention.	Do.
36	29	Amp. thigh.	3d and 4th.	72 and 96	Healed in 18 days.	Do.
37.	18	Primary amp. of leg.	4th.	48	Died, 7 days.	Do.
38.	12	Amp. thigh, disease.	4th.	Boy withdrew needle from femoral in 4 h. 96 sec.	Slight suppuration, healed in 21 days.	Do.
39.	29	Amp. thigh, disease.	3d and 4th.	96, 120, 144	Soundly healed, 19 days.	Do.
40.	29	Primary amp. of arm.	4th and 5th.	72, 96, 144	Trifling suppuration, soundly healed, 27 days.	Do.
41.	47	Fatty tum., 2 lbs. 10½ oz.	5th.	72	Soundly healed, 19 days.	Do.
42.	15	Amp. shoulder, medullary disease.	4th and 5th.	48 and 96	First intention.	Do.
43.	63	Amp. leg, disease.	5th.	48	Recovery	Do.
44.	45	Hæmorr. from perinæum.	6th.	52	Recovery.	Do.
45.	12	Amp. thigh.	5th.	48	No report.	Dr Fiddes.
46.	50	Amp. of fore-arm.	5th.	72	Do.	Do.
47.	40	Excision of mamma.	3d.	72	Do.	Do.
48.	6	Amp. of leg.	4th and 5th.	72	Do.	Do.
49.	19	Amp. of arm.	5th.	48	Do.	Do.
50.	52	Amp. of leg.	4th and 5th.	48	Do.	Do.
51.		Wound of radial.	4th.	48	Do.	Do.
52.	46	Excision of mamma.	4th, 5th, 6th.	48	Do.	Do.
53.	10	Amp. of fore-arm.	4th and 5th.	72	Do.	Do.
54.	10	Amp. of leg.	4th and 5th	72	Do.	Do.

These results, glanced over without remark, are assuring enough to encourage any hesitating surgeon fearlessly to adopt acupressure as something more than an experiment upon his part. They must convince him that the procedure, rightly employed, is a safe one; and should it meet in his hands with even a much less mead of success than that which is displayed in the faithful records of these pages, he may well rejoice in the possession of a means of arresting hæmorrhage, which our authors, we think, too modestly describe as "an excellent method." It is a safe measure to avoid strong expressions, and this cautious employment of language, which under rather than over estimates their good opinion, will not make their verdict the less confided in by strangers alike to themselves and the surgical procedure they so strongly advocate.

Sir J. Y. Simpson in his brochure transmutes the silent weight of their testimony into an active quantity in his summation of the results attained by the surgeons of the Aberdeen School:—

> "Out of these thirty-two major operations, which Dr Pirrie has given in detail,—including seven amputations of the thigh,—thirteen cases at least healed entirely by the first intention, or *more than one in three*. Out of his first eight cases of acupressure, one only of the wounds was cured entirely by immediate union, and without a drop of pus. Out, however, of his last twenty-four cases, fourteen of the wounds were healed by the first intention, and without a drop of pus, or *more than one in two;* showing, as was to be naturally expected, that he became more successful in his results as he became more extensively and intimately acquainted with the practice of acupressure. To state these important facts in another form—
>
> "Among his first 8 cases 1 completely united—or 1 in 8.
> " Among his last 24 cases 14 completely united—above 1 in 2.
>
> "He had eight cases—among these thirty-two—of excision of the diseased mamma. In five of these eight cases the resulting mammary wounds closed at once, and without a single drop of pus.
>
> "Has any surgeon, living or dead, ever, in using delegation, met with such marvellous success as Dr Pirrie has thus attained under the use of acupressure?
>
> "Could all the hospitals of Great Britain, or of Europe, or of the world, if grouped and collected together, furnish, within the same period of three years, as great a number of cases of large operative wounds, where the ligature was employed, healed entirely by the first intention, and without the appearance of 'a single drop of pus,' as the one little hospital of Aberdeen?"

This method of arresting hæmorrhage is no mere question of how to do it; it reaches far beyond, and to every right-thinking man opens up a vista of moral considerations which must more and more engage the attention of surgeons in the daily practice of their healing art. Chloroform and ether — the induction of anæsthesia—struck the key-note of which the echoes come sounding in upon us from every quarter, calling on us not to halt in our course of doing good, but resolutely to face the inquiry, striving to draw aid from every quarter, that as we may now inflict wounds without pain, so we shall cure them without unnecessary pain, and without subjecting the sufferers to unnecessary risk. Surgery must not always be cruel and inexorable,—the *cito* of the old writers must never be permitted to blot out of sight the now more than ever needful consideration of how it shall be both *tuto* and, still further, even *jucunde* to the patient submitted to our art. Surely it were something to be desired that in all our amputations what Dr Pirrie describes should be sought for, if not actually attained. A few days after amputating the thigh "I entered the ward with the surgical pupils of the hospital,—he was whistling most beautifully the 'Braes o' Mar,' and amusing himself by passing a little model carriage over an inclined plane he had constructed in his bed. This is one of the many instances we have lately seen in the Aberdeen Hospital of a capital operation and its after treatment having been made *delightful* to the patients as well as to the surgeon."

It is something passing strange in the history of an amputation to find a patient, four hours after the removal of the thigh, amusing himself with the pin-heads by which the femoral and other vessels were secured, and removing, without the loss of one drop of blood, three of these pins, one of which compressed the femoral trunk. Yet this was safely effected by a little lad of twelve, in the experience of Dr Keith.

It need excite no wonder in such circumstances that, writing in February 1867, Dr Keith says "Acupressure here is *triumphant*—nothing else being dreamt of—and I may say in daily use."

In the face of such facts as those with which this volume abounds, it is with reason that Dr Keith asks, "Can anything but prejudice oppose a trial, when *such* present and prospective advantages are proved to accrue."

Well may Sir James Simpson congratulate not only himself, but suffering humanity, upon the wide-spread interest already excited in this hæmostatic, as a means second, indeed, to his great anæsthetic discovery, but commensurate to the relief of suffering, and the preservation of life in circumstances where a few short years ago the accepted dogma had declared that perfection was attained, and that change could only be in the retrograde direction.

Grateful must be the sense of inestimable benefits conferred upon humanity, even to him whose praises are in every mouth, and whose Sovereign has delighted to crown with honour; and grateful to us, his fellow-townsmen and fellow-workers in the like calling, as rejoicing in his well-merited and hardly-earned honours, is the publication of this volume emanating from the Aberdeen School, because it confirms by practice what had formerly been inculcated chiefly in theory, and because it establishes upon a basis that cannot be gainsayed,—acupressure as a sound surgical procedure in all time to come.

Transactions of the Obstetrical Society of London. Vol. VIII., 1867.

Catalogue and Report of Obstetrical and other Instruments Exhibited at the Conversazione of the Obstetrical Society of London, etc., 1867.

THE Obstetrical Society has, under the presidency of Dr Barnes, shown praiseworthy zeal and diligence in 1866. It has issued from the press the two valuable volumes whose titles are above given.

The volume of transactions is larger than usual, a condition for which we find no good reason. The contents do not demand it. Indeed, we are of opinion that considerable restriction should be imposed by the society on the admission of papers into its transactions. Every society has a deal of matters of passing or of trivial interest laid before it, which get abundant notice in the pages of the weekly journals. We do not say that there is any trash in this or in the other volumes of the society's transactions, but we do say that much of what is given might have been omitted with advantage. Valuable observations and valuable reflections or reasonings should form the volume. Among these we cannot include the "remarks" of members on papers read, in other words, the discussion; nor do we include even all the papers remarked upon, such as those relating to intrauterine instruments, clitoridectomy, the placental circulation, and phthisis pulmonalis. But the volume has substantial and, we believe, perennial value, for it contains valuable cases of uterine laceration recorded by Dr Radford, interesting observations and reflections on the births of monsters by Dr Playfair, an excellent contribution (though the author and printer have been both very careless) to our knowledge of puerperal eclampsia, from the pen of Braxton Hicks, whose name always indicates an article with observation and thought in it; and, lastly, a valuable statement, by Baker Brown, of the use and value of the actual cautery in ovariotomy.

The volume on instruments is scarcely a subject for criticism. It deserves all praise. It is of great value as a historical document, and we cordially thank Dr Meadows for it. Farther we do not go, except to say that discoveries must be encouraged, ingenuity fostered. On the other hand, we remind our readers that the best practitioners use the fewest instruments; that a collection of all sorts of instruments is, in an important sense, a collection of pestilential rubbish; that, in all departments of medicine, no delusion is more rife than the importance attached to ingenious, novel, and extraordinary treatment, whether by medicine or instruments; that he who knows least of disease is also he who is quickest in recommending medicines and instruments. Any one of our greater men could, if he were so misguided, discover, as it is called, new remedies and new instruments by the dozen. The Obstetrical Society aims at higher objects. It seeks to increase and diffuse obstetrical knowledge. W. Hunter, a prince of obstetrical science and practice in these countries, discarded instruments of all kinds, almost entirely; and not only instruments, but also medicines; and the obstetrical historian records this as one of his chief claims to the gratitude of posterity.

Lectures on Clinical Medicine. By Professor TROUSSEAU. Translated and edited, with Notes and Appendices, by P. VICTOR BAZIRE, M.D. Lond. and Paris. Part II. London: Robert Hardwicke: 1867.

IN noticing the second part of this translation of Trousseau's Clinical Lectures, which has just been published, we have to repeat the favourable opinion we expressed on the appearance of Part First. While the meaning of the original is carefully preserved, the English rendering is sufficiently free to afford pleasant reading, and contains as little admixture of the French idiom as is possible, without altering M. Trousseau's characteristic turn of thought and expression. The present, like the previous part, is devoted to nervous diseases, and is scarcely, if at all, inferior in interest to its predecessor. The subject of progressive muscular atrophy, which is first discussed, is still sufficiently new to excite special interest; and this lecture is followed by a valuable appendix, in which the editor carries the information down to the most recent date. Further on, we find the lecture on cerebral or brain fever, more commonly known as acute hydrocephalus. This was a peculiarly favourite subject with Professor Trousseau, as all who have had the good fortune to listen to his oral lectures at the Hôpital des Enfans Malades must recollect. Exception may be taken to some of his views of the pathology of the affection, but his peculiar power in the graphic delineation of symptoms is perhaps nowhere seen to greater advantage than in this lecture. Under the term chorea, which is treated of in Lectures XIV. and XV., M. Trousseau comprehends not only the gesticulatory form, usually called St Vitus' dance since Sydenham so misnamed it, but also the leaping and spinning-top movements, writer's cramp, and even senile tremors, and paralysis agitans. This extension of the name chorea must be attended to, otherwise it may mislead those who have been accustomed to use the term in a more limited sense. M. Trousseau's observations on these interesting affections are full of instruction, and his chapter on paralysis agitans is not only excellent in itself, but remarkable as recording his valuable experience on a subject which French writers have in general entirely overlooked. Although generally acquainted with English medical writers, to an extent not common with his countrymen, M. Trousseau does not appear to have seen Parkinson's Essay, although he alludes to it, and in consequence his opinion as to the relation of what he calls chorea festinans and paralysis agitans is less clearly and less correctly expressed than might have been expected. The titles of the remaining articles on facial paralysis, infantile convulsions, puerperal eclampsia, tetanism, and neuralgia, sufficiently indicate their tenor and their value.

It is unnecessary that we should repeat our recommendation of Trousseau's clinical treatise, either as embodying the mature results of the author's long experience, or as presenting a lucid exposition of some of the newer investigations of disease by French physicians. It is a work, undoubtedly, which should be in the hands of all practitioners and students. We must, however, for this very reason, urge upon the translator the necessity for a speedier issue of the different parts composing the work. No doubt, each part, or indeed nearly every lecture, is complete in itself; but it is very desirable that the continuity of the entire treatise should be preserved. The translator does himself injustice by delay. We observe that the contents of Part III. are already advertised, and we hope, therefore, soon to have the pleasure of announcing its publication.

Part Third.

MEETINGS OF SOCIETIES.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XLVI.—MEETING III.

19*th December* 1866.—Dr Moir, *President*, in the Chair.

I. WRITING-MACHINE FOR THE BLIND.

Dr George W. Balfour exhibited a writing-machine for the blind or those of weak sight, called the "Albert Help," from the name of its inventor. It consists essentially of two parts, which, for convenience of carriage, may be unhooked and separated from each other. The one part consists of a frame of wood, one inch and a-half broad by ten inches and a-half long, and about half an inch thick; in it is sunk a brass roller, upon which another roller, covered by India-rubber, is moved noiselessly by a square nut, the two rollers being retained in juxtaposition by a spring at each end. The second part hooks on in front of this first portion, and consists of a brass plate with an elevated ridge or paper guide at the right hand, and a piece of catgut stretched across it as a pencil guide. The manner of using this writing-machine is as follows:—The "Help" must be placed in front of the writer, the square nut at the left hand, and the plate with the guides hooked on in front of the rollers. The paper to be written upon must be passed under the pencil guide and against the ridge or paper guide so as to be square with it and with the rollers, which will make the paper go through easily. The rollers are then to be turned inwards by means of the square nut till the paper comes through to the back, so that the writer may ascertain by feeling whether it is square or not. While writing, the pencil guide must be placed between the middle and forefinger of the left hand, so that the writer may feel that the pencil is working against its guide; at the end of each word, three fingers must be moved up to where the pencil is before it is lifted to begin the next word; this prevents the writer losing his place, and insures a sufficient space between each word. When a tailed letter, such as a g or a p, is made, the forefinger of the left hand must yield a little so as to permit the pencil guide to descend. When the right hand comes in contact with the paper guide or ridge the line is finished, and the left hand must then give the square nut one quarter turn inwards, so that what was the top of the nut now faces the writer; this process moves the paper written upon through the space required before the next line can be commenced, and the process may then be continued. The pencil recommended is Perry's patent blue pencil. This simple and efficient writing-machine for the blind is sold at 10s. 6d., and can be obtained from its inventor, Mr Albert Cay, 7 Blackford Road, Edinburgh.

II. EXCISION OF THE KNEE-JOINT.

Dr Watson read the second part of his paper on excision of the knee-joint. (See p. 909 of the present number.)

Dr Gillespie had performed the operation of excision of the knee-joint six times, and in his last three cases had used Dr Watson's apparatus. By means of it complete rest of the limb and ease to the patient had been procured, in consequence of the diffusion of the weight of the limb over its whole length. The facility for dressing which Dr Watson's apparatus allowed was also a great advantage. Dr Gillespie had been much pleased with the results obtained. Dr Gillespie agreed with Dr Watson as to the propriety of attaining immobility of the knee as a result of the operation. In one of his cases, which had been cured with a stiff leg, having excellent power of locomotion, inflammation had subsequently taken place in consequence of hard work. A piece of bone came away, and although the bones were apparently in close and perfect apposition, mobility at the knee had ensued, rendering the limb useless for comfortable progression. The patient had been readmitted into the Infirmary, and was at present there, and the limb put up in Dr Watson's apparatus with the view of procuring immovable union.

Dr Joseph Bell had repeatedly seen the apparatus in use in Dr Watson's ward in the Royal Infirmary, and considered that it possessed very decided advantages. Dr Bell thought that Dr Watson, in his paper just read, had understated the success of his results. One case included among the fatal ones had died a year after the operation; and in two other cases, the operation had been undertaken under very unfavourable circumstances,—one being that of a lady, aged 52, long a martyr to dyspepsia, and the other, that of an individual whose bad general health was ill able to sustain the shock of a severe operation.

Dr Watson stated that, in his cases, while the average period, from the date of operation till complete consolidation and healing was 64 days, the average total residence in hospital was 145 days. The patients might, without risk, have been discharged from hospital at the earlier period, but had been in every instance retained, that the condition of the limb, as to usefulness, increased muscular development, and permanent consolidation, might be definitely determined before they were lost sight of.

PROCEEDINGS OF THE EDINBURGH OBSTETRICAL SOCIETY.

SESSION XXVI.—MEETING V.

23*d January* 1867.—Dr Charles Bell, *Vice-President*, in the Chair.

I. CASE OF UMBILICAL HÆMORRHAGE. BY DR LINTON.

I have thought of mentioning this case to the Society, not on account of any peculiarity which it presented, but with a view of eliciting from the members the result of their treatment in similar cases. Such cases, I had always been led to understand, were far from being of frequent occurrence, and were usually attended with great risk to the child; but from the frequency with which some of my professional brethren, to whom I had spoken on the subject, seem to have met with them, and from the comparative ease with which the hæmorrhage has been checked, I am inclined to think, either that the nature of the cases were not identical, or that there is a stage in the disease which, seen in time, is more amenable to the use of styptics than the one in which I found my case when first called. The former is the likelier supposition.

On 24th November, Mrs D. was delivered of her second child, a plump, well-proportioned, healthy-looking male. There was nothing worthy of remark about either placenta or cord, and there was no bleeding from the tied portion of the latter previous to separation, which took place on the seventh day after birth, the umbilicus presenting its usual appearance. On the forenoon of the tenth day, the third after separation, a slight oozing of blood was noticed, which, however, had not continued, as in the evening the rag which dressed the navel was perfectly clean. But on the following morning the child's clothes were found to be literally soaked with blood, the hæmorrhage having gone on to a considerable extent during the night. I was called about 9 A.M., and found matters as had been represented. There being no bleeding at the time, I contented myself by simply applying some pounded alum to the part, and covering it with a compress of lint. For an hour or two there was a cessation of the hæmorrhage, but about noon I was again sent for. The solution of perchloride of iron in glycerine was now used with a compress as before, but with only temporary success, the bleeding having commenced anew about 4 P.M., at which time I again saw it. I now tried to touch it with nitrate of silver, but from the difficulty of reaching the bleeding point, owing to the deep furrow in which it was situated, I was unsuccessful. Dr Stephenson, and Dr Currie of New York, who was with him at the time, now saw the child with me, and after again applying the perchloride of iron, but without the slightest effect, we resolved on trying the plaster of Paris, which I had previously provided myself with. Applying it moist we found it soon hardened, and for a time were in hopes that the bleeding had been checked, but soon we observed the surface of the plaster become tinged with blood, and on removing it found that the under surface was saturated, and that blood had penetrated its entire thickness, the plaster taking in the moisture in much the same way as bibulous paper. We repeated the trial in the same way, but with a like result. A third attempt was made, this time using the dry plaster. Taking a mould of the umbilicus, the concavity was considerably enlarged and filled with the dry powder, and applied over the bleeding part. For a time this seemed to have the desired effect, the bleeding having ceased for about five hours, but during the night it again broke out and was allowed to continue till I called about 10 o'clock next morning, the parents having objected to the part being transfixed. Nothing further was tried, and the child died about 4 P.M. There was a slight umbilical hernia. No jaundice had been remarked previous to the hæmorrhage commencing, but the anæmic condition of the child gave it the appearance as if it was suffering slightly from that affection. There was no post-mortem. With regard to the plaster of Paris, I think that it would have been successful had its softness not prevented any firm pressure being applied to it.

Dr Wilson said that lately he had seen a very bad case of umbilical hæmorrhage accompanied with congenital jaundice. He warned the friends that he feared a fatal issue, and the child died two days after separation of the cord.

Dr Charles Bell stated that the parts surrounding the navel were generally spongy, and there was a deteriorated condition of the blood, which deprived it of its power of coagulation, and in such cases he should advise the tinct. of muriate of iron to be given in full doses. He thought the only chance of success was by constitutional treatment. He recommended also compression with a half-nutmeg, which should be retained in its place by means of adhesive plaster and a bandage, as in umbilical hernia.

Dr Ritchie considered such cases more common in males than females. An interesting conversation ensued on the subject of hæmorrhage in general.

II. CASES OF PUERPERAL CONVULSIONS. BY MR FURLEY.

There is no class of cases the obstetric practitioner is called on to treat more formidable in appearance, more terrible in progress, or more fatal in results than that of puerperal convulsions. The friends of a patient suffering from convulsions are almost always put into such a state of consternation that they beset the medical man with anxious inquiries as to the prognosis of the case, and if he should happen to be a young practitioner, he is invariably requested to obtain the assistance of a professional brother for the purpose of having an additional opinion,—a circumstance which the young practitioner is only too glad to avail himself of, in order that his great responsibility may be shared.

The object of this paper is to bring under the notice of the members of the Society an account of five cases of puerperal convulsions that have occurred in my practice, and as the cases differ considerably from each other in their nature, progress, and termination, I trust the Society will pardon me if I remind them of the varieties of convulsions that may occur during the puerperal state. The different kinds of these convulsions are the hysteric, apoplectic, and epileptic. Of the two former, I have no case to record, and I believe are rare, especially of the apoplectic form. All the instances I have seen have been of the epileptic character, and are all the more interesting that they presented points of difference in their mode of invasion, in their period of occurrence, in their length of continuance, and in their termination. As to the frequency with which these cases occur, according to the statistics collected by Dr Churchill, one case of convulsions occurs in 618¾ labours. I have not kept a complete record of all the cases I have attended, but I am certain they have occurred much more frequently with me than that average would indicate.

Convulsions may occur during gestation, at the beginning, during, or after parturition. Of the five cases I have had, one occurred during gestation, one at the beginning, and three during the progress of labour. I shall first describe the case that occurred during gestation. Mrs W., aged 27, strong, plethoric, and of a sanguine temperament, with reddish hair, was pregnant, as she considered, about seven months with her first child. She had been complaining to her neighbours of her legs and feet swelling for some days before she was seized. The night previous, she complained much of headache, and during the night paced up and down her room. About six in the morning her husband persuaded her to go to bed, and shortly after she took a fit, and he, terrified, ran out of the house and roused the neighbours. On his return with assistance, the fit had subsided, and she remained in that semi-delirious or semi-intelligent condition which all of you have seen, and which I cannot describe. When I saw her she was entirely comatose, taking convulsions on an average every twenty minutes, sometimes two within a quarter of an hour. On making inquiries, I found that her confinement was not expected for two months, and on making an examination per vaginam, the irritation of which seemed to be the cause of a convulsion which immediately took place, I found that labour had not begun. I pressed the point of the finger upon, I cannot say *through*, the os. I could distinguish something round, which I thought was a head, I then applied the catheter, but all the urine I could obtain was little more than a spoonful, which, on being heated, coagulated immediately,—so much so, that it seemed one mass of albumen without a drop of fluid surrounding it. I make particular mention of this fact, as it appears of some importance in connexion with the treatment adopted, as will be taken notice of directly. Having then ascertained that the convulsions had occurred during gestation, that they were recurring more frequently as time wore on, and that the urine was highly albuminous, it appeared necessary to adopt the most active measures for her relief. The indications for treatment were of course, to diminish the force and frequency of the convulsions, and bring on labour as speedily as possible. To lessen the convulsions, then, her head was shaved and ice in a bladder applied to it. She was then bled to about twenty-five ounces, as I reckon two soup-plates to be. After the bleeding, the face and head, which had been highly congested, became more pallid, and the breathing not so stertorous in its character. After the lapse of three hours, acetate of potash in large doses, every two hours, was administered. To bring on the labour, the membranes were ruptured and a flexible catheter introduced, which was retained till the os was considerably dilated. These measures were adopted about noon; the convulsions continued till about seven in the evening, when they ceased entirely.

At one o'clock on the following morning, the os was sufficiently dilated to admit of the forceps being applied. I inserted a catheter into the bladder, but could obtain no urine. I then administered chloroform, and applied one blade of the forceps, and on placing the hand to apply the second blade, I thought that the head might be easily brought down by employing the inserted blade as a lever. This plan succeeded, and a fœtus apparently of six months, and dead, was delivered. Ten hours afterwards I applied the catheter, drew off about six ounces of urine, which, on being heated, did *not* coagulate. Thus in twenty-four hours after the treatment I have mentioned had been adopted, the albumen had apparently disappeared. At this time she was still comatose, but towards evening she began to speak, calling her husband by name, inquiring what o'clock it was, etc. Next day symptoms of peritonitis set in, which continued to increase in severity till the fourth day, when she died.

The next case I shall bring under your notice is one in which the convulsions commenced at the beginning of labour. It is singular that the appearance, in almost every respect, of Mrs M., was the same as that of the patient whose case I have just described. Strong, plethoric, short-necked, of a sanguine temperament, with reddish hair; her age, however, being only twenty-two. The first convulsion occurred about ten o'clock at night, when I was immediately sent for. When I arrived the fit had passed off, and she appeared quite intelligent, and answered all the questions I put to her. On examining per vaginam, I found labour had begun, and the os dilated to the size of a shilling. No sooner was my examination completed, however, than she took a convulsion, very severe in its character. Cold was applied to the head after being shaved, and she was bled to about thirty ounces. Under this treatment the os gradually dilated, but the convulsions occurred as frequently as ever; their force, however, was considerably diminished by the inhalation of chloroform, which was administered at each recurring fit About fifteen hours after first seizure, she was delivered by the forceps of a child apparently at the full time, but dead. It was healthy-looking, and had died during the process of parturition, as the fœtal heart was heard at the beginning of the labour. After her delivery, the urine remained still albuminous, and the convulsions still continued in unabated force and frequency until the evening of the next day, when she expired. In this case the patient could not be got to swallow. Injections were then had recourse to, but each attempt seemed to bring on a fit, and that method of administration was also abandoned. Acetate of potash was not, nor could be, given.

The next case I shall describe, is that of Miss H., aged 17; little, genteel, fair-haired, and not by any means strong. She had imprudently become attached to a young man, who was lodging in her mother's house. When I was called to see her, no suspicion seemed to be entertained of her condition. She simply complained of a sore belly, and on applying my hand over the abdomen, so flat did it seem, that my suspicions were barely aroused, when fortunately she took a pain; this pain seemed so much like a labour one, that I requested her to lie on her side, and examined per vaginam. There was now no mistaking the nature of the case, and her mother inquiring what was wrong. "What's wrong," said I, "why, she is in labour." No sooner had I said the words than she took a convulsion, during which the child was born. It was the smallest living child I ever saw. When the convulsions ceased, the child was separated. On proceeding to detach the placenta, she had another convulsion quite epileptic in its nature. She had in all seven, and then became comatose. Next morning she began to speak nonsense, and in the evening was maniacal. She remained so for nine days, and was gradually restored to her senses. In this case, the patient had completely concealed her condition by tight-lacing. I am not generally inclined to believe mothers' statements, when they say that they knew nothing about it; but in this case, I really believe that the pregnancy had been effectually concealed. I never ascertained whether there was any albumen in the urine or not, the convulsions were so few and rapid. I may mention, however, that the child lived twelve months.

The fourth and fifth cases were not remarkable. Both were slender-made women, in their first pregnancies. The convulsions came on when the os was almost fully dilated. Albumen was found in the urine in both cases. Both were delivered by the forceps under chloroform. The fits were not numerous. The coma lasted only a few hours. Both mothers and children survived.

Remarks.—In the foregoing cases it will be observed that those that were fatal, were the women that were strong and plethoric, and the three that were of a slender make, recovered. In the plethoric cases the convulsions were more numerous and more severe, and notwithstanding their being bled to twenty-five or thirty ounces, neither of them got better. The remedies I should be inclined to rely on in such cases are bleeding, to an extent that might be considered sufficient to produce syncope, if the patient were conscious, and acetate of potash in large doses, frequently repeated, and administered by the mouth or rectum. In those cases where the patient is of slender build, I think there is nothing better than the inhalation of chloroform.

Dr Rosa said he had seen five cases of puerperal convulsions, all of which he had treated by copious bleeding, and in all cases successfully. Each case occurred during labour, and in one case only he used forceps. One patient remained comatose for some days, and was bled from both arms.

Dr Charles Bell approved of bleeding, even to the extent of forty ounces. He maintained that it was correct practice to bring on labour if the child was dead, otherwise inflammatory action might ensue.

Dr Sidey had seen about twenty-five cases of the same kind, all of which recovered but one, and she was not bled. He disapproved of labour being brought on in order to prevent convulsions. He mentioned an interesting case of uræmic convulsions in a man. When he saw him, he looked as if he was dead. He was comatose. He tried to bleed him, but unsuccessfully at first. Afterwards the blood began to flow, and the patient became sensible after he had lost two cupfuls. He rallied after having been bled to sixty ounces. The convulsions returned; he was bled again to forty ounces, and ultimately recovered. Convulsions may be kept up by retention of urine pressing on the uterus after delivery. When the urine is drawn off, convulsions generally cease.

Dr Linton had met with three cases of puerperal convulsions. Death followed in one case, even after bleeding.

CONVERSAZIONE OF THE LOTHIANS' MEDICAL ASSOCIATION.

ADDRESS BY SIR JAMES Y. SIMPSON, BART.

ON Thursday, 14th March, the Lothians' Medical Association held a conversazione in No. 5 St Andrew Square, Edinburgh, which was attended by a large number both of town and country practitioners. The chair was occupied by Sir James Y. Simpson, Bart., in the unavoidable absence of Professor Christison.

The CHAIRMAN, in opening the proceedings, said—I regret the absence of Professor Christison very much upon this occasion, because he knows the objects and aims of this Association much better than I do. You are aware, as well as I am, that the leading aim and object of the Association is to attempt to elevate the whole profession, by elevating the status of the individual members, and at the same time to bind us all together by more kindly ties of brotherhood and good fellowship. After referring to the expensive education required by medical men, the great objects they have in view, the harassing nature of the pursuits, and the little sympathy and consideration shown by the public in the sending of messages, the Chairman went on to say—Despite all this, however, we must candidly acknowledge to ourselves that there is no profession perhaps—with the exception of the clerical—if even that is an exception—that is more begrudgingly underpaid than ours. Some quarter of a century ago, when the income-tax was first inflicted upon us, every man being compelled to pay it who had £150 per annum of revenue, my old master at Bathgate, Dr Dawson, told me that he calculated that perhaps there was one, but not more, of our profession in the whole county of West Lothian that would be liable for payment of the income-tax—only one man who had £150 per annum of income. These matters are better now; but I suspect that, even with all the improvement that has taken place, the present state of matters is one which requires still to be greatly amended. Now mark you this, that we have nothing to bring into the market of the world except our skill and our time. We have no goods to barter for the gold of the world except our advice, and the time which the patients may require at our hands; and the members of the medical profession have most lavishly poured out their time, their advice, and their skill upon all ranks of society, from the highest to the lowest, and principally and especially to the lowest. And I think that it is quite possible to prove that in Scotland we have done it, and do it still, without any adequate payment, to a much greater extent than it is done in England or in Ireland. At the present day we are told, by the report of the poor law, that to doctors in charge of the pauper inhabitants of different parts of Scotland, there was expended last year some £30,000, of which sum £10,000 was furnished by the Government; and that is a mighty small dividend for such an enormous amount of labour as is devoted to the paupers. But what I especially want to say upon this subject is this, that it is a very small dividend to Scottish practitioners, compared with the sums which are claimed and enjoyed for this department of labour by English and Irish practitioners. We expect that there will be very soon a revisal by Government of the poor-laws of Scotland. Will it not be then the duty of such an Association as this, composed as it is of a large number of those who are placed in charge of paupers in their several districts, to urge upon the Government such a change as will at least make the Scottish practitioner as well paid as his brethren in England and Ireland are for the same duties. We have at present some 160 parishes in Scotland that refuse, forsooth, to ask the Government fund, because they require to supplement it; and I would beg to observe, that if any of these 160 parishes are within the bounds of the Lothians, it would be the duty of this Association to try to stir up such a commotion as would make them, for shame's sake, go and pay their medical men better, and have the Government funds distributed amongst these medical men. Then there is another matter in which we are underpaid in Scotland in relation to the poor, namely, that the medical man who attends the poor receives a certain amount, without any extras for extra cases, whereas in England they are paid extras for midwifery cases, and for every kind of operation; and we must concuss the Government, when there is a revisal of these laws, to put us on the same status as the medical men on the other side of the Tweed. Passing from that, let me say that vaccinations in England are paid at the rate of from 1s. 6d. to 2s. 6d. for every person that is vaccinated. You are aware that in Scotland our authorities will only begrudgingly give the money to the poor-law doctors for the vaccination of paupers, while in England they are paid for the vaccination certificate of all who will be vaccinated. An English medical gentleman lately stated to our secretary that he got for his last quarter's vaccination £32; and why should we be so greatly underpaid here, compared with the payments made in England? I repeat that question, and I would beg to repeat it again and again. Vaccination is a thing which is forced by law upon every one; and we should be quite wrong to submit to being improperly paid for that little operation—an operation, however, which is of great consequence to society, from the effect which it has in averting disease, and lessening the rate of mortality. I am informed that it is the practice of many medical men in Edinburgh, and of some in the country, to vaccinate the children of their patients without asking a fee for it at all. I cannot see the least degree of justice in a medical man bestowing the time necessary for this operation for nothing. We will not find that we get anything corresponding from the tailor, the butcher, or the baker; and why should we throw our time away? It is calculated that if every person vaccinated in the three Lothians were to pay for the vaccination 3s., it would give an income of £2000 a-year to the medical profession. Supposing you started a widow's fund, that sum would yield £50 a-year to forty widows; and why should all that money be thrown away? There is no necessity for it. It is unnecessary and gratuitous charity, in every sense, and I would beg those who do it to give up the practice and charge for vaccination. To the higher classes 3s. would be infinitely too little. Supposing the average charge were 1s. 6d., still that would give an increase of £1000 a-year to the income of the profession in the Lothians. Perhaps there is much greater difficulty in the medical man recovering sufficient remuneration for medical attendance and advice from the class immediately above the paupers, than from the poor-law authorities for paupers themselves. We have a class of poor that do not come under the Poor-law Act in any way, and it is the medical charge of that class that constitutes one of the greatest medical problems in the profession at present. In order that these may have attendance we have numerous dispensaries, and I think that one object of this Association should be to bring about a revisal, to some extent, of the rules as to the kind of persons to whom dispensary aid is given, because there is no doubt of the fact that it is at present greatly abused, to the great loss of the profession, on account of people crowding to them who ought to be paying a regular medical attendant. To enable you rightly to understand this, let me merely say, that since coming to the room I have had a talk with a gentleman, who tells me that there came not long ago to the dispensary with which he was connected a manager of a bank, and a major in the army; and we know also that there come not unfrequently to the Sick Children's Hospital ladies in silk dresses, sometimes in cabs with their servants bringing their children for advice. Now, I say that that is utterly wrong; those who can afford a cab and a servant should be able to afford payment for the doctor. We have nothing but our advice and skill to give to the public, and if they come and purloin these from us it is wrong. We ought to put ourselves strongly against this kind of medical kleptomania that is perpetrated by many people in Edinburgh. The medical problem how to provide sufficient medical aid for the class of people immediately above the pauper class is one well worthy of the consideration of this Association, and in considering it we must keep in view that we do harm rather than otherwise by direct help. The help we ought to give them is to teach them to help themselves, and in some towns that has been carried on most successfully. You will find, for example, that in Northampton there has existed for twenty-two years an institution where all the patients are obliged to pay a small sum according to their condition; because, mark you, when they do pay, they think much more of the advice than when they get it gratuitously. What was the result in Northampton? The first year they only collected £28 in all; but in 1865, the last year for which I have the report, they collected at that dispensary in small fees from these poor patients, three medical gentlemen taking charge of them, £1245, and the account which I read stated that, after deducting all the expenses, these three medical men divided amongst themselves £918, being upwards of £300 a-year to each; and I say it is our duty, for the good of Edinburgh, to find out how they get on there, in order to transplant, if possible, such a system here. Then, it is still more necessary that something of this kind should be done for our brethren in the country, who are much more seriously taxed than our brethren in the town in this and many other respects, because there being no dispensaries the whole onus of attending to these cases, and even, in some instances, of keeping up the people charitably, is devolved upon the poor country doctor, who is sometimes obliged not only to be physician, but also nurse and host to poor patients. Why should there not be clubs and dispensaries in the country? This is a subject that would require great consideration by a committee; but surely it ought to get the most anxious attention of such an Association as this. I might go on further to observe, in relation to the medical profession, that sometimes we are asked to perform public duties without the least payment, and asked without compunction on the part of those who make the demand upon us. Sir James here referred to the large amount of time given gratuitously to hospital practice, and alluded to the custom becoming prevalent in some parts of England of paying the hospital surgeons and physicians. Then, we have others of our brethren who are not attached to hospitals, but who hold public situations, and who are comparatively under-paid. I believe there is no man better fitted for the situation which he fills, and that one of the most important situations in the city, than Dr Littlejohn, our officer of health; and yet, I believe the Town Council, forsooth, think they do a wonderful thing when they pay Dr Littlejohn a paltry £500 a-year, and prevent him practising. Why, they pay the town clerk, whose education was not nearly so expensive, £1000; and they give the procurator-fiscal, whose education also cost much less, £1500. I do not say that these officers get too much; but I say that if they get so much, Dr Littlejohn gets far too little. Then we have a class of patients to think of who are higher than those I have spoken of,—those belonging to the middle and upper classes, and amongst them there is a class who do not always pay well,—though I hope there are many exceptions, many who pay lavishly and pay well,—and let me say that I for one do not complain; but we are met to try to do good to each other, and particularly to those of our brethren who belong to the country, and have all the hardships of rural practice to encounter. Some of the middle and upper class often pay the doctor last. They have a certain sum to pay away at the New Year, and the last to be paid is the doctor, who often gets only what remains, and that rather grudgingly. Then we hear them very often complain of fees that are not extravagant. A friend told me that the grieve on one of his farms, four miles from Kinross, being ill, was asked who attended him. The reply was, "Dr Dewar." He was then asked, "Well, did you like him?" "Oh, he's a capital doctor, but he charges awfully." "What does he charge?" "Do you know, sir, he charges 2s. 6d. a-visit. I could get a cow-doctor for 5s." There was a time when things were a little better in relation to midwifery, when the custom was that the patient collected her fee, and knew what was required; and I am told that many a country practitioner now would be very glad to bargain for a guinea or half-guinea paid upon delivery,—paid upon the spot. Various cases were given where the medical charges had been disputed,—one where the practitioner was actually refused a less sum than a sheriff-officer would have demanded for going the same distance,—a man whose occupation necessitated only sufficient education to enable him to sign his name. It is also well known that patients often expected medical men to go distances for less money than a street porter would charge to go the same distance. Then we find things equally startling with regard to fees in cases where there should be no dispute. Dr Keiller was lately offered £15 for going to St Andrews, and remaining over the night with a lady who died worth £60,000, the lawyer adding that he thought it quite enough. Then we have in Edinburgh, but still more, I am told, in the country, particularly in the middle classes and those a little lower, a class of people who go from one doctor to another, because in this way they get off without paying one at all. After showing how much the cultivation of friendly feeling among medical men would tend to counteract this state of things, Sir James continued. What we want is combined action in the whole matter. I believe that our profession is underpaid by at least £100,000 a-year in Scotland. I say that charging what is requisite, and right, and just, will not detract from the character of the medical man. I hope you are not to judge of me, and I know I am not to judge of you, as wishing to inculcate the doctrine that we are, as medical men, to abstain from giving advice except when there are fees. The medical profession pours out in the lap of charity far more of the goods it possesses than any other profession; and it will never stint its charity. But still that is no reason why we should not try to improve our own status by getting fees which are justly due to us. We require food, clothing, and lodging as well as other members of society, and some of us have wives and children to feed also; and why should we stint them by charging improper fees? I think, on the contrary, that we degrade the profession by taking a low pecuniary view of the value of our professional services. I have heard it argued against this Association that it was established for the purpose of trying to raise medical fees, and that it was derogatory to do so. It is not. Look at what the lawyers have done. Not very many years ago, judges had £500 a-year, and now the lowest salary of our judges is £3000. They have beaten up and argued till they got that improvement. Have they derogated from the character of the bench by this? Certainly not. The sheriffs, a few years ago, had, many of them, only £200 or £300 a-year; and now the lowest salary paid is £500. Have they derogated from their character in the slightest degree by urging the propriety of their being better paid? Then look at the clergy. They are constantly trying in the Established Church to get an augmentation of stipends from the teinds; and it is quite proper that they should do so. I belong to a Church where there is a sustentation fund, and they do not think there is anything derogatory in trying to increase the subscriptions to that fund. It is a false Scotch pride, in my opinion, not to face the question courageously, and not to say, as we are perfectly entitled to say, that the profession of medicine is greatly underpaid. Those members of the profession, therefore, who abstain from helping us in such a good cause seem to me to be just conniving at the medical profession being under-paid, and assisting the public to keep us down in regard to payment.

Professor BENNETT said—I must confess, that when I heard of the proposed formation of this Association, I looked forward to it as one of the means which I thought best calculated to raise the character of the profession in Edinburgh and throughout Scotland. I think that, if well managed, and especially if we can get a large number to join us—because that is essential—then any series of resolutions which might be drawn up with regard to those matters affecting the welfare of the profession may be carried out with some hope of doing good. Hence my great desire, as a member of Council, has been to see that, in the first place, an effort be made to increase our members. I am told that in the Lothians there are somewhere about two hundred medical practitioners. Now, I think, if we could get two-thirds of these, it is very possible that any resolutions which we might adopt might be so impressed upon the rest that we might have a hope of carrying them out; but if this Association is to consist of only a small number, and a considerable number stand aloof, especially if seniors stand aloof, it is very much to be feared that any resolution we might arrive at would not be generally carried out. I would therefore beg, in the first instance, to suggest that an organization should be established—and I think, from the number of gentlemen that I see around me, that this is perfectly possible—for the purpose of bringing into the Association the great majority of practitioners in the Lothians. This is of great importance, because any resolutions that a small number might pass would be of little value. With regard to the general purposes for which this Association has been established, you are of course all familiar with these. From the tenor of the observations which have already been made, it is perfectly clear that the paramount idea is the subject of fees. That was the subject that first led to the formation of the Association, and I must confess that that is very naturally the subject that ought to be first discussed. Now, on this point I would observe, that there is a vast difference between the remuneration that is offered to the medical practitioners in England and in Scotland, and there appears to me no good reason for this whatever. At the same time, it is very embarrassing that in the same profession the remuneration on one side of the Tweed should be different from that on the other. It requires the same time to educate for the profession here, and it demands the same ability, and it seems altogether contradictory and unreasonable that the remuneration should be different. But if you contrast the remuneration offered to the Scotch with that offered to the English practitioner, you will find that the English practitioner is far superior. If you go into country towns or large cities in England, you will find the medical practitioner enjoying a very large income and holding a very high position. There may be such incomes in Scotland, but they are far more numerous in England than in the north. A great many more anecdotes as to improper fees might be related; but the important question for us to consider is, How is this to be obviated? what is to be done in order to remedy the evil? In the first place, we must try to test the cause of the present state of things, and I think it will be obvious that we are to blame ourselves for this matter. I am not aware that there is a greater number of practitioners in Scotland, compared to the population, than in England, or that there is greater opposition amongst us here than there is in England among medical men. Therefore, reflecting on the matter, I cannot help thinking that we have ourselves to blame entirely. If it is our own fault, then it can only be remedied by our own determination. Hence it follows that an Association of this kind, if it is sufficiently extensive, and if it comes to certain resolutions, is the only means by which this state of matters can be altered. A good deal has been said about the great amount of gratuitous attendance. I think we should lose no time in appointing a committee of country and other practitioners to examine carefully into this matter, and perhaps they might agree upon a scheme for a rate of payment which, if not imperative, might at all events be considered the rate which it was proper to demand in cases such as those which had been mentioned. In this way we should fall upon some practical means of remedying the evil which is at present admitted to exist. Then, in the same way, I believe there is a great deal of difficulty or uncertainty as to what practitioners ought to do in certain circumstances; and a committee might be appointed in order to determine what ought to be the rate of remuneration that should be demanded under most of the circumstances which present themselves to the medical practitioners. Then, lastly, there is a great deal about consulting fees and other matters, all of which deserve very great consideration from such a committee. I believe myself that the public, if they were only made aware of how the medical practitioner ought to be treated, would be very willing to do what they thought right; and I think we have been very lax in taking too small sums, and that, in consequence of this, the public have gradually got the idea that anything will serve the purpose of remunerating the doctor. The question comes to be, whether an Association of this kind could frame rules on such subjects, and then give publicity to them, so as to inform the public what should be done. I would propose that the Association should immediately proceed with the appointment of a committee to consider the best mode of acting with reference to these matters. Then I think that there is another very important matter, and that is that we should try and bring about an assimilation in the remuneration in England and Scotland. After referring to the reduction of consulting fees in England and Scotland after the introduction of railways, Dr Bennett continued,—I think that the London physicians, in diminishing the fee from one guinea to fourteen shillings, did not diminish it quite enough, whilst I think that the Scotch consulting physicians brought it down too low; and I think, that through the agency of an association of this sort, a little communication might be had with our London brethren with the view of trying to make the fees equal. I should say that in these days of railways half-a-guinea a mile would be a reasonable fee; and if, by associations of this kind, we could bring about something like unity of feeling, it would tend to diminish those inconveniences which most of us have more or less experienced. You remember that celebrated case of Dr Bowes and Sir Thomas Gladstone, where he asked that fee, and was told that a Liverpool physician was not entitled to ask so much. I think we should try to frame a body of rules to which we should have the means of appealing as being the rule of the profession; and if we could adopt that, I think that a great deal of bad feeling would be removed and a great deal of advantage would result. A great many people now-a-days grudge a guinea for a consulting fee. Now, a guinea in the days of Queen Anne and the first George was worth three or four of our modern guineas, and yet that was the usual fee. I cannot help thinking that we should modify these matters—that whilst we try to increase the fees for our country brethren, and regulate the consulting fees, and fees for distances, we should also consider what ought to be the higher consulting fees, which ought to be increased also. These are questions which should be considered by a committee, and if we could only come to some harmonious conclusion upon them, I cannot but think that it would tend very much to the good of the profession and to the harmony of all classes in it.

Dr KEILLER said, that twenty-five years ago he was connected with the Eastern Medical Association of Scotland, which had the same objects in view as this Association, and which failed in carrying them out from the want of unanimity among the members. The Eastern Medical Association was established in Dundee, and embraced the counties of Forfar, Perth, and Fife. They held several large meetings, but when they came to discuss the subject of fees there was a hitch. A medical committee was appointed, of which he was convener, and at one of their first meetings it was proposed that they should try to get up a code of fees. Out of four hundred schedules sent out to medical practitioners only thirteen were returned, and opinions expressed in those thirteen were so dissimilar that they were unable to frame a code of fees. Unless, therefore, this Association was supported by a great many practitioners in the Lothians, and unless they appointed a very large committee, he feared that the result in regard to fees would be the same as in the Association to which he had referred. At the same time it would be of great advantage to have a table of fees to fall back upon in certain circumstances. In the case to which the Chairman had referred in which his fee was disputed, the lawyer, in a letter addressed to him, said that it would be greatly for the advantage of the public if a definite scale of fees were published, for it was a constant source of perplexity to executors. No doubt it would be of great advantage to have a table of fees—not that they should exact the fees in all cases, but that they might be able to fall back upon the table in cases of difficulty.

Dr STEPHENSON said that in reference to the problem of supplying with medical advice the class of poor patients above paupers, there were certain broad principles which ought to be attended to. In the first place, no person who was not a pauper should be considered unable to pay something for medical advice. It deprives a man of his feeling of independence, and fosters improvident habits, to bestow medical relief upon him for nothing. The present system of charging by the visit was inapplicable in his case, for it would be derogatory to the profession to accept such a fee as his means would permit. Then, again, we must regard another important principle, that no obstacles in regard to fees should be thrown in the way of a poor man obtaining prompt and efficient relief. He thought some means similar to insurance might be adopted, where the annual sum charged would be proportioned to the income of the patient, while in the aggregate it would afford medical men better remuneration for that class of work than they at present receive. He would, however, put it on a different footing than the English Club and Provident Dispensary. These institutions violated other principles, inasmuch as they did not leave the patients at liberty to select whatever medical man they chose, or three or four medical men held out inducements to other gentlemen's patients to leave them, who were not attached to such an institution.

Dr Moir, Dr Fowler, Corstorphine, Dr Gordon, Juniper Green, Dr Jefferiss, Dalkeith, Dr Messer, Penicuik, Dr Carruthers, Marchfield, and others took further part in the discussion. After a vote of thanks was passed to Sir James Simpson for his address, and to Dr Stephenson, the Secretary, for his labours in arranging the meeting, the company partook of tea before separating.

As the objects of this Association are not as yet generally known, we subjoin an *Extract from the Constitution.*—" The general objects of the Association shall be to procure combination of action in all matters affecting the interests of the profession, and to promote friendly feeling among the members. These shall be arrived at by (*a.*) Watching all bills that may be brought into Parliament in which the interests of the profession may be involved, and taking action thereanent if it be so resolved.—(*b.*) Giving aid and comfort to any member who may be unjustly made the object of legal prosecution, in connexion with professional duties.—(*c.*) Supporting the just claims of the Medical Department of the Army, Navy, and other public services.—(*d.*) Recommending, and as far as may be introducing into the usage of medical men such practices in regard to fees and other matters as may best secure the interest of the members, and the dignity of the profession.—(*e.*) Holding occasional meetings, at which the affairs of the Association and subjects of general importance to the profession may be discussed, and decided upon if thought necessary.—(*f.*) Granting funds for the prosecution of the Science and Art of Medicine, in any of its departments."

Part Fourth.

MEDICAL NEWS.

THE LATE JOHN GOODSIR,

PROFESSOR OF ANATOMY IN THE UNIVERSITY OF EDINBURGH.

ALTHOUGH preceded by several years of disabled health, and at the last expected for some weeks before its occurrence, the death of Professor Goodsir has, nevertheless, produced a profound impression. The career of a great genius stopped before his work was done; immense stores of information, the result of years of indefatigable research, now perishing with their possessor; the withdrawal of a noble example of learning and labour, which all might admire, and some might strive to emulate; the extinction of a lofty enthusiasm for science, which acted like an inspiration on the generous youth who listened to his instruction; such are the losses to the University and to Science, which Goodsir's death entails, and which, for a time at least, must be irreparable.

John Goodsir was born in 1814, at Anstruther in Fifeshire, where his father and grandfather were medical practitioners. He early displayed great talent, and when little more than a boy was sent to the University of St Andrews, where he had the advantage of passing through the curriculum in Arts. Intended for the medical profession, he was next apprenticed to Mr Nasmyth of Edinburgh, the eminent dentist, and during his apprenticeship he attended medical classes, both in the university and under extra-academical teachers. He studied anatomy under Dr Knox, and natural history under Professor Jameson, and he soon evinced a peculiar aptitude for anatomical research. At this period, he formed one of a band of distinguished students who have since risen to eminence, including Edward Forbes, Samuel Brown, George Wilson, Sir James Y. Simpson, and Professor Spence. After obtaining the license from the Royal College of Surgeons, Mr Goodsir returned to Anstruther to assist his father. Already inspired, however, by an enthusiasm for science, he found time, amid the distracting duties of medical practice, to pursue his anatomical investigations. The position of his native town on the seacoast afforded him a rich supply of material for researches in comparative anatomy, of which he availed himself with characteristic ardour. In addition, his indomitable industry enabled him to collect and prepare a pathological museum of considerable extent and value, which, while chiefly illustrating morbid anatomy, included also specimens of healthy structure. Among other objects of interest this collection contained preparations illustrating the ulcerations of the intestines in typhoid fever, which at that time were little known in this country. In 1839, he published in the Edinburgh Medical and Surgical Journal, his now well-known memoir on the development of the teeth. This paper at once attracted the attention of scientific anatomists, both at home and abroad, and established his reputation as a great original observer. Soon afterwards the office of Conservator of the Museum of the Royal College of Surgeons fell vacant, and Goodsir was appointed, chiefly through the influence of Professor Syme, who early recognised Goodsir's genius, and did all in his power to promote his advancement. Goodsir now devoted himself exclusively to anatomical and physiological research, and his unwearied industry was rewarded by ample results. In conjunction with his friend Edward Forbes he published some interesting papers on marine zoology. About the same time, he discovered the remarkable parasitic *sarcina ventriculi.* But his most valuable discoveries and observations were thrown into the form of lectures on various departments of anatomy, physiology, and pathology, which he delivered before the Royal College of Surgeons in the summer of 1842, and the winter of 1842-3. These lectures were never published; but the principal part of what was new and important was communicated to the Royal Society of Edinburgh and printed in their tranactions, and afterwards appeared more fully in the "Anatomical and Physiological Observations," which he published in 1845 in conjunction with his talented brother Mr Harry D. S. Goodsir, who succeeded him as Conservator of the College of Surgeons' Museum, and who afterwards perished in Sir John Franklin's last voyage to the Arctic regions. This treatise, short as it is, is the most important work which Goodsir has left. The papers, unusually condensed, are full of originality, and opened up new views in Anatomy, Physiology, and Pathology. The chapters on Centres of Nutrition, on the Structure of the Placenta, on the Structure of Lymphatic Glands, on the Structure and Economy of Bone, on the Functions which Cells perform in the Processes of Absorption and Secretion, on the Ulceration of Cartilage, etc., indicate the extent and variety of his knowledge, and prove how completely and intuitively he had mastered the cell-theory of Schwann, and how capable he was of extending that theory to further and original developments, both in Physiology and Pathology. In acknowledgment of this, Virchow has recently dedicated his work on Cellular Pathology to Goodsir, as "one of the earliest and most acute observers of cell life," and it is generally allowed that the observations of Goodsir respecting the origin of morbid products from the physiological constituents of the textures are a happy anticipation of the fundamental doctrine of Virchow's cell pathology.

On the retirement of Mr William Mackenzie, in 1844, Goodsir succeeded him as Demonstrator of Anatomy in the University of Edinburgh; and the Chair of Anatomy becoming vacant in 1846 by the resignation of Dr Monro, *tertius*, the Town Council, by an eminently judicious choice, appointed Goodsir to the professorship.

Goodsir had now attained the object of his ambition, and immediately set to work to avail himself of the opportunity which his high position afforded for the cultivation and advancement of his favourite science. The class of anatomy, which, in consequence of the third Monro's utter incompetence during a long incumbency, had dwindled down to small numbers, and was completely disorganized, soon rose to be the largest and best conducted class in the University, numbering in the winter session from 300 to 400 students. It is hardly possible to exaggerate the enthusiasm with which the new Professor inspired the students not only for human, but also for comparative and physiological anatomy. Besides his ordinary series of lectures, Goodsir delivered voluntary courses of lectures on comparative anatomy in summer, which his artistic skill enabled him to illustrate fully, and which were largely attended both by students and by professional men and amateurs. During the illness of Professor Jameson, he conducted the class of natural history, lecturing chiefly on zoology. Occasionally also he gave special lectures on points of anatomical science which were exciting particular interest at the time, such as the minute structure of the retina, the lamina spiralis of the cochlea, the principle of construction of joints, etc. These additional lectures he continued to give till a few years ago. While discharging these professorial duties, Goodsir devoted all his leisure time to the enlargement of the Anatomical Museum, and prepared a large series of specimens illustrative of comparative anatomy, which still remain in the University, and form the most beautiful part of the collection.

In 1850 Goodsir, in order to encourage original research, commenced a scientific periodical,—the Annals of Anatomy and Physiology,—of which journal only three parts were published. About the same time, he stood candidate for a surgeoncy in the Royal Infirmary, which, however, he did not obtain, and he soon after gave up practice altogether. Full of plans, and eager in their prosecution, Goodsir worked indefatigably and without remission. The day and the greater part of the night were spent in anatomical investigations; the intervals for food and sleep were curtailed or omitted. His powerful frame, long overtaxed, at length began to give way, and 1853–4, he was compelled to relinquish his duties, and spend a year on the Continent. During his absence, the anatomical class was conducted by Dr Struthers, then the extra-academical lecturer on anatomy in Edinburgh, now Professor in Aberdeen University. On his return, Professor Goodsir resumed his duties with all his former energy and success. The disease which had attacked him, however, although apparently arrested for a time, made slow and insidious progress, and assumed the characters of the intractable locomotor ataxia. Notwithstanding his impaired strength, and the attacks of pain from which he suffered, Goodsir refused to relax his labours, and, unfortunately, persisted in toiling with the same indefatigable excess to which he had accustomed himself in earlier life.

It is greatly to be regretted that during his professorial life he published so little. Papers on the pathology of bone, on animal electricity, on the morphology of vertebrata and invertebrata, together with addresses to the Royal Medical and Medico-Chirurgical Societies, were only slight indications of his activity. With ample material prepared, conversant with the most recent progress of the science, and fertile in original views, which found occasional expression in his lectures, he displayed a deep-rooted aversion from publication. Abhorring whatever was crude and superficial in medical literature or science, Goodsir postponed publication till his views should have attained that completeness and maturity which formed his ideal—an ideal which his crippled health never allowed him to reach.

Early last year, Goodsir was laid aside by an attack of illness which excited some apprehension, but he fortunately recovered. It was no doubt injudicious in him to resume his professorial duty last November; at Christmas he was obliged to cease lecturing, and Mr Turner, who for many years has ably assisted him as demonstrator, was appointed to conduct the class. Goodsir's strength now rapidly gave way; aware of his approaching end, he prepared himself to meet it; and on the afternoon of 6th March he quietly breathed his last, at Wardie, near Edinburgh. On the 11th March, the funeral, though strictly private, was attended by a large number of his students. The pall-bearers were his brother the Rev. J. T. Goodsir, Mr Nasmyth, Sir James Y. Simpson, Bart., and Professors Syme, Balfour, Spence, and Henderson, According to the wish he had expressed, and which, fortunately, could be accomplished, Goodsir's mortal part is placed in the beautiful Dean Cemetery, beside that of his friend Edward Forbes.

This sketch of his life, short and imperfect as it is, renders any extended comment or eulogy unnecessary. Goodsir had been long recognised as one of the first anatomists and physiologists of his time, during a period when anatomy and physiology have surpassed all other sciences in the rapidity and extent of their advancement. The comparison, which at once suggests itself, is with the great John Hunter. Like him, Goodsir was familiar with the practice as well as with the science of medicine, and like him, he based his discovery of principles on an induction which embraced the whole field of biological science. Accurate, and indefatigable in investigating the details of structure, Goodsir was especially remarkable for the penetration and the comprehensiveness of his genius, and for the bold originality of his conceptions. Every department of anatomy he had studied minutely—human, comparative, morphological, physiological, pathological — and each department he had enlightened by his discoveries. *Nullum genus non tetigit, nullum quod tetigit non ornavit.* To those who have watched with pain Goodsir's great powers struggling resolutely, but in vain, against the inroads of the disease which began so early in his career, it has long been apparent that the great plans which he had proposed—the completion of an unrivalled museum of comparative anatomy, and the full development of his views on life and organization, organic forces and organic forms—must remain unaccomplished, and that the major part of his genius and work must perish with him. It is doubtful if he has left writings in a condition ready for publication. It is more probable that to the numerous eminent pupils whom he trained may we look for those fruits, of which he lived only long enough to supply the germ.

GOODSIR FELLOWSHIP IN ANATOMY AND PHYSIOLOGY.

On the 20th March a preliminary meeting was held in the hall of the Royal College of Surgeons—Dr Dunsmure, President of the College in the chair—to consider the propriety of founding a Fellowship in Anatomy and Physiology in commemoration of Professor Goodsir. Unanimous approval of the project was expressed, and the following gentlemen were named as the nucleus of a committee, with power to add to their number, with the view of raising the necessary funds, and taking other steps towards the establishment of the proposed Fellowship:—Sir David Brewster; Professors Christison, Syme, Sir J. Y. Simpson, Bart., Maclagan; Dr Smith, President, and Drs Burt, Sanders, Matthews Duncan, Craig Maclagan, Fellows of the Royal College of Physicians; Dr Dunsmure, President, and Drs Andrew Wood, Handyside, and

Joseph Bell, Fellows of the Royal College of Surgeons; John Muir, Esq., LLD., Member of the Edinburgh University Court; and David Smith, Esq., Treasurer of the Royal Society of Edinburgh—Dr Craig Maclagan to act as Honorary Secretary. We have no doubt that the late illustrious Professor's pupils, in various parts of the world, will desire to contribute toward a memorial so suitable, and so permanent, of their great teacher.

NAVAL MEDICAL DEPARTMENT.

The profession will have noticed with gratification that the Board of Admiralty have, in great part, removed one of the grievances complained of by the medical officers of the navy, by giving effect to the Order in Council of May 13, 1861, which provides that "Medical officers shall be entitled to the same honours as other officers of the Royal Navy of equal rank."

The *Gazette* of 15th ult. contained the promotion of Dr Deas, C.B., Inspector-General, to K.C.B.; and of Dr Davidson, Inspector-General, two Deputy Inspectors-General, and one Staff-Surgeon to C.B.

No doubt the *decorés* won their laurels a good many years back—so long ago as the Russian War; but we regard this instalment as an omen of good augury and do not despair of yet seeing China, Japan, and New Zealand services acknowledged and rewarded. The Victoria Cross also, already fairly earned, should now be awarded to more than one naval surgeon.

COMMISSIONS IN THE INDIAN MEDICAL SERVICE.

The Military Secretary, India Office, encloses a List of the Candidates of Her Majesty's Indian Medical Service, who were successful at the Competitive Examination at Chelsea in August 1866, and who have undergone a Course of Instruction at the Army Medical School, together with the total number of marks obtained at the Examinations at Chelsea and Netley.—*Maximum*, 6900.

Names.	*Studied at*	*Marks.*
E. F. Brockman,	London,	5420
W. D. Stewart,	Edinburgh,	5028
O. C. Duff,	„	4420
W. G. May,	„	4172
H. D. Compigné,	„	4113
F. Metcalfe,	London,	4080
J. F. Sargent,	„	3962
P. F. Mullen,	Ireland,	3922
J. B. Gaffney,	„	3718
R. H. Curran,	„	3695
H. Hyde,	„	3505
C. J. M'Kenna,	Edinburgh,	3488
J. W. Strong,	„	3293
W. Nolan,	Dublin,	3236
J. J. Wood,	Edinburgh,	3158
W. Finden,	„	3048
A. M'M. Paterson,	„	3000
A. R. Cowell,	Ireland,	2798
M. Heffernan,	Edinburgh,	2670
J. Simpson,	Aberdeen,	2543
S. O. B. Banks,	Ireland,	2395

MIDWIFERY STATISTICS OF SWEDEN.

The statistics for 1862, the last which have been published, inform us that of the 134,060 mothers delivered of children during that year, 225 had to be delivered with the aid of instruments, being one instrumental delivery in every 595 mothers. Of these 225 instrumental deliveries, 208 were by the forceps alone, bringing into the world 210 children, of whom 139 were living, and 71 dead. In the remaining 17 instrumental deliveries, cutting instruments had to be used, which sacrificed the lives of all the children.

Of the 208 women delivered by forceps, 3 died; being one death for every 69·3 forceps cases. Of the 17 women delivered by cutting instruments, 3 also died; being one death in every 5·7 mothers so delivered.

Of the 208 women delivered by forceps, in 124 it was their first child. 20 of the women delivered by forceps were under 25 years of age; 54 were between 25 and 30 years; 101 were between 30 and 40 years; and 33 were above 40 years of age.

Of the 17 delivered by cutting instruments, in 6 cases it was their first child.

2 of the women were under 25 years of age; 4 were between 25 and 30 years; 9 were between 30 and 40 years; and 2 were above 40 years of age.

The women in Sweden are attended by midwives, who must undergo an examination as to qualification, and obtain a license before being allowed to practise. In 1862 there were 1639 licensed midwives in Sweden, of whom 118 were in the city of Stockholm; and during that year, 79 women received licenses to practise simple midwifery after examination; and of them, 54 were found competent to use instruments, and were licensed accordingly.

CORRESPONDENCE.

THE FRENCH INTERNATIONAL MEDICAL CONGRESS.

(*To the Editor of the Edinburgh Medical Journal.*)

PARIS, 8 *Mars* 1867.

MONSIEUR ET TRÈS-HONORÉ CONFRÈRE,—Un Congrès médical international sera ouvert à Paris le 16 Août 1867 sous les auspices de Son Excellence le Ministre de l'Instruction publique. Recherchant la meilleure marche à suivre pour assurer le succès de l'œuvre, le Comité central a reconnu qu'il faut avant tout demander aux médecins de la province et de l'étranger une participation active à l'organisation du Congrès, et que la voie des délégations est à la fois la plus simple et la plus sûre.

A la suite de cette déliberation, la Commission a dressé une liste de délégués qui ont pour mission de provoquer et de recueillir les adhésions, et de les faire parvenir au Secrétaire général soussigné; ce mandat n'impose d'ailleur aucune obligation personelle. J'ai l'honneur de vous informer, Monsieur et cher Confrère, que vous avez été choisi comme Correspondant délégué du Comité central. Nous espérons que vous voudrez bien répondre à notre appel, et nous aider de votre précieux concours dans l'accomplissement de cette œuvre scientifique. La liste des Délégués sera prochainement publiée, et dans le cas où vous ne pourriez accepter la mission que nous sommes heureux de vous confier, je vous prie de m'en instruire aussitôt.

Veuillez agréer, Monsieur et très-honoré confrère, l'éxpression de ma considération la plus distinguée.

Au nom du Comité, le Secrétaire général, JACCOUD.

N.B.—Aux termes de l'art. 3 des Statuts, le prix de la souscription pour les médecins français est de 20 francs; les médecins étrangers ne sont soumis à aucune contribution pécuniaire.

[The Editor of this Journal will be glad to receive the names of any gentlemen intending to be present at the International Medical Congress, and will forward them to Dr Jaccoud. An early application is desirable.]

THE PERMANGANATES IN DIPHTHERIA.

DUNDEE, 16*th March* 1867.

SIR,—I have to acknowledge the kindness of Dr Charles Bell in sending me a copy of his paper on Diphtheria, as published in the London Medical Review, in the commencement of 1861, and republished, in pamphlet form, in 1864.

I need only remark that, had I been aware of the fact that he had published his views on this subject, before my paper was sent you, which appeared in the February number of your Journal, I would have sought merely to corroborate his observations, which are, I believe, far from being as widely known as they deserve to be.—I am, Sir, yours, etc. W. WATSON CAMPBELL.

APPOINTMENTS.

DR GEORGE BALFOUR has been appointed Physician to the Royal Infirmary of Edinburgh in the room of the late Dr Scoresby-Jackson; and Dr T. GRAINGER STEWART has been appointed Physician to the Sick Children's Hospital.

Part First.

ORIGINAL COMMUNICATIONS.

ARTICLE I.—*French Lunacy Statistics.* By ARTHUR MITCHELL, A.M., M.D., F.R.S.E., etc., Deputy-Commissioner in Lunacy for Scotland.

THE French Minister of Agriculture, Commerce, and Public Works has recently issued a Yellow Book entitled—" Statistique des Asiles d'Aliénés de 1854 à 1860."

This voluminous document has been prepared with great care and skill, and the information it supplies is full and minute. Though it deals mainly with the period from 1854 to 1860, it uses, for purposes of comparison, the statistics of the period from 1842 to 1853, and in certain matters takes us as far back as 1835. It is clear, therefore, that it is a work of unusual interest and value.

The following paper is an effort to epitomize M. Béhic's Report. It is neither a review nor an analysis, but an epitome—an abridged account of the topics discussed in the Report, given in a way which, it is hoped, will make it useful.

The remarks made in interpretation of the figures, or as showing the conclusions to which they appear to lead, are understood generally to be very much like those of M. Béhic, but here and there throughout the paper other observations occur, which are intended to direct attention to points of exceptional interest, to show peculiarities in the mode of making the calculations, to indicate that the results may have bearings which are not manifest at first sight, or to attain some other end regarded as desirable.

No comparison is made between French statistics and those of this or other countries. This could not have been done in a short paper, unless attention had been confined to two or three points. It is thought that it will serve a more useful purpose to give such an epitome of the whole Report as an interested reader might prepare for himself.

The distinction between pauper and private patients is not kept up in the tables, and this is to be regretted as many useful lessons are taught by a comparison of the history of the two classes.

All vital statistics are difficult of interpretation,—the unseen influences in operation being so numerous,—but this difficulty points to nothing more than caution in drawing conclusions, and, more particularly, in drawing those conclusions which are to be received as guides of conduct.

With these prefatory and explanatory remarks I shall proceed to give a succinct account of the latest French Lunacy Statistics,—endeavouring to embody in it as much as possible of the information which M. Béhic gives us in his admirable Report.

I. *Number, Character, Distribution, Size, etc., of Asylums for the Insane in France.*

At the end of 1860 the whole number of establishments in France devoted to the care and treatment of the insane was 99, and of these 57 were public and 42 private.

Of the 57 public asylums, one (Charenton) belonged to the State, 37 belonged to the Departments, and 19 to the Hospices. The private asylums are divided into two classes:—(1.) Those kept for profit and devoted to the wealthy class of patients; and (2.) Those founded by religious bodies and maintained chiefly for the benefit of the poor. Of these last there are 17.

Both sexes are treated in 73 of the asylums, males alone in 10, and females alone in 16.

Of the 86 Departments into which France is divided, 60 are already provided and 26 are still unprovided with asylum accommodation. Only 34 of the Departments, however, have asylums of their own—it being permitted to Departments to contract for accommodation either with private asylums or with the asylums of other Departments.

At the end of 1860 accommodation existed for 31,550 patients in the whole number of asylums. The number of patients actually in them was 30,239. So that there were 1321 empty beds to meet an increase, which for the five years before had been 950 per annum.

The average population of each of the 99 French asylums on 1st January 1861 was 305,—the average for the departmental being 402, for those belonging to the hospices 369, and for private asylums 184. There is a great variety in size, however, and we find

16 with a population below 50;
11 „ „ from 50 to 100;
12 „ „ from 100 to 200;
19 „ „ from 200 to 300;
12 „ „ from 300 to 400;
29 „ „ above 400.

Seventy are thus below 400 in their population and 29 above it. Of the 29 the Salpêtrière had 1362 patients at the end of 1860, Maréville 1347, Clermont 1247, Bicêtre 903, etc. There were altogether 11 with more than 500, and these 11 contained 10,036 patients, or more than one-third of the whole.

Among the different classes of asylums the patients were distributed as follows on the 1st January 1861 :—

In asylums belonging to the state or departments,	15,470 or 51·1 per cent.
,, ,, ,, hospices, . .	7,026 or 23·2 ,,
In private asylums, ...	7,743 or 25·6 ,,

As regards the pauper insane, their mean proportion to the whole number of inmates in the different classes of asylums, for the six years 1856–61, was as follows :—

In the state asylum (Charenton), ...	24·8 per cent.
In departmental asylums, ...	86·6 ,,
In asylums belonging to the hospices, . . .	88·6 ,,
In private asylums, ...	63·0 ,,

Of the whole number of the pauper insane at the end of 1860, 3-10ths were placed in asylums situated beyond the Departments to which the patients belonged.

II. *The Insane who are in Asylums :—their Number, Increase, Sex, Curability, etc.*

(*a.*) Their number and its increase since 1835.

The rate at which the increase of the number of the insane in asylums has taken place will be apparent from the following statement, which gives the whole number of the insane so disposed of on the 1st of January 1835 and 1836, and on the 1st of January every five years thereafter :—

(1st January 1835, . . 10,539)

1st Jan. 1836, . . .	11,091	1st Jan. 1851, . . .	21,353
,, 1841, . . .	13,887	,, 1856, . . .	25,485
,, 1846, . . .	18,013	,, 1861, . . .	30,239

The total increase of patients in asylums during the 26 years is thus 19,700, or 187 per cent., and the average yearly increase is 750.

The rate of increase in each quinquennial period, if calculated on the number with which each period began, is a diminishing rate, but if calculated on the primitive or starting number it is an increasing one; in other words, the absolute yearly increase is greater during the later part of the period than during the earlier.

Calculations by the first method, showing a diminishing rate of increase, are as follows :—

Period of 1836–1841	give an annual increase of	5·04 per cent.
,, 1841–1846	,, ,,	5·94 ,,
,, 1846–1851	,, ,,	3·71 ,,
,, 1851–1856	,, ,,	3·87 ,,
,, 1856–1861	,, ,,	3·14 ,,

If the rate of increase, however, be calculated on the primitive or starting number, the results wear a different look, as will be seen from the following tabular statement :—

	Total Absolute Increase.	Yearly Absolute Increase.	Percentage of Increase on Starting Number.
Period 1836-1841,	2796	559	5·0
„ 1841-1846,	4126	825	7·4
„ 1846-1851,	3340[1]	668	6·0
„ 1851-1856,	4132	826	7·5
„ 1856-1861,	4754	950	8·6

There is still another way of looking at the rate of increase of asylum populations, that of calculating the yearly rate at successive periods of 5, 10, 15, 20, and 25 years, and this is exhibited below.

Period		
Period 1836 to 1841,	Annual Rate of Increase,	5·04 per cent.
„ 1836 to 1846,	„ „	6·24 „
„ 1836 to 1851,	„ „	6·16 „
„ 1836 to 1856,	„ „	6·48 „
„ 1836 to 1861,	„ „	6·68 „

We scarcely think the foregoing figures support M. Béhic's conclusion that the increase of asylum populations gives indications of an early arrestment.

(*d.*) Considered in relation to sex.

For the seven years, 1854-1860, the mean yearly proportion of the sexes was 51·9 women to 48·1 men, and the proportion varies little from year to year. It has been much the same, indeed, since 1842. Females are thus steadily in excess of males in the asylums of France (as of other countries); yet in the admissions the reverse holds good, 53 males being admitted for every 47 females. The greater mortality of males and their shorter average stay in asylums are adduced as facts accounting to some extent for the excess of females in the whole population.

When the *Idiots* are compared with the *Lunatics*,[2] it is found that the excess of females is entirely confined to the latter, and that among the former, viz., the idiotic, males are considerably in excess of females;—thus in 100 lunatics there are 40·76 males and 59·24 females, while in 100 idiots there are 51·22 males and 48·78 females. This is found to be the case in all countries.

(*b.*) Class of the insane of which the increase has chiefly consisted.

The insane in asylums are divided into three classes, Cretins, Idiots, and Lunatics.[2] During the last six years, 1856 to 1861,—the only ones for which we have a division into these classes,—the cretins have varied little and have not been numerous, averaging 42, or about 1 in 7000 patients. The idiots, however, constitute about 12 per cent. of the whole asylum population, and they have increased during the last six years by 32 per cent., while the increase of lunatics has been only 14 per cent. Of the total increase, however, lunatics form about 81 and idiots 19 per cent.

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[1] The diminished increase during this quinquennial period is chiefly confined to the year 1849.

[2] We shall use *Lunatics* as synonymous with *Les Fous*, that is, as embracing all classes of the insane, except Idiots and Cretins.

(*c*.) Considered as curable or incurable.

The *lunatic portion* of asylum populations give, on an average, (for the years 1856-1860) 18·59 per cent. curable to 81·31 per cent. incurable. The two sexes are nearly alike. Four-fifths of *lunatics* in asylums, therefore, consist of the incurable.

The *idiotic portion*, again, of asylum populations yield only 4 per 1000 who can be tabulated as susceptible of amelioration.

For 1860 the entire population of the asylums gave 15·68 curable, and 84·32 incurable; and this is about the average for other years.

III. *The Insane in Private Dwellings, their Number, Form of Mental Disease, Sex, etc.*

The whole number of the insane in private dwellings was estimated

By the census of	1851	at	24,433
"	1856	"	34,004
"	1861	"	53,160

We have here an increase of 117 per cent. in ten years. Instead, however, of regarding this as real, it will probably be more correct to look on it as due to increasing care in taking the census.

Like those in asylums, the insane at home are divided into two classes, thus—

	1856.		1861.
Lunatics, . .	11,714	...	15,264
Idiots, . .	22,290	...	37,896
Total,	34,004	...	53,160

We learn from this that of the whole number of *lunatics* known to be in France, 35·75 per cent. were out of asylums in 1861, while of the whole number of *idiots* in the country, 91·26 per cent. were so disposed of.

Of the insane at home, the census of 1861 showed the sex, according to the form of mental disease, thus—

	Lunatics.		Idiots.
Males, . .	7,220	...	21,636
Females, . .	8,044	...	16,260
Total,	15,264	...	37,896

This yields the familiar result, viz., that among those labouring under the acquired forms of insanity, there are more females than males, while, among the idiotic, there are more males than females.

It is worthy of remark, that in France no special *care* is exercised *by the state* over the insane who are out of asylums, great though their number is.

IV. *Proportion of the Insane to the whole Population of the Country.*

(*a*.) The proportion of *the insane in asylums* to the whole population of France was—

In 1836, . .	1 in 3024
And in 1861, . .	1 in 1214.

During these twenty-five years the increase of the population was 9·47 per cent., and the increase *of the insane in asylums* about 172 per cent.

(*b.*) The proportion of *the whole number of the insane* (those out of, as well as those in asylums) to the population, was—

In 1851, . . .	1 in 796
In 1856, . . .	1 in 598
And in 1861, . . .	1 in 444

(*c.*) Again, taking the insane as divided into *lunatics* and *idiots*, the two classes give the following proportions to the population—

	Lunatics.		Idiots.
In 1856, .	1 in 1031	...	1 in 1430
And in 1861, .	1 in 875	...	1 in 900

It is impossible to believe that this increase of the idiotic is in a strict sense real.

(*d.*) The census of 1861 distinguishes the sexes, as well as the form of disease, and gives these results—

Lunatics,.1 male for 915 males, and 1 female for 839 females in the population.
Idiots, ...1 male for 796 males, and 1 female for 1034 females in the population.

Here again we have a preponderance of females among *lunatics*, and of males among *idiots*.

V. *Total Number of Insane Persons known to be in France.*

The three last estimates of the total number of insane persons known to be in France are as follows :—

	1851.		1856.		1861.
(1.) In asylums, . . .	20,537	...	26,286	...	31,054
(2.) At home or in private dwellings,	24,433	...	34,004	...	53,167
Total,	44,970	...	60,290	...	84,214

As already stated, it is held to be probable that a great part of the increase in the number of those in private dwellings is due to a better taking of the census in the later of these years.

VI. *Movement of the Population of Asylums.*

(*a.*) Admissions and discharges—General results.

The total number of admissions into the asylums of France for seven years, from 1854 to 1860, was 68,992. Those who were discharged from or who died in the asylums amounted to 63,277. The admissions, therefore, were in excess of all discharges by 5715, or 815 per annum, so that increased asylum accommodation was needed every year for more than 800 patients. The average yearly increase of the population of the asylums for the last twenty-six years is 750.

The annual number of admissions has been an increasing one. The following statement, however, which refers to the four last years shows considerable steadiness :—

	Admissions.	Discharges.	Difference between Admissions & Discharges.	Pop. at end of year.
1857,	10,024	9301	723	27,028
1858,	10,314	9464	850	27,878
1859,	10,086	9203	883	28,761
1860,	10,785	9307	1478	30,239

The increase of the number of the insane in asylums appears thus to be due more to the excess of the admissions over discharges, than to an increase in the number of admissions; that is, it appears to be due to an accumulation of old cases, rather than to a growing production of insanity in the country.

(*b.*) The number of the insane annually treated in asylums.

This is arrived at by adding to the number in the asylums on the 1st day of each year the whole number of admissions during the year. The mean for seven years (1854-1860) thus estimated is 36,268.

It is of importance to know the nature of this estimate, as on it the French calculate the proportion of their cures, thus making their results, when compared with those of other countries, appear more unfavourable than they might be.

(*c.*) The mean annual population of asylums.

This is calculated in the following manner:—The number of days which each patient under treatment during the course of the year spends in the asylum are added together, and the sum is divided by 365. This gives a true mean population, and thus estimated, the mean number resident in asylums for the seven years, from 1854 to 1860, was 27,023. It is on this that the proportion of deaths is calculated.

In the course of making these calculations it comes out that the average stay of males in asylums is steadily shorter than that of females—the difference for *lunatics* being as 261 to 275 days.

VII. *Admissions.*

(*a.*) Their number and increase.

Ever since 1835 there has been a gradual increase in the number of admissions into asylums. Thus, in 1835 the admissions were 3947 against 10,785 in 1860, being an increase of 173 per cent. The rate of increase was not absolutely steady, though nearly so; thus, the admissions in 1840 were less than those in 1839, and the admissions both in 1848 and 1849 were less than those in 1847. It is deserving of remark, however, that during these years the increase of the whole population of asylums went on nearly as usual, showing the increase to be chiefly the result of an accumulation of old cases.

The rate of increase is described as diminishing. Thus the mean yearly admissions—

From 1840 to 1844 were 6441
„ 1845 to 1849 „ 7530
„ 1850 to 1854 „ 8975
„ 1855 to 1860 „ 9959

This gives a proportional increase per annum—

From the first to the second period, of 3·38 per cent.
From the second to the third period, of 3·83 „
From the third to the fourth period, of 2·00 „

It is very doubtful, however, as already explained, whether this satisfactorily shows that the rate of increase in the admissions is falling in such a way as will lead to an early arrest of the growth of the population of asylums.

The increase of the admissions is thought to be due, in great measure, to the diminished prejudice as to the incurability of insanity, to the low rate charged by asylums, to the increased facilities of transmission, to the greater ease with which patients may be visited by their friends, to the introduction of the humane treatment of the insane, and to greater rigour on the part of the municipal authorities in causing the removal of the insane to asylums. With reference to this last, it is remarked that it is not only the maniac and furious lunatic who is now removed, but the dement, the idiot, and the imbecile.

(*b*.) Readmission of cases of relapse, and transfers from one asylum to another, and readmissions after escape.

Transfers from one asylum to another, and readmissions after escape, etc., are included in the total admissions. This often proves a source of confusion and trouble in comparing the lunacy statistics of one country with another. The total number of admissions for the five years, 1856-1860, was 50,455, and the number of transferences from one asylum to another was 4219, which are included in the total number of admissions. By excluding them, it is clear that we should seriously affect many calculations founded on the admissions.

It would appear that of 100 patients admitted, 77, on the average, are admitted for the first time, and 11 after relapse. Among these last there are more males than females, in the proportion of 102 to 100.

Most relapses occur during the first year after recovery. Thereafter they become more and more rare. The following table, dealing with the admissions after relapse between 1856 and 1860, illustrates this point:—

Readmitted in the 1st month after recovery,	468
„ in the 2d and 3d months „	499
„ in the 4th, 5th, and 6th months after recovery,	619
„ in the 7th, 8th, 9th, 10th, 11th, and 12th months after recovery,	973
Total in 1st year,	2559
„ in 2d year after recovery,	874
„ in 3d year „	516
„ in 4th and 5th years after recovery,	619
„ in 6th, 7th, and 8th years „	431
„ in 9th, and above, „	508
Total,	5507

It appears from this that 46·4 per cent. of relapses occur during the first year after recovery, 15·8 per cent. during the second, 9·3 per cent. during the third, and 28·3 per cent. after longer periods.

(*c.*) Admissions considered in relation to sex.

Between 1842 and 1860 inclusive, 86,326 males, and 76,825 females were admitted, or 112 males to 100 females. The disproportion held good every year of the whole period, but it varied from 105 to 121 males for every 100 females. This excess of males in the admissions goes against the opinion that the female is more liable to insanity than the male.

If the idiotic are dealt with separately, the excess of males over females is found to be still greater, as might be expected.

Though more males than females are steadily received into the asylums, it will be remembered that the actual population of these establishments always consists of more females than males.

(*d.*) Admissions considered in relation to age.

The average age of all patients admitted between 1856 and 1860 was, for males, 39 years 10 months, and for females, 41 years 9 months.

The numbers of *lunatics* admitted at different ages, when compared with the whole population of the country at corresponding ages, give as follows:—

	Males.	Females.
From 15 to 20 years, ...	1 in 8579 ...	1 in 9387
20 to 30 „ ...	1 in 3631 ...	1 in 4415
30 to 40 „ ...	1 in 2424 ...	1 in 2980
40 to 50 „ ...	1 in 2684 ...	1 in 3346
50 to 60 „ ...	1 in 3667 ...	1 in 4004
60 to 70 „ ...	1 in 4609 ...	1 in 3866

These figures indicate that *lunacy* shows itself with comparative infrequence below the age of 20, and that it becomes more and more frequent from 20 to 40, having its maximum for both sexes between 30 and 40.

It appears further, from them, that at nearly all ages the males of the population furnish a larger proportion of admissions than the females.

(*e.*) Admissions considered in relation to marriage.

Lunatics admitted for the first time between 1856 and 1860, gave the following results:—

	Males.	Females.	Total.
Unmarried, ...	9545 ...	7624 ...	17,169
Married, ...	7731 ...	6671 ...	14,402
Widowed, ...	1327 ...	2718 ...	4,045
Condition unknown, . .	545 ...	298 ...	843

If we compare these with the numbers of the married, unmarried, and widowed in the general population, excluding all below 15 years, we have as follows:—

	Males.	Females.	Males and Females.
Unmarried,.	1 admission out of 2532	1 do. out of 2926	1 do. out of 2707
Married, ...	1 „ „ 4611	1 „ 5314	1 „ 4937
Widowed,...	1 „ „ 3329	1 „ 3191	1 „ 3236

The proportional number, therefore, of the unmarried received into asylums exceeds that of the married by nearly 50 per cent.

In weighing the value of this result it must be remembered that the unmarried, having fewer to care for them, are more likely to drift into asylums than the married, and also that it is possible that a latent and unexploded insanity may be a cause of celibacy, as well as celibacy a cause of insanity.

It is difficult at once to determine the true value of statistics like these, but it is certain that the collection of them will eventually lead to the discovery of many important truths, if they are cautiously and carefully brought together and handled in a philosophic spirit.

It would have helped to a safer conclusion as to the relation of marriage to the production of insanity, if we had also known the state, as regards marriage, of those of the insane who are not in asylums.

(*f.*) Admissions considered according to the months in which they occur.

Between 1856 and 1860 the admissions of *lunatics* were thus divided over the months of the year,—

Jan.,	7·38 per cent.	May,	9·04 per cent.	Sept.,	8·19 per cent.
Feb.,	7·44 ,,	June,	9·42 ,,	Oct.,	7·99 ,,
March,	8·46 ,,	July,	9·77 ,,	Nov.,	7·28 ,,
April,	8·92 ,,	Aug.,	8·77 ,,	Dec.,	7·34 ,,

July, therefore, gives the maximum and November the minimum, and the progression on each side is nearly equal and steady.

It is probable, that as the accumulation of facts goes on, we may be able to make more of this point than is presently possible.

(*g.*) Admissions considered in relation to the cause of the disease.

(1.) Heredity.—Of the 38,988 patients admitted between 1856 and 1860, no information was obtained regarding 8250. Deducting these, the remainder consisted of 28,621 *lunatics* and 2117 *idiots*.

Of the *lunatics*, 4056 had had either father or mother insane, or about 14·0 per cent.,—giving 13·5 per cent. for males, and 14·1 for females.

Of the idiots, hereditary predisposition was found in 260 or 13·0 per cent.,—giving 11·0 per cent. for males and 15·0 per cent. for females.

A more minute analysis is made for 1859 and 1860, but, here again, it has to be noted that the proportion of those about whom no information was obtained is excessively large—so large as seriously to affect the value of the conclusions to which the figures appear to lead.

They seem to show among other things that insanity is more readily transmitted from father to son and from mother to daughter, than from father to daughter or from mother to son. Thus the heredity established—

In 264 males was derived in . .	128 from the father, 110 from the mother, 26 from both parents,	And in 266 females was derived in	100 from the father. 130 from the mother. 36 from both parents.

(2.) Physical and moral causes.—Among *lunatics* physical causes greatly preponderate, yielding 60·7 per cent. as compared with 39·3 per cent. due to moral causes. This holds good in both sexes, but moral causes are proportionally more frequent among females.

The most prominent of the physical causes are senility, want, onanism, drunkenness, uterine disease, blows and falls, and epilepsy.

Among males, drunkenness is out of all sight the strongest cause, as it is in this country. It may be remarked here, that the recent introduction and use of spirit from Germany may possibly increase the injurious effects of alcohol among us. The German spirit, being distilled to a great extent from the potato (one of the solanaceæ), it has been thought that it may contain some principle analogous in its virtues to that of the belladonna, and some physicians in North Germany are inclined to think that potato spirit causes delirium tremens more readily than malt spirit.

Among females, uterine disease takes the first place among the physical causes of insanity.

The more important and prolific of the moral causes are domestic griefs and troubles, religious excitement, reverse of fortune, and disappointed ambition.

Of all the causes, physical and moral, alcohol is by far the richest,—and this in a country where the popular notion is that drunkenness is not common.

(*h.*) Admissions considered in relation to the duration of the disease at the time of admission.

Taking the figures from 1856 to 1860, we find the duration of the disease in the patients admitted to have been as follows:—

Less than 1 month in	17·4	per cent.
From 1 to 6 months in	26·9	„
„ 6 to 12 months in . . .	13·4	„
„ 1 to 2 years in . . .	11·1	„
„ 2 years and upwards in . .	14·0	„

So that in more than a fourth of those patients admitted, regarding whom information on this point was obtained (26,471 out of 36,459), the disease had lasted for more than a year.

(*i.*) Admissions considered in relation to the complications of insanity.

Paralysis and epilepsy are the most important of these, and they appear respectively as 8·07 and 3·77 per cent. of *lunatics* admitted, and 1·87 and 9·40 per cent. of *idiots* admitted for the first time in the five years from 1856 to 1860. Both among paralytics and epileptics the proportion of males is higher than that of females.

Goitre appears as 0·26 per cent. of *lunatics* and 0·86 per cent. of *idiots* admitted for the first time; 3636 idiots and 54 cretins gave 32 goitres.

VIII. *Cures, and Discharges from other Causes.*

(*a.*) Their whole number, etc.

The following table gives the actual number of discharges for the seven years from 1854 to 1860:—

	Discharged cured.		Discharged from other causes.		Total Discharges.		Prop. per cent. of the Patients treated.
Males, . .	10,099	...	8,768	...	18,867	...	15·08
Females, .	9,648	...	8,202	...	17,850	...	13·85
Totals,	19,747		16,970		36,717		14·46

It will be observed that these proportions are calculated on the numbers treated, that is—the number in the asylums on the first day of the year *plus* the admissions during the year.

The proportion of male exceeds that of female discharges, and this feature exists every year since 1842, and occurs alike among those discharged cured or not cured.

Of the patients treated, the average percentage annually discharged was 14·46, but this is sensibly less than it was in previous years, having been as high as 16·36 for the period between 1842 and 1853. It appears, indeed, on looking more minutely into the figures for the later periods, that the proportion of cures is steadily diminishing, and the proportion of discharges from other causes steadily increasing. The following table makes this apparent:—

	Percentage of whole number of Patients treated.		
	Discharged cured.		Discharged from other causes.
1854, . . .	8·79	...	5·37
1855, . . .	8·86	...	5·66
1856, . . .	7·83	...	6·05
1857, . . .	7·62	...	7·32
1858, . . .	7·37	...	7·83
1859, . . .	7·19	...	7·14
1860, . . .	7·00	...	7·13
Average, .	7·77		6·68

The nature of the "discharges from other causes than cure," is disclosed in the following statement, which refers to the period from 1856 to 1860.

	Lunatics.		Idiots.
Escapes, ...	574	...	19
Transfers from one asylum to another, .	4,238	...	447
Discharges improved, etc., . .	7,251	...	690
Total discharges from other causes than cure,	12,063		1156

(*b.*) Cures in relation to sex.

Excluding idiots, the mean annual proportion of *lunatics* discharged cured, between 1856 and 1860, was for both sexes 8·24 per cent. of those treated; but for males alone it was 8·69, and for females alone 7·81, or about 1-9th less for females than males.

The greater frequency among males of disease referable to alcohol accounts in some measure for this result.

(*c.*) Cures according to the duration of treatment.

The figures from 1856 to 1860 show well the gradual diminution of the chances of cure with the duration of treatment. Thus, of 100 cures (among *lunatics*),

37·66	take place after less than	3 months of treatment, or	more than	1-3d
24·10	„	3 to 6 months of treatment, or	. . about	1-4th
11·28	„	6 to 9 „	. . „	1-10th
7·65	„	9 to 12 „	. . „	1-13th

So that 4-5ths of all cures occur in the first year of treatment, and there is little difference in this respect between the sexes.

(*d.*) Cures according to the months in which they are completed.

Summer and autumn give the larger, and winter and spring the smaller number—in the proportion of 54·7 to 45·3 per cent. July shows the highest figure and February the lowest.

(*e.*) Cures according to age.

If the ages of the cured are compared with the ages of the patients admitted between 1856 and 1860, we have the following results:—

The number of cured
below the age of 15 years is 27·9 p. c. of the number admitted below that age.
between 20 and 30 years is 46·6 p. c. „ between those ages.
between 30 and 40 years is 47·6 p. c. „ „

After 40 there is a gradual and steady diminution Idiots are excluded from this calculation.

(*f.*) Cures in relation to the cause of the disease.

Idiots are excluded here also. Of 13,687 *lunatics* cured, between 1856 and 1860, the cause of the disease is only given in 9789 cases, and these show that, of 1000 patients cured, in 536 the disease was due to physical, and in 464 to moral causes. It will be remembered that only 393 per 1000 of the admissions were due to moral causes, so that, in this class, the cures were relatively greater than in the class where the insanity was due to physical causes.

About one-half of all the cures which occur in the period and which have the cause of the disease assigned, are included in the subjoined statement.

Disease due to	Drunkenness . . .	in 1738	of the cures.
„	Domestic griefs . . .	in 1171	„
„	Various bodily diseases .	in 761	„
„	Diseases peculiar to women	in 723	„
„	Religious excitement . .	in 460	„

An hereditary predisposition to insanity was known to exist in 1522 of the patients cured, or in 15 per cent. of the *lunatics* cured.

IX. *Deaths.*

(*a.*) Their number and proportion to the mean population.

Between 1854 and 1860, that is during seven years, 26,560 patients died in the French asylums. These consisted of 14,532 males, and 12,028 females.

When compared with the mean population of the asylums for the period (estimated in the way described), this gives a percentage of deaths annually of 14·03. For the period 1842 to 1853, the percentage was 13·75; so that the rate of mortality has of late somewhat increased, and it has already been shown that the rate of cures has diminished.

During the last two years, 1859 and 1860, however, the death-rate has fallen, being 13·18 and 12·57 per cent. of the mean population.

The figures for the years between 1856 and 1860 separate the deaths in the two classes of the insane, and give 9·52 per cent. for *idiots*, and 14·05 for *lunatics*,—that is, in an equal population, 147 *lunatics* die for every 100 *idiots*.

(*b*.) Deaths according to sex.

The whole number of deaths consisted of 14,532 males and 12,028 females. This gives 16·02 males and 12·21 females per cent. of the mean population for each sex,—that is, for equal numbers of patients, the deaths of males are to the deaths of females as 131 to 100. Nearly the same proportions appear in the figures for the period 1842 to 1853. A similar excess of males appears in the deaths in French prisons.

It has been already shown that there are more females than males in asylums, in spite of the fact that more males than females are admitted. To some extent this is accounted for by the other facts disclosed, viz., that more males than females leave the asylum *cured*, *improved*, and *dead*.

(*c*.) Deaths according to length of treatment.

On this point the figures clearly speak (1856 to 1860). More than one-quarter of all the deaths occur among patients (*lunatics*) who have not been more than three months under treatment. Twelve per cent. of the deaths occur within one month after admission, about seven within two, and about six within three. Deaths soon after admission are more frequent among the male than among the female patients, being as 27 to 23 for the first three months.

(*d*.) Deaths according to the season of the year in which they occur.

Deaths among the insane in asylums vary in frequency with the different months of the year; but, in this respect, the influence of weather is the same on the insane as it is on the general population. Most deaths occur in winter and spring, and fewest in summer and autumn. July shows the minimum, and January the maximum.

(*e*.) Deaths according to age.

If the number of patients of each age under treatment each year had been given, the ages of the patients at death would have been of more value than they can be without such information.

One thing we learn from the figures under this heading, viz., that *lunatics* and *idiots* may live to a ripe age. Thus, of 17,167 *lunatics* who died in the asylums between 1856 and 1860, 3976 were sexagenarians, or more than a fifth; while nearly 6 per cent. of the *idiots* who died were equally old.

The mean age at death for the two classes was as follows:—

	Lunatics.		Idiots.
Males, . .	46 years.	...	32½ years.
Females, . .	50¼ „	...	34½ „

In both classes females show the greater age. As regards the first class, it will be remembered that on admission the average age of males was 39 years 10 months, and of females 41 years 10 months, so that the average length of treatment of female *lunatics* dying in asylums is about two years more than that of male *lunatics*.

(*f.*) Deaths in relation to the cause of the insanity.

We have information on this point regarding 13,328 of the deaths which occurred between 1856 and 1860, and we find

Physical causes giving	8406	deaths among *lunatics* (idiots excluded).
Moral causes giving	3703	„ „
Total,	12,109	

It has been shown that of 1000 *lunatics* admitted, 607 have their disease attributed to physical causes; and it now appears that of 1000 such patients dying, 694 have their disease referred to the same class of causes—a result which could have been anticipated.

Of the 12,109 deaths in the foregoing statement, 1534 were cases of senile dementia, 1268 were cases of insanity after the excessive use of alcohol, 1187 were complicated with epilepsy, 1131 were cases of insanity following various bodily diseases, 807 were cases in which domestic griefs and 683 cases in which want had led to the lunacy. This accounts for 6-10ths of the whole.

In 12 per cent. of all the 13,328 deaths an hereditary predisposition to insanity was established.

(*g.*) Deaths—accidental and self-inflicted.

For the seven years, 1854–1860, 87 accidental deaths and 116 suicides occurred in the asylums of France, or, annually, 12 deaths from accident and 16 from suicide.

Both in deaths from accident and in those from suicide, males are much in excess of females; thus,—

	Accidental Deaths.		Suicides.
Males, . . .	57	..	71
Females, . . .	30	..	45
Totals,	87		116

X. *Cost of Patients, Cost of Cultivating Land, Gratuities given to Patients who Work, Number of Workers, etc., in Departmental and other Asylums.*

The average daily cost of patients in the Departmental asylums for the seven years, 1854 to 1860, was 1 fr. 32 c., and it varied in different years from 1 fr. 25 c. to 1 fr. 39 c.[1]

The total cost of the patients in these asylums has increased from nearly 5½ millions of francs in 1854 to nearly 7½ millions in 1860. The outlay is made up of three leading items; thus, for 1860—

Gratuities or recompenses to patients who work, .	95,037 fr.
Expense of cultivating the ground, . . .	156,938 fr.
Cost of maintenance, etc., ...	7,099,223 fr.
	7,351,198 fr.[2]

The first item merits consideration in this country. In the Departmental asylums of France alone it has increased from 56,170 fr. in 1856 to 95,037 fr. in 1860. In the first year each of the workers had an average gratuity of 6 f. 79 c., and in the last 9 f. 19 c. Usually the gratuity is divided into two parts, the smaller being given to the patient to be spent by him on his own pleasure, and the larger being reserved and accumulated as a hoard to be given to him on his discharge.

The proportion of the patients occupied to the whole number of patients treated in all asylums is more than one-half, being about 55 per cent. for males, and 62 per cent. for females.

In the Departmental asylums, looked at separately, the proportion of workers is higher than it is in the asylums as a whole, being for both sexes 63 per cent. as an average for the last six years. It is among the workers in these asylums that the 95,000 francs were distributed as remuneration in 1860.

At present we discuss percentages of the occupied, but as occupation becomes more and more the condition of the whole population of asylums, we may eventually speak rather of percentages of the unoccupied. As occupation becomes more general, however, there are reasons for thinking that the dietary of the patients must be improved, if the mortality is to remain as low as it is at present.

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[1] In 1860, the daily cost of patients chargeable to the Departments, wherever placed, varied:—

In departmental asylums, from . .	0 f. 75 c. to 1 f. 50 c.
In asylums belonging to the hospices, from	0 f. 77 c. to 1 f. 50 c.
In private asylums, from	0 f. 70 c. to 1 f. 15 c.

The highest charge was at the Hospice de Bicêtre, and the lowest at the private asylum of Clermont-Ferrand.

[2] The whole cost of lunacy in 1860 was about nine millions of francs.

Among the receipts of the Departmental asylums, figures one—the profit derived from the work of the patients. We are not told how this is estimated, or in what it consists, but it has risen from 247,582 fr. in 1856 to 414,039 fr. in 1860. During the same period, too, the profits from cultivation of land have been nearly doubled. These facts indicate progress in a direction which is very desirable.

XI. *Maintenance of the Insane Poor—its Sources, etc.*

About four-fifths of the insane in the asylums of France are paupers, in the sense of being supported by the public, and are either at the charge of the State, the Departments, the Communes, or Charitable Establishments.

On the 1st of January 1861, the whole number of the insane in asylums was 30,239, of whom 6071 were private and 24,168 pauper patients. Of the paupers, 411 were at the charge of the State, 155 at the sole charge of Communes, 71 at the charge of Charitable Establishments, and 23,531 at the charge of the Departments.

By the Law of the 30th of June 1838, which is that now in force, the Departments are specially charged with the maintenance of the insane poor, and the cost of doing this appears in the first section of their budget. But, by a combination analogous to that which secures relief to foundlings, the law imposes on the Communes and Hospices an obligation to share the cost with the Departments,—the pauperism of insanity being to some extent dealt with as a thing *sui generis*.

For 1860, in most of the Departments the proportion which the Communes were required to furnish towards the maintenance of their insane poor varied from 33 to 10 per cent., according to the wealth of the Communes.

In order to prevent the facility of throwing on the Departments the support of a large number of the ordinary poor, *under pretext of insanity*, some Departments impose on the Communes a higher contingent for the maintenance of *inoffensive* than for the maintenance of *dangerous* lunatics—in the richer Communes *one-half* of the cost of supporting the *inoffensive class* being defrayed by the authorities more strictly local. The contingent rarely exceeds one-half, but in the Seine-Inférieure it rises to 70 per cent. on the Communes, and, for special reasons, it is above 50 per cent. in four or five other Departments. These facts deserve the consideration of the framers of the Metropolitan Poor-Law Act.

ARTICLE II. — *On the Production of Inverted Uterus.* By JAMES MATTHEWS DUNCAN, M.D., F.R.S.E., Lecturer on Midwifery, etc.

(*Read to the Obstetrical Society, 27th March* 1867.)

MY principal object in this paper is to throw light on the subject of the mode of production of inversion of the uterus, and especially to establish the theory of its causation in cases which more than others deserve the name of intussusception, in which the uterus is inverted, not merely spontaneously, as the phrase is, but inverted by its own active efforts.

In pursuing this object, I shall not describe the phenomena of any individual cases of this rare disorder. I have met with instances of the accident, and may, at a more fitting time, record them. They would only encumber me at present, because no individual case has other than very limited bearings on the extensive subject to be discussed. Although no individual case is of much value, yet the whole knowledge which we have of the affection is founded on individual observations, and it is this knowledge which forms the basis of all the reasonings to be now detailed.

In the outset, and to give some previous idea of what is to follow, I here state that much illumination of the subject has arisen to myself from regarding retention of the placenta by uterine spasm or stricture, generally called in this country hour-glass contraction of the uterus, as the affection most closely allied to inversion of the uterus produced by uterine action. I have shown all my views to many successive classes of obstetric pupils, and I now desire to give them a wider publicity. Holding that hour-glass contraction is a disease nearly identical in its first stage with the first stage of spontaneous active uterine inversion, I shall, I hope, be able to make these two conditions throw light, one upon another, so as to establish the true theory of both.

I shall not enter into historical details regarding this subject. Many authors, British and foreign, have made advances towards what I regard as the true theory of uterine inversion. Paralysis, or non-contraction, or weakened action of parts of the uterus, with contraction or spasm of other parts, has been, by many authors, pointed out as contributing to an understanding of the phenomena of encysted placenta, or hour-glass contraction. And there we have the germ of the theory I now propose.

Hegar, in his work[1] on retention of the placenta, makes the following statements:—"According to Seiler, the cause of the incarceration is the activity of the uterus in those parts to which the placenta is not attached, along with inaction of the seat of its insertion. . . . According to Desormeaux, the enclosure of the placenta arises from this, that the part to which it adheres does

[1] Die Pathologie und Therapie der Placentaretention, etc., Berlin, 1862, s. 30.

not make progress in its contractions equal to that of the other parts." This extract justifies what I have said in the former paragraph. I know no author who has, to an equal extent, advanced the subject of the causation of active uterine inversion.

But, although no author has, so far as I know, founded a theory of active inversion on paralysis of the uterus at or about the site of the placental insertion, yet, in recent times, there has been an increasing amount of support given by authors to the opinion that inversion is frequently produced spontaneously by morbid uterine action. Formerly, the chief place in the causation of the disease was given to mechanical interference and generally injudicious or ill-executed interference by the accoucheur. This change, whether well-founded or not, is a step towards what I regard as the true theory of inversion. Yet I shall, subsequently, show that, so far as I know, it has led only to erroneous speculations as to the nature of the phenomena.

I may here say that I am inclined to think that the above-named change in opinion, has probably gone too far, as I believe that pulling the cord or otherwise removing the placenta, and other mechanical interferences, whether well or ill done, may, in the condition of general uterine relaxation, often be the only assignable cause of inversion; and, in the condition of irregular uterine action, be a part of the cause, leading to depression of the placental insertion into the uterine cavity, changing the case from one of uterine spasm with encystment of the placenta or hour-glass contraction, into one of commencing inversion, producing thus the first stage of complete inversion. To this view as to the change of opinion some facts lend help, such as the frequency of the affection in unskilled hands, and its rarity in such institutions as the Lying-in Hospital of Dublin.[1] But I am not prepared to demonstrate what is my impression, and it forms no part of my present object to do so. I shall only, farther, quote a passage from Denman to show that he believed mechanical interference to be sometimes the cause of a beginning of inversion which was subsequently completed spontaneously or by uterine action, an opinion in which I concur:—"I have (he says) been assured that in some cases there has been a spontaneous inversion; that the accident happened at least when no force, or none capable of producing the effect, had been used; and then it was imputed to the shortness of the funis, giving the disposition before the birth of the child; or to some untoward action of the uterus. But with this assurance, or explanation, I do not feel quite satisfied, because degrees of force must always be vaguely estimated; though if a disposition to an inversion be first given by the force used in pulling by the funis, it may be completed by the action of the uterus; or, if the least possible degree of inversion were given by the shortened funis, it might certainly be completed by a very slight additional force in pulling by the funis, or by the mere weight of the placenta."[1]

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[1] "No example (say Messrs Hardy and M'Clintock in their work on Midwifery and Puerperal Diseases, p. 223) of acute inversio uteri has ever fallen under our notice, and the accumulated experience of Drs Clarke, Labatt, Collins, Kennedy, and Johnson, in the hospital, does not furnish a single instance of the occurrence of this accident, though the number of women delivered during their united masterships amounts to upwards of seventy-one thousand."

Among recent authors known to me, Drs Radford and Tyler Smith appear to me to have arrived most nearly to the true theory of spontaneous active uterine inversion. Both collate the affection with irregular uterine action; Tyler Smith distinctly compares it to hour-glass contraction; and both speak of irregularity of uterine action as comprising a weakened action in parts of the uterus and strong action in others, or, in Tyler Smith's words, both spasm and inertia. Doing so, had they at the same time kept in mind the view of Seiler and Desormeaux as to the condition of the uterus in cases of incarcerated placenta, a view which they themselves apply in some cases of such retention, they would, no doubt, have arrived at the true theory of spontaneous active uterine inversion.

Radford[2] believes that action of the inverted part with want of contraction of parts below it is the theory of active uterine inversion. "There are," says Radford, "several varieties of irregular uterine contraction, and in all, some part of the organ must be in a state of weakened action, whilst another is in a highly excited condition. It is not essential to inquire here which of these two states lays the foundation of the mischief. The uterus is divided, in hour-glass contraction, into two compartments; in the upper the placenta is uniformly found. A contraction sometimes takes place in the body and cervix, leaving a chamber at the fundus, in which also the placenta is always found. There, again, occurs a contraction in the os uteri, with deficient action in the body and fundus. This condition leads to a retention of the placenta, and in some instances it takes place after the placenta has been removed, giving rise to internal flooding. There are states of the uterus where some portion or portions contract with greater force than the rest. Now, it appears to the writer that inversion is another instance of irregular contraction, in which the fundus acts powerfully, whilst the cervix and os uteri are in a state of atony. It is obvious that if the fundus and body continued their action after the expulsion of the child, before the cervix and os uteri have regained their proper powers, an inversion must take place."[3]

To comment on this opinion of Radford fully would lead to largely anticipating what is better deferred to another part of this paper. I shall, therefore, now only say, that I cannot conceive on what grounds Radford can support his view. For the powerful action of the fundus, whilst the cervix and os uteri are in a state of atony, is a natural, ordinary, healthy concourse of conditions. They appear to me to be the conditions which best secure the female against the dreaded accident; and I cannot imagine the production of it while they last. If Radford attaches special virtue to the word "powerful" in his assertion, it would only lead, so far as I can see, to clearer perception of the impossibility of inversion while the conditions last.

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[1] Introduction to the Practice of Midwifery. Waller's edition, p. 421.

[2] Saxtorph is said by West to be the originator of Radford's theory. See West's Diseases of Women, 3d edition, p. 225. But the statement of Desormeaux and P. Dubois (Dict. de Médecine, tome xxx. p. 354) seems to me to transfer the credit to Astruc.

[3] Essays on various subjects connected with Midwifery: On Inversion of the Uterus: p. 16.

The only kind of uterine inversion consistent with Radford's conditions of powerful contraction of the fundus, and relaxation below that part, is inversion of the lower part of the uterus alone. It is not rarely observed after delivery. I have depicted it diagrammatically in fig. 1. It is, in ordinary circumstances, quite trivial, demanding no treatment, because it soon spontaneously disappears. It is produced under the presence of Radford's conditions for real uterine inversion, or inversion of the fundus, when there is bearing down or any other pressure on the fundus, pushing the uterus downwards through the brim of the pelvis.

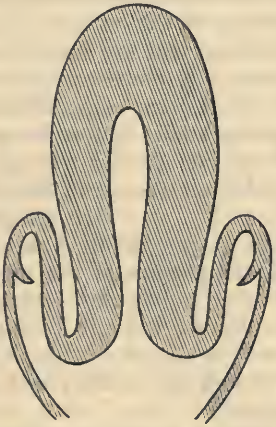

FIG. 1.—Diagram of Inversion of Lower Part of Uterus.

Tyler Smith believes that, in the production of inversion, action of the fundus carries or pushes this part into the uterine cavity, where the introcedent part is seized by an hour-glass contraction, and propelled through the lower part of the uterus, then in a state of inertia, but soon to be in a state of active contraction. This theory differs but inconsiderably from that of Radford, as Dr Smith's own words will show. Nevertheless, Dr Smith's account of the production of a spontaneous active uterine inversion is the best which I have anywhere met with. "I have always," he says, "been of opinion that, in the great majority of cases, the accident happens in consequence of irregular but active contractions of the uterus itself. No doubt, cases may occur in which inversion is produced by great tension upon the cord, while the placenta is firmly adherent to the fundus, and that all violent traction of the cord while the placenta is attached to the fundus is reprehensible. But when inversion is thus produced, there must be a consenting action of the uterus." . . . "All the facts connected with this catastrophe show that it generally depends, not upon a passive, but an active, condition of the organ." . . . "It has even been known to occur in the unimpregnated uterus." . . . "The nulliparous organ has been known to invert itself, as the result of spasmodic action, in long-continued menorrhagia, or as the result of a small polypus or fibrous tumour in the cavity of the uterus, or upon its peritoneal surface." . . . "There is, first, cuplike depression of the fundus uteri; coincident with, or immediately following upon, this depression, there is hour-glass contraction of the body or lower portion of the uterus. The annular contraction of the body of the uterus grasps the introcedent fundus as it would a foreign body, and carries it downward for expulsion through the os uteri, the os itself being at this time either in a state of inertia, or actively dilated, just as at the end of the second stage of labour. After the inverted uterus has passed through the dilated os uteri, this part of the organ becomes contracted, preventing re-inversion from taking place." . . . . "In simple hour-glass contraction, the cavity of the uterus is divided into two parts by the contraction of the middle portion of the organ; but when, owing to irregular action of the fundus, this part of the organ descends into the cavity of the uterus, and the hour-glass contraction then occurs, the fundus uteri is seized by the contracting ring of the uterus, borne down through the os uteri and vagina, and inversion is thus rendered complete."[1]

Before advancing farther, it will be advantageous, with a view to making my own views more clear, to point out some errors in Tyler Smith's explanations. And, first, I draw attention to his assertion that the unimpregnated uterus, not having a polypus or fibrous tumour in its interior, has been known spontaneously to invert itself. I have no doubt Tyler Smith knows of some recorded case or cases of the kind to which he can appeal in proof of his statement, but he has not given any reference to them.[2] In the meantime I shall only say that I cannot believe that such an inversion ever occurred. The unimpregnated uterus is so thick and hard, and its cavity so small as to render inversion necessarily slow in its progress, even when a polypus attached to its fundus renders the occurrence of inversion quite intelligible. The polypus, in such a case, is a uterine content, which the uterine walls can seize and expel from the uterine cavity, even though it drags the fundus uteri after it. This slow inversion is an operation the study of which might have suggested the recently demonstrated curability of the most chronic inversions. For, if the hard and small-cavitied unimpregnated uterus can be inverted, it is reasonable to expect that the similar but displaced parts of a chronic inversion can be replaced by means like to those which produced the displacement. The power which Dr Tyler Smith and others have shown that we possess of replacing a chronic inversion is only not a very great and renowned discovery because of the rarity of the affection to be cured. Similar great results are achieved in various departments of surgery and midwifery by like means, namely, the substitution of gentle and continued effort for more powerful and less continued effort.

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[1] Manual of Obstetrics, pp. 480 and 489.

[2] On this point see some apposite remarks by Dr West, "Diseases of Women," third edition, p. 223. See also Colombat, "Maladies des Femmes," tome i. p. 321.

But although spontaneous active inversion of the unimpregnated uterus containing a polypus is easily understood, and although it may be admitted that spontaneous passive inversion of the healthy unimpregnated uterus is conceivable; yet, to me at least, spontaneous active inversion of the unimpregnated uterus, having no organic disease, is quite inconceivable. For it is necessary to suppose, that muscular action of its walls could push some part of them into the uterine cavity, should bend some part inwards, which is impossible. Muscular action of a bulging part may make the line of its fibres straight, may destroy the outward bulging curvature of that part of the uterine wall, but can not go further and produce a bulging inward. As a bulging inwards or the first step to inversion cannot be attained, spontaneous active inversion is impossible. Holding such views, I cannot accept even the evidence of cases unless of the most unimpeachable character in every respect.

For the same reasons as are resorted to in last paragraph, spontaneous active uterine inversion after delivery is impossible, if we adopt Tyler Smith's theory; for no action, regular or irregular, can produce introcession of a part of the uterine wall. Before active uterine inversion can be begun, such introcession must take place. Some part of the uterus must be in a position to be seized by the remainder. Some other condition than uterine action must therefore be called in.

Four kinds of uterine inversion occur after delivery:—

1. Spontaneous passive uterine inversion.
2. Artificial passive uterine inversion.
3. Spontaneous active uterine inversion.
4. Artificial active uterine inversion.

The only uterine condition essential to the production of all these kinds is paralysis or inertia, or complete inaction. This is the condition of the whole organ at the time of production of the two first kinds. In the two last kinds it is accompanied by uterine activity, and as these cannot co-exist in the same part, the paralysis is partial, and the activity partial. Action of the uterine wall cannot cause introcession of it. Activity of the whole of the uterus renders inversion impossible. Activity of a part of the uterus renders introcession of that part impossible. There must, therefore, be paralysis of the whole or of a part, before inversion can be begun.

Supposing paralysis, then, how is introcession, or the first stage of inversion, produced? In cases of artificial inversion, cases of the second and fourth kind, foreign force is applied either from above to push the paralyzed uterine wall into the uterine cavity, or from below to pull it into the cavity. Spontaneous uterine inversion admits of no foreign power being called in, and we must find the desiderated power in the uterus or in the mechanical conditions of the abdomen. We have already shown that the uterus cannot effect it, and must fall back upon the mechanical conditions of the abdomen. Conditions fitted to produce the result are to be found in the ordinary bearing-down effort, or in absence of the retentive power of the cavity,[1] however produced.

In cases of active uterine inversion, cases of the third and fourth kinds, it is necessary that the activity or muscular contraction of the uterus be in a part situated below the paralyzed part. For as inversion consists in causing the descent of the fundus of the uterus, this cannot be effected by any uterine efforts, except those made by parts below the fundus.

Paralysis or inertia of the whole uterus is a well-known condition. If this condition is required to explain any kind of inversion, it is at least not necessary to enter upon proof of its occurrence.

Paralysis of a part of the uterus is also well-known, for every practitioner must often have felt the lower part of the uterus in this state (already described, p. 985), while the upper parts were firmly contracted. But, for our purpose, it is necessary to show that paralysis or complete inertia of the higher parts of the uterus may occur while the lower parts are in a state of active contraction. Of this occurrence, hour-glass contraction is the most frequent exemplification.

Much discussion has taken place as to what is and what is not real hour-glass contraction. By some the term is applied to contraction of the mass of circular fibres at or near the situation of the internal os of the cervix, while others apply the term to such contractions, higher in the uterine walls, as encyst the placenta in the region adjoining the internal orifice of a Fallopian tube. Most British authors use the term without implying any definite locality of the contraction, provided it is not at the os uteri. My experience leads me to join Hélie,[2] and many other authorities, in asserting the occasional occurrence of the contraction in either of the situations already named.

Hour-glass contraction cannot exist unless the parts above the contraction are in a state of inertia, unless there is a local paralysis of the uterus. This paralysis must be perfect, or nearly so, if the hour-glass contraction persists, for were the highest parts of the uterus even in moderate action the hour-glass contraction would soon be overcome.

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[1] See Edinburgh Medical Journal, December 1865.

[2] Recherches sur la Disposition des Fibres musculaires de l'Uterus, etc. Paris, 1864, p. 58, etc. etc.

Local paralysis of the higher parts of the uterus generally includes within the affected area that part to which the placenta is attached, or affects that part alone. Just as the placenta is generally found attached to the inverted uterus, so is the placenta almost invariably found in the sac above an hour-glass contraction, and the inactivity of the placental insertion is in both cases frequently shown by the persistency of the attachment. In my own experience I have met with exceptions to these conditions, but they are universally admitted to form the rule.[1] As an example of an exception, I may mention that I have met with a persistent hour-glass contraction of only a narrow uterine band running across the insertion of a morbidly adherent placenta. I was called to this case by Dr Linton.[2]

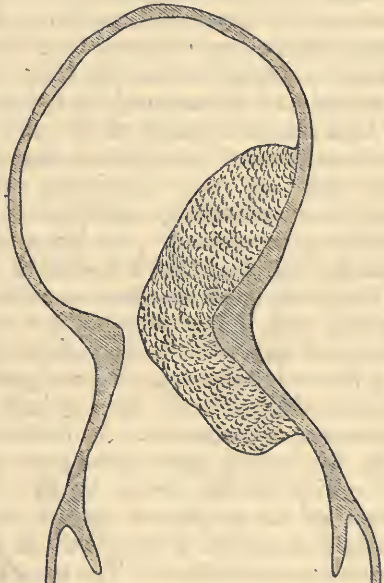

FIG. 2.—Diagram of the Hour-glass Contraction in the Case recorded.

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[1] Radford (see the quotation, p. 984) and others erroneously make the rule invariable.

[2] This interesting case is in exact opposition to the generally true statement of Hegar (p. 39),—"Bei totaler Adhäsion ist der Kuchen vollständig in der Höhle enthalten." For the placenta was everywhere morbidly adherent.

In cases of paralysis of the placental insertion bleeding will naturally be expected to occur if the placenta be partially or wholly separated. But, in the case of hour-glass contraction, the bleeding may be prevented by the contraction; and, as Tyler Smith has said, the same prevention of hæmorrhage may arise, in a case of inversion, from "the os uteri acting as a tourniquet to the uterus," and it is more than probable that this condition soon arrives even in cases of passive inversion.

In their descriptions of and reasonings upon cases of hour-glass contraction, of inversion and of hæmorrhage, many authors have erred in taking a hard part of the uterus for a part necessarily in a state of action. Now, while hardness of uterine wall is in most cases a good and sufficient sign of muscular action in the hardened part, it is not invariably so. For, in cases of hour-glass contraction and of inversion, the hardness may arise from passive tension. The bladder of urine and ovarian cysts frequently illustrate hardness of flaccid bags produced by passive distention. In like manner, a paralyzed portion of uterus may be hard from being tightly replete with placenta or other contents retained by stricture; and the fundus of an inverted uterus may be hardened by the circumstances of its inversion, apart from muscular action of the hard part.

In the medical literature of post-partum hæmorrhage, especially of long-continued draining, much interesting evidence of the not very rare occurrence of persisting paralysis of the placental insertion is gradually accumulating.[1] In his work on pathological anatomy, Rokitansky describes it as a disease of the puerperal state as follows:—"We must (says he) here mention a very singular circumstance, which may, on account of the consequent danger, become important, and may even be misunderstood in post-mortem examinations; it is paralysis of the placental portion of the uterus, occurring at the same time that the surrounding parts go through the ordinary processes of reduction. It induces a very peculiar appearance. The part which gave attachment to the placenta is forced into the cavity of the uterus by the contraction of the surrounding tissue so as to project in the shape of a conical tumour, and a slight indentation is noticed at the corresponding point of the external uterine surface. The close resemblance of the paralyzed segment of the uterus to a fibrous polypus, may easily induce a mistake in the diagnosis, and nothing but a minute examination of the tissue can solve the question. The affection always causes hæmorrhage, which lasts for several weeks after childbirth, and proves fatal by the consequent exhaustion. We have met with it twice, once after abortion, and once after parturition at the full period." . . "Dr Betschler, during his visit to Vienna in 1840, communicated a similar case to me as having occurred at Breslau; and there can be little doubt that Dr Burckhardt (*vide* Berliner Centralzeitung, x. 19) speaks of this condition under the title of acute fungus hematodes uteri, as of a new and hitherto unknown cause of flooding after childbirth."[1]

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[1] In the works of an author so old as Mauriceau we find a case of this kind described by him, and based on a post-mortem examination. I quote from Desormeaux and P. Dubois (Dict. de Médecine, tome xxx. p. 349),—"Il y a simple dépression, quand le fond de la matrice est un peu déprimé en dedans, comme est le cul d'une fiole de verre, ainsi que le dit Mauriceau, qui a constaté ce déplacement sur le cadavre."

Referring to Rokitansky's description, we find Kiwisch saying,—"Atony of the uterus is either general, or it affects only a part of this organ; and in the latter condition, the partial paralysis of the placental portion, with sinking inwards of it, appears as a peculiar, and, at the same time, very dangerous condition. We have as yet had opportunity to observe it in only a single case."[2]

Klob also gives an account of the affection—"Defective contraction (says he) of that part of the uterine wall which forms the placental insertion, is of extraordinary importance. In such cases, this part of the uterus, having the shape of the uvula, or of a body of a conical or round form, with a neck-like constriction, sinks inwards into the distended uterine cavity, while the other parts of the organ seem tolerably well contracted. At the corresponding part of the external surface of the uterus can be seen a funnel-shaped depression, or at least a more or less considerable inversion of the wall. The inverted tumour-like part filling up the uterine cavity is spongy, bleeding, shows the peculiarities of the placental insertion, and in most cases there are on it some remains of the placenta, or there adhere to it coagula of blood in pretty large lumps."

"Rokitansky points out, that in the origination of such depressions of the paralyzed placental insertion, the tuggings, on artificially separating the placenta, might have influence; and I would have this specially attended to on this account, that I particularly cannot understand wherefore the paralyzed portion of the uterine wall always sinks inwards into the uterus, while at the same time, in consequence of the energetic contraction of the other parts of the walls of the uterus, its cavity is reduced to small dimensions. I take it to be absurd to propose to regard the pressure of the abdominal viscera as the cause of the sinking inwards of the paralyzed part. Absolute or relative shortness of the umbilical cord may, in such cases, come into prominence as a frequent cause of the pulling."

"The consequences of this condition are hæmorrhages, which may at last lead on to a fatal termination."

"The frequency of this occurrence in abortion is striking, and Engel's first case concerns a woman, 34 years of age, who had already gone through seventeen labours successfully, and in her eighteenth pregnancy, suffered an abortion in the fourth month. Engel points out the large number of the pregnancies, and their quick succession, as the cause of the paralysis of the placental insertion, and mentions that in the dead body (and how much more in the living) the swelling formed by the projection inwards of this paralyzed part might easily be confounded with something else, as a polypus decomposing at its lower end."[1]

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[1] Sydenham Translation, vol. ii. p. 304.

[2] Klinische Vorträge. IVte Auflage, i. bd. s. 427.

I shall now, in conclusion, give a brief account of the four kinds of uterine inversion that may occur.

Spontaneous passive[2] inversion occurs in cases of paralysis, or inertia of the whole uterus; the organ being large, its walls lax, and capable of being inverted by little force. Bearing down produces, in general, collapse and compression of the organ; but it may produce inversion if the depressing force is applied under favourable circumstances, and the inversion will be complete if the bearing down is strong and continued. Should the original condition of inertia persist, the neck not contracting around the inverted organ, then replacement will be at least as easily performed as inversion. It is to this category that I am disposed to refer the cases of inversion post-mortem which Bœrner and Klaatsch have recorded.

Artificial passive uterine inversion demands little description. It is the kind of inversion commonly described by the older authors. It differs from the spontaneous passive inversion only in this, that foreign force replaces the bearing down. The foreign force may be applied from above, by pushing, or from below, by pulling the cord, or manœuvring with the placenta. It would be a more frequent occurrence than it is, were it not the case that the interference which tends to produce it also tends to bring on that general uterine action which prevents it. It is in all respects similar to the former kind. In both kinds of passive inversion hæmorrhage will probably occur, if the placenta is separated, and the conditions of their production persist.

Spontaneous active uterine inversion is the kind which modern authorship is bringing more and more into notice as the most common kind. I have already said that I am disposed to think this tendency is being pushed too far. In this kind, paralysis of the fundus, or of a portion of it, probably of the placental portion, occurs. The state of the retentive power of the abdomen, or positive bearing down, leads to this portion projecting into the uterine cavity. It is seized by the adjacent contracting segments of the uterus, is pushed down and expelled through the os uteri into the vagina, or beyond the vagina. It is difficult of replacement, in consequence of the contraction of the uterus around the inverted parts.

Artificial active uterine inversion differs in nothing from the kind last described, except in this, that the inversion of the paralyzed portion is effected by pressure from above, or by pulling on the cord, or other interference from below.

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[1] Pathologische Anatomie der weiblichen Sexualorgane, s. 264.

[2] A very good practical account of passive inversion is given by Desormeaux and P. Dubois, at p. 350 of the article already referred to. See also Colombat, Traité complet des Maladies des Femmes, tome i. p. 316.

It can scarcely be considered out of place to add, in conclusion, a few remarks on spontaneous replacement of an inverted uterus. It is almost useless to say that spontaneous replacement of the first stage of an inversion often occurs, and is accounted for by contraction of the inverted part, or by alteration of the condition of the retentive power of the abdomen changing an inversion into an elevation of the paralyzed portion of uterus. But spontaneous replacements of completely inverted uteri have been reported by Dailliez [1] and others. The possibility of this spontaneous replacement has also been denied. But I see no reason to doubt it. An inverted or opposite condition of the mechanism which produces spontaneous passive inversion may also produce spontaneous passive replacement, whether at an early or at a late period. Spontaneous active uterine replacement is inconceivable, because the necessary conditions, being, described generally, the same as those for spontaneous active inversion, cannot be supposed to exist.

ARTICLE III.—*Abstract of the Medical Report, for the Year ending* 31*st December* 1866, *of the General Prison for Scotland.* By J. BRUCE THOMSON, F.R.C.S. Edin., Resident Surgeon.

THE last Annual Medical Report of the General Prison for Scotland contains several statements that appear to me not only valuable to the public, but to the medical profession; and I am therefore induced to offer a short abstract of the Report for insertion in the *Medical Journal.*

I. *Of the General Health, Sickness, and Death-rate.*

The general health has been good during the last year, and uniformly so for ten or twelve years past, since the introduction of the improved dietaries,—previous to which period, diseases of debility, scurvy, scrofula, and other diseases from defective dietaries did prevail. The illnesses of the prisoners are generally the result of hereditary taints and habitual vices, and sometimes of frequent and long imprisonments.

Epidemic diseases, even when prevailing in the immediate locality, seldom enter within the prison walls. We were therefore startled with the visitation of Asiatic cholera during last year, which proved fatal in two cases—the only two we had, and which must prove very interesting to the profession. These cases, occurring to prisoners, without any possible external communication, and without any known internal causes—indeed independent of any of the admitted or alleged causes exciting or predisposing to Asiatic cholera, deserve particular study.

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[1] Dict. de Médecine, tome xxx. p. 359.

The particulars of these cases I give very briefly:—

D. C., aged 37, was admitted from the Perth County Prison, on the 30th July 1858, insane. This prisoner enjoyed uninterrupted good physical health until he was seized, on 10th July last, with Asiatic cholera in its most severe form. The symptoms were—copious vomiting, and purging of rice-water appearances; spasms in legs and arms; blueness of lips, fingers, and toes; absence of urine; great and rapid prostration. Mr Christie, surgeon, acting for the Resident Surgeon, who was absent on leave, consulted with one of the Perth physicians, and they decided that the case was one of Asiatic cholera. The patient was removed to a room isolated from all the departments of the prison, and placed under a nurse, who was restricted from communication with other prisoners. Active treatment was adopted: to keep up the heat of the body, turpentine applications were applied to the stomach; bottles of hot-water in blankets wrung out of hot-water, on which turpentine was sprinkled, surrounding the body; sedatives; astringents; calomel and opium, with stimulants of brandy and iced-water, were diligently used, but with no avail. In twenty-three hours from the period of attack, the patient sunk into collapse, and died.

The following precautionary orders were immediately issued—that all the body-clothes and bedding of the deceased be immediately burned; that the corpse be interred with the least possible delay; that the room in which D. C. took ill, and that in which he died, be disinfected by chlorine water and chloride of lime as soon as the body is removed; that all the officers be instructed to keep a strict watch on all prisoners going to the water-closets, and at once report every case of disturbance of the stomach and bowels; that all sewers, water-closets, and sinks, be flushed daily with fresh water. Chloride of lime to be freely used.

Professor Christison made a special visit, with the view of determining whether any local cause could be found to explain the occurrence of this case of cholera, and in his report to the Managers, of date 12th July last, from which I take leave to quote, the Professor says—"No cause has hitherto been discovered relative to food, drink, drainage, water-closets, ventilation, etc., which will account for this remarkable incident, even as a predisposing cause."

I ought to mention that no premonitory diarrhœa had appeared, neither has there been any subsequent to this case worthy of notice, as will appear from my daily notices sent to the Managers by special instructions. Indeed, there were fewer complaints of the stomach and bowels than have prevailed generally, especially in the autumn season; and in the lunatic department, where this fatal case of

Asiatic cholera took place, there was no diarrhœa, until the next case of Asiatic cholera, three months and eighteen days after the one just narrated.

W. M'D., aged 48, was admitted from the Prison of Dunfermline on the 3d October 1865. He enjoyed good bodily health until 28th October last, when he was attacked by Asiatic cholera. This case deserves especial attention, because of its appearing in the same dormitory of the Lunatic Department in which the former patient was attacked, who died of cholera. In this dormitory were other ten prisoners, not one of whom has had even diarrhœa. Without recapitulating the symptoms of the case of W. M'D., suffice it to say that they were precisely those noted in the former case, and much the same treatment was followed. All the precautionary measures of isolating the patient were adopted. Within forty-eight hours from the period of attack, the patient collapsed and died.

At the time of the former case no Asiatic cholera prevailed in the district; but when the latter case occurred the city of Perth was suffering from this epidemic.

All the precautionary means recommended above to prevent the spread of this disease were rigidly enforced after the first case, and are still actively employed, and no diarrhœa has followed.

The ventilation and general sanitary arrangements of the Prison are satisfactory.

The reduced scale of dietaries, certified and approved by the Secretary of State on 1st May 1866, works well, so far as our short experience determines.

The following table shows the kinds of diseases and the amount of sickness and deaths which belonged to prisoners during the decennial period, 1857 to 1866 inclusive:—

Class of Diseases.	Cases of Sickness.	Deaths.
1. Zymotic,	205	6
2. Constitutional and 3. Tubercular,	99	29
4. Of nervous system,	59	18
5. Of circulation,	17	10
6. Of respiration,	93	10
7. Of digestion,	104	7
8. Of urinary and generative organs,	51	5
9. Of uterine organs,	35	
10. Of locomotive organs,	36	—
11. Of skin,	142	—
12. Accidents, etc.,	12	—
Unknown,	—	1
Old age,	2	2
Worn out,	—	9
	815	97

The average daily population for the ten years, 1857 to 1866, was 6604, or, per annum, 660, showing a death-rate of 1·46. A slight increase of late years has taken place since penal servitude was substituted for transportation. This prison has been established for twenty-five years; and the daily average population added up makes 12,299, while the sum total of the deaths has been 178, being at the average rate, per annum, of 1·45 per cent., or about 14 per 1000.

The foregoing statistics of the low rate of sickness and mortality are very remarkable when we keep in mind the degraded and deteriorated class of people who form the prisoners there. They are all the long-sentenced prisoners of Scotland, confined from nine months to life, and who are born and bred in crime.

II.—*Of the Mental Condition of Prisoners.*

The Report for 1866 gives us facts and figures, establishing what is now generally admitted, that prisoners exhibit a large amount of mental disease. The causes are said to be chiefly congenital, aggravated by depraved habits of life and early neglect. It is difficult to estimate what share imprisonments, long and frequent, may have in producing or aggravating their mental weakness.

The daily average prison population in 1866 amounted to 719; out of these there were—

Associated as unfit to bear separation on account of epilepsy, weakmindedness, etc., ...	48	
Under special observation, separate, but weakminded,	51	
Total, . .	99	
Prison daily average population, ...	719	
Weakminded, etc., ...	99	$= \frac{1}{7}$

giving 1 out of every 7 weakminded, and requiring special attention. Out of these, 7 became insane in the course of 1866, and were transferred to the lunatic department, a branch prison for the cure and treatment of criminal lunatics.

The following table shows the admissions to the Lunatic Department during the last ten years:—

	1857	1858	1859	1860	1861	1862	1863	1864	1865	1866	Totals.
From the General Prison,	4	4	6	6	4	2	3	3	9	7	48
From other Prisons, .	5	6	8	2	1	3	1	5	6	1	38
	9	10	14	8	5	5	4	8	15	8	86

The number of prisoners who have become insane in the general prison during the last ten years being 48, and the average daily prison population for ten years amounting to 6604, we have the following equation, viz., $\frac{6604}{48} = \frac{1}{137}$, or 1 out of every 137 prisoners becoming insane during imprisonment here in the course of the last ten years.

A table added to the Report gives the particulars of all confined in the Lunatic Department for criminals during 1866, amounting to 54. Under the term "criminal lunatics" are comprehended the following classes of prisoners, viz.:—1. Prisoners found insane in bar of trial; 2. Prisoners tried and found insane at date of crime; 3. Prisoners who have become insane while undergoing their sentences in prisons. Several of these criminal lunatics who have committed heinous offences have been reported to be sane and have been so for many years, but the grave nature of their offences and their antecedent history has led to the conclusion that they cannot be certified as safe persons to be at large. Their mental maladies, which are quiescent under asylum treatment, would be likely to recur under the trials of social life.

ARTICLE IV.—*On the Reduction of Dislocations of the Hip-Joint by Manipulation.* By THOMAS ANNANDALE, F.R.S.E.; Lecturer on Surgery; Assistant-Surgeon, Royal Infirmary.

ALTHOUGH the method of reducing recent dislocations of the hip-joint by simple manipulation is described by Hippocrates, Wiseman, and other early writers, and has in later years been successfully employed by several surgeons, its adoption in this country is as yet by no means general.[1] This method is so simple and successful in the majority of cases—so much more easy in its accomplishment than the plan in which the pulleys are used, and is so likely to be valuable to those of my brethren who cannot always have beside them efficient assistants and appliances, that I have thought it right to add my testimony in its favour by recording the following cases:—

In August 1866, I was asked by Drs Otto and Macgregor of Pathhead to see with them a patient who had sustained a dislocation of the hip-joint from a heavy piece of stone falling upon him. The patient, a strong muscular man, about 48 years of age, had all the signs of a dislocation of the head of the left femur, upwards upon the dorsum ilii, well marked; and I accordingly proceeded, with the aid of my colleagues in the case, to perform reduction in the following way:—

The patient was placed on a mattress laid on the floor, and then put completely under the influence of chloroform until the muscular system was thoroughly relaxed. The pelvis being steadied, I seized the left ankle with my right hand, at the same time grasping the corresponding knee with my left hand, and flexed the leg to its complete extent upon the thigh, and the thigh upon the abdomen. With the limb in this position, the knee and foot were made to take a circular sweep outwards as far as they would go, and then the whole limb was suddenly extended. This movement caused the head of the bone to slip into the acetabulum at once with a distinct jerk.

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[1] See Hamilton on "Fractures and Dislocations," 2d edition, page 616. Also an interesting paper and series of cases by Mr Nunneley, in British Medical Journal for October 20, 1866.

In some of the dislocations of the hip-joint the direction of the circular sweep should be made inwards, and a little rotation of the limb often assists the head of the bone into its place.

Occasionally a recent dislocation of the hip-joint can be reduced by simple extension and manipulation, without the employment of the pulleys. The next case was treated successfully in this way :—

A miner, æt. 48, having sustained a dislocation of the hip-joint backwards into the ischiatic notch, was admitted into the Clinical Surgical wards of the Royal Infirmary in March 1866.

The patient having been placed on a mattress, was put fully under the influence of chloroform, and then by slight extension and manipulation of the limb, Mr Syme readily returned the head of the bone into the acetabulum.

If the head of the bone should have become fixed in a recent or old-standing dislocation, and cannot be moved by flexion and manipulation, it will, I think, be found useful to make some extension in the proper direction, either with or without the pulleys, until the head of the bone is dislodged, and then, suddenly removing the extending force, to adopt the flexion and manipulation method.

In this way I lately easily reduced the following case of dislocation of the hip-joint upwards on to the pubes.

A man, æt. 62, was admitted into the Clinical Surgical wards of the Royal Infirmary on the 2d of March 1867, with a dislocation of the hip-joint on to the pubes, of three days' standing. Having placed the patient on a mattress, and put him completely under the influence of chloroform, the flexion method was tried, but the head of the bone would not move from its abnormal situation. Extension of the limb in a direction outwards was then made by means of the pulleys, and as soon as the head of the bone could be felt to leave the pubes, the extension was suddenly withdrawn, and at the same moment the flexion plan was adopted, the foot and knee being also rotated inwards. As soon as this was done, the head of the bone slipped into the acetabulum.

As an example of what may be done by manipulation in reducing a dislocation of another joint, let me relate a case which was sent to me a few weeks ago.

A big strong man, æt. 50, came from the country, in February last, with a dislocation backwards of the first phalanx of his left thumb. Careful and repeated attempts had been made to reduce the dislocation in the country, but they had proved unsuccessful. Having chloroformed the patient, I first tried extreme flexion, and afterwards extreme extension (in a direction backwards) of the thumb, but without succeeding in reducing the bone. It was then my intention to have divided subcutaneously the agents preventing reduction; but before doing so, I grasped the thumb, slightly flexed it, and after a few gentle rotations and manipulations, I was rejoiced to find that reduction had been accomplished, and therefore my operation was not required.

ARTICLE V.—*On the Physiological Actions of the Ordeal Bean of Calabar, and on its Antagonism to Tetanus and Strychnia-poisoning.*

By EBEN WATSON, M.A., M.D., Lecturer on Physiology in Anderson's University, and Surgeon to the Royal Infirmary, Glasgow.

I BELIEVE that I need make no apology for prefacing the account of my investigation of the physiological actions of the Calabar bean with the following notes written by the Rev. W. C. Thomson, who was lately a missionary in Calabar, and is at present studying medicine in Glasgow, where he became acquainted with my employment of the bean in the treatment of tetanus, and I with his knowledge of its administration in trial by the ordeal. I was thus led to request him to commit to paper his recollections of what he had observed in Calabar, and these are now published here, with his consent, because they form an important contribution to the subject of my paper. I am not aware that any such detailed or accurate account of the symptoms presented by persons under the Calabar ordeal has previously been communicated to the profession.

"The Calabar ordeal bean, *Physostigma venenosum* of Professor Balfour, comes to us, as its name intimates, from Calabar, a name by which Europeans designate a small but influential tribe of negroes inhabiting the left bank of the Old Calabar river, which flows into the Bight of Biafra, west coast of Africa, immediately to the north of the island of Fernando Po. By the natives of Calabar the bean is called *esere*,—has long been known to them, and has long been used by them as an ordeal, for the purpose of detecting and extirpating the superstitious crime of witchcraft.

"The plant from which the beans are obtained is a *climber*, attaining often to very considerable dimensions, and frequently found growing on the banks of the river, at the water's edge, where it climbs the overhanging trees, almost entirely masking their foliage with its own rich festoons. About the month of August it makes an abundant display of pretty pink and white papilionaceous flowers, of which, however, comparatively few come to maturity. The fruit, which is ripe in November, is a pod not unlike that of our own horsebeans, containing rarely three, commonly one or two beans. On the 29th October 1862, rather too early in the season perhaps, I gathered the fruit of two considerable plants, one of them having a stem an inch and a half or two inches in diameter, and at its lower part towards the root so twisted upon itself as to simulate a cable of three or four inches in diameter. From the first of these plants 230 pods were procured, representing about 170 clusters of flowers, and yielding exactly 375 beans. From the other plant the number of pods obtained was 261, representing at the same average about 190 clusters or racemes of flowers and yielding exactly 430 beans. These beans when gathered were still full of moisture, nearly as large again as the dry bean, of a dark grey colour—not the dark chocolate brown which they ultimately acquire—and they averaged in weight 54 beans to the pound.

"The natives of Calabar obtain their supply from the shores of the river, where the beans are cast up in sufficient quantity to serve their purpose. In former days every free man of the least consequence in his village kept a stock in hand, so as to be able to contribute his quota as occasions of public trial by the ordeal might require; or to test the loyalty of suspected wives or slaves of his own; or even in case of his being threatened with a public trial, to put himself quietly to the proof, with proper remedies at hand, so as to obtain a prognosis that might encourage him to face the ordeal with hopes of an honourable acquittal, or else warn him in time to flee from certain and ignominious death. And yet it is a curious fact, that hardly any of these people know anything at all of the plant; indeed one may safely venture to say, that none of them are able to identify the plant in new situations, except by the aid of the fruit.

"According to Dr Fraser, of the Edinburgh University, who has written on the subject, the dry bark of the plant is quite harmless, though the sap which exudes from it when a fresh part is wounded is first astringent and then acrid to the taste. The leaves also are quite harmless. Goats devour them greedily without any deleterious effects ensuing.

"In publicly administering the Calabar bean as an ordeal, it is commonly given both by the mouth and in the form of a clyster. The suspected person has some entire beans handed to him, which he is compelled to eat up in this condition. Meanwhile others are bruised in a mortar or on a slab, and being well mixed with water, one part is taken as a draught and the rest administered as an enema. If after vomiting and purging, when the ordeal has been given in both forms, the person recovers, he is declared to be innocent and harmless, if not he is pronounced guilty, and the community congratulates itself on having thus avenged past and prevented future mischief.

"It would be very gratifying indeed to be able to say, though it were only now after the presence of missionaries amongst them for some twenty years, that the negroes of Calabar have quite abandoned this stupid and most fatal custom,—a custom by which formerly hundreds of lives were sacrificed annually, and by which many still occasionally perish. A few such cases have come under our own observation, and of these I proceed to give such particulars as I can clearly recall to mind.

"CASE 1.—This was the case of two men and a woman who were subjected to the ordeal out of a company of twenty-one persons accused. The trial took place during the night, and was conducted by lamplight in the public square. The two men had been dosed ere we reached the spot, and were sitting on the ground quite motionless with their legs gathered in their arms, and their faces resting on their knees. The woman was in the act of receiving the draught portion of the ordeal, which was no sooner swallowed than it was rejected by the stomach. The quantity of the mixture taken could not have been less than a quart, and might contain the substance of four or five beans at least. After this, immediately after she had vomited, she was removed in charge of her female friends to receive the remaining portion as an injection, while the men were taken into the town hall or palaver house for the same purpose. There was no opportunity in this case of observing effects, but next morning it was found that one of the men had died, and that the remaining man and woman had so far recovered as to be able to go the round of many friends to receive the congratulations and presents customary on such occasions. Both were living and in vigorous health two years afterwards.

"CASE 2.—This was the case of a young woman,—a mere girl indeed, twelve or fourteen years of age perhaps,—who had devoured a bean for the purpose of self-destruction. At the time our attention was called to her she was sufficiently sickened to be very penitent, but not sufficiently so to feel quite safe. She could not vomit, much as she desired to do so. A dose of the sulphate of zinc, however, speedily cleared her stomach, and she was soon quite herself again. In this case the circumstances were such, that the poison must have lain more than twenty minutes on the girl's stomach before the emetic took effect.

"CASE 3.—This also was that of a female—a young woman of about twenty years of age. She had quarrelled with a neighbour,—had been called a witch,—and took it so much to heart that she straightway went off to the woods and there quietly put her character to the test by eating up half a bean. This being discovered she was brought to the public square of the village to be put through the ordeal before the headman in regular form. Meantime we were sent for, and reached the spot before the beans in process of preparation for her were ready. But the half-bean she had taken was already begining to affect her. There was very obvious twitching of the muscles of the back, pretty profuse perspiration, and she repeatedly and urgently begged for water. Yet she resolutely for several minutes refused to receive a solution of tartar emetic from a lady missionary who was pressing it on her acceptance. At length, however, her sufferings appeared to persuade her; she received the emetic, threw up the poison, and the case being considered spoiled, further proceedings were abandoned. She was quite well next morning. In this case the poison—half a bean—must have lain from twenty minutes to half an hour on the stomach before the emetic was taken.

"CASE 4.—Here again the unhappy person is a woman, thirty-five or forty years of age. Having lost her son, she accused a sister-in-law of having caused the young man's death, and demanded that she should be tried by the usual ordeal of the bean. In such a case, when one individual thus challenges another, if the accuser be willing to partake of the ordeal along with the other, it is customary for the authorities to compel the accused to yield. It was so in this case. The bereft mother ate up several whole beans, quite deliberately, taking an occasional sip of water to facilitate deglutition. When next seen—little more than half an hour afterwards, perhaps—she was perfectly helpless. She was sitting on the ground supported by a female friend, who sat on a low stool behind her. She seemed to be perfectly flaccid. Leaning against her friend, her drooping head had to be supported, while she rested, hung by the axilla on her friend's thighs, over which her arms hung lifelessly as over the arms of a chair. Her back was much bent backwards, and her legs thrust out flat on the ground before her. At this time she was still able to speak, and obstinately refused everything in the shape of an emetic, observing that if she were such a one as the bean should destroy, it were best for all that she died. She complained of intense nausea; there was a constant flow of mucus; occasionally an attempt to vomit, when small quantities of mucus were thrown up, which friends anxiously but vainly searched in the hope of finding the larger particles of the bean, the appearance of which is, in their estimation, a hopeful sign of innocence and of life. In a few hours she was dead. The other unfortunate woman, whom, however, I did not see, also died.

"CASE 5.—This also was the case of a female, of about the same age as the preceding. Finding herself named along with certain others in connexion with a great feast of witches, she indignantly determined to purge her character, and demonstrate her innocence. Her own brother gave her a bean, or perhaps more than one, which no dissuasion on the part of others could prevent her eating. I reasoned with this woman for more than ten minutes, vainly endeavouring to get her to receive an emetic from me, and she must have taken the poison ten minutes or a quarter of an hour before I saw her. Indeed her friends were already becoming anxious about her, and even joined with me in urging her to use means to clear her stomach. This she stoutly refused to do, and became quite angry with us for proposing such a thing. All this while the poison seemed only to render her a little uneasy. By-and-by, as we still urged her to receive an emetic, she started up and went off in a fury, calling myself and all who sided with me all the hardest and worst names her vocabulary could supply. This was early in the forenoon. In the afternoon I saw her quite lively and comfortable, and in excellent humour. She had vomited after I saw her in the forenoon, and had so escaped.

"CASE 6.—This case happened at the same time as the last, and was that of a strong man of middle age. Having snatched up and swallowed a handful of bruised beans that had been prepared for others, he ran about wildly for sometime, pursued by his friends, who at length captured him, and brought him back to his house. I saw him just as he was brought back,—it might be about a quarter of an hour from his taking the poison,—perhaps less than that. He was greatly agitated,—was evidently afraid that it was going to be a bad case with him, and he perspired profusely, which his so recent violent exercise might sufficiently account for. He would not hear of an emetic being prepared for him, and even refused to receive water from his friends, who had urged him to accept an emetic, and desired him to drink with the view to encourage vomiting. An hour or more after I saw him it was reported that he had vomited. The same afternoon he visited his friends, danced before them as is customary, and received their congratulations. However, in the evening, as he was proceeding to his farm, three or four miles from the village, the vomiting returned, and he died in about a week thereafter.

"CASE 7.—This was the case of a little boy of about eight years of age. Some extravagant gestures that he had indulged in for his own amusement and the diversion of his little companions had appeared, in the estimation of the wise men of the village, to implicate both himself and his mistress in certain witch proceedings of a frightfully cannibal description. In consequence of this both he and his mistress were subjected to the ordeal. The latter I did not see after she had received the poison. By all accounts, however, it was rapidly fatal in her case. As respects the little boy, I was summoned on his behalf almost immediately on its being determined that he should have the ordeal; but before I could reach the spot he had been largely drugged. I found him sitting firmly on the ground, with his belly greatly distended, apparently from the large quantity of the mixture he had been compelled to swallow. My efforts to get possession of this little boy, or to have an emetic given to him, were all in vain for many minutes, nor was I allowed to remove him till his case was deemed beyond remedy. At this time he was lying apparently quite flaccid, overpowered by the poison. He was removed by two persons, one seizing him by the wrists, the other by the ankles, and, while being thus conveyed from the place, it was observed that his head hung back without any appearance of muscular effort to support it. By this time he had also lost command of the lower sphincter muscles, contents of the bladder and rectum were both escaping. He was still, however, able to answer to his name by a feeble, abrupt moan. I could not get him to swallow any part of the emetic we poured into his mouth. It was quickly poured back again as soon as given. Beyond this quiet extrusion of the emetic from his mouth, and the feeble answers he gave to his name, I cannot recollect that he made any voluntary movement from the time he came into my hands till his death. He quickly became, to all appearance, quite unconscious; there was much frothy mucus about his mouth and nose; his pulse grew feebler, and in two hours from the time I was summoned to his help he was dead.

"CASE 8.—This was the case of three persons,—an elderly woman, a young woman, pregnant, and a young man under trial for the third time. The elderly female, a strong masculine sort of woman, died. I saw her but for a moment or two, and she was then quite overpowered, and was sitting, supported by a female friend, exactly as described under Case 4, and in the very same flaccid state. The younger woman survived, but in her case the poison had the effect of causing abortion. As respects the young man, I saw him swallow what might be the substance of five or six beans, very coarsely bruised. To this quantity must be added what he received as a clyster. When I next saw him, on the following morning, he was quite well. Each of these persons had had the poison exhibited to them in both forms.

"In concluding these observations, I cannot help regretting that they are not more precise and particular,—the more especially as one can hardly wish to have such opportunities as the cases afforded repeated over again. But, such as they are, I trust they may be useful as, to their limited extent, a kind of corroborative evidence; and now that the Calabar bean has fallen into wise and good hands, I feel assured that it is on the way to become henceforth, instead of the curse it has been, a restorer of health and preserver of life."

In my experiments with the Calabar bean, I have used several preparations of it, but chiefly those of the kernel; because I believe that to be by far the most active, if not the only active part of the bean. I found this opinion on the following experiment. I had a very concentrated tincture made from three drachms of the spermoderm of the bean, and I injected this to the amount of one drachm of fluid into a cavity carefully prepared beneath the skin of a dog. He soon became sleepy, and in fact fell asleep on his legs. He did not fall, but nodded his head and closed his eyes; but he was easily startled, and ran away quite briskly. In about half an hour he had completely recovered from his drowsiness.

I concluded that this state was not the specific effect of the spermoderm. It did not agree with the results of Dr Fraser's more numerous experiments, and it might be accounted for by the very cessation of the excitement just previously undergone during the injection of the tincture, and perhaps also by the operation of the alcohol. I am quite willing, however, to consider this a doubtful experiment, as it is negative in its result, and was not repeated. I do not think that the question of the possibly different actions of the spermoderm and kernel of the Calabar bean is of any great importance; since the former is a very small quantity compared with the latter, and they are not likely to be separated by pharmaceutists in their preparations. Indeed I have discovered that they really are not separated by them, but that the alcoholic extract, for instance, is made from the powder of the whole bean, which, in my opinion, only serves to make it a little weaker, and therefore more manageable as a drug than otherwise it would be.

Of the kernel I have used the powder, the tincture made from it by percolation, the alcoholic extract in the semi-solid form and suspended in water and in sherry; and re-dissolved by boiling in alcohol. I have also used the watery extract, and have found it extremely active. It was prepared by slowly boiling seven pounded beans in four ounces of water for three-quarters of an hour. The result, after straining, was one and a half ounce of a starchy fluid, which was mixed with milk, and about a third of it was poured down a dog's throat. The usual characteristic symptoms of the poison were produced in less than a minute, and he had ceased to breathe in five minutes more. A similar extract is produced by boiling in acetic acid, but I have not used it. It is extremely bitter to the taste. All these facts point to the existence in the bean of an active principle which is soluble in alcohol, water, and I believe also in acetic acid at the boiling temperature, but which has not yet been separated. There is no doubt that this principle is a very subtle poison, but, as it exists in the bean, it is not so very virulent as many others with which we are already acquainted; as, for instance, woorara, which is in many respects similar to it. In some of the cases observed by Mr Thomson, large quantities of the crushed bean were taken and retained in the stomach for nearly half an hour, yet, when vomiting had been induced by sulphate of zinc, no bad effects followed. In those of his cases in which death occurred, several hours elapsed before the fatal event, and in all of them the most remarkable phenomenon was paralysis of voluntary motion, without pain or impairment of the mental faculties.

In my experiments on the lower animals quite similar effects were produced. All were paralyzed, and some recovered after a time from what seemed a very thorough influence of the bean. I subjoin a short account of these experiments.

Exp. I.—Performed, 21st December 1866, on a Guinea pig. At 4.35 P.M. I injected ♏ v. of the tincture of Calabar bean under the skin of the right flank by means of a hypodermic syringe. Almost immediately it was found to have lost the power of moving the right leg, and, soon afterwards, of the left also. It lay down on its belly and seemed averse to any exertion whatever. When its ear was pinched it immediately cried, but did not move. When I aimed a blow at its head with a cane, and struck the floor in its near vicinity, making a sharp noise, it started and winked, but did not attempt to move any part of its body to escape the threatened blow. It therefore saw and heard, and knew the danger of a blow, but could not or would not try to escape.

In a few minutes more the palsy had reached the anterior extremities. At 4·45, only ten minutes after the commencement of the symptoms, vomiting of frothy fluid like saliva occurred, and, at this time, by way of attempting an antidote, ♏ iii. of vinum belladonnæ were given and soon repeated; but they had no effect, and the animal ceased to breathe at 4.53.

I immediately opened the chest, and found the heart pulsating quite regularly, which it continued to do for some minutes afterwards; and when, some hours later, a more extended inspection was performed, it was found that the lungs were congested and airless, while the cavities of the right side of the heart were distended with dark partially coagulated blood, and those of the left were nearly empty.

Exp. II. 21st December 1866.—The subject of this experiment was a large and very strong cat, which struggled very much and was difficult to manage.

After two previous, and, as we thought, unsuccessful attempts to make it swallow doses of the bean, the third attempt was really successful; and at 4.15 P.M., one grain of the alcoholic extract was administered by the mouth (mixed with a little water). The animal's eyes soon became filmy and the pupils somewhat contracted. Bloody saliva poured from the mouth, and the tongue was protruded, hanging over the jaw. It could not stand, but lay on its side, having apparently lost the power of its four legs almost simultaneously. Soon after this it began to kick out convulsively, with its fore-legs chiefly, but it never seemed to attempt to rise or to run away. The respiration then became jerky and gasping, and the cat died somewhat suddenly about 4.30.

On inspection some time after death, the body was found quite rigid. The lungs were much congested, and, when incised, dark venous blood escaped from them. The left side of the heart was hard and firmly contracted; its cavities contained almost no blood, while the cavities on the right side were fully distended with dark and partly coagulated blood.

Exp. III.—Performed on 27th December 1866, on a small but active rabbit.

At 8.53 A.M., a dose of ♏ x. of the tincture of the bean was given by the mouth, and as it produced no effect, a second dose of other ♏ x. of the tincture was given at 9.52. Soon afterwards a very copious discharge of both urine and fæces took place. There was no other effect, and therefore, at 10.20, a third dose, consisting of a grain of the alcoholic extract mixed with water, was administered. In eight minutes thereafter, the rabbit sank upon its hind-legs in a sitting position, and apparently could not move. When held up by the ears it kicked out, but not vigorously, with the hind-legs. When set down again it trembled on its legs and quivered in all its muscles. The pupils were quite natural. When suddenly threatened, it did not move its body, though it showed by winking and moving its ears that it knew what was meant.

At 10.30, it lay down, but immediately struggled, and after a great effort got up and stood weakly, supporting itself against the wall. Then it fell down and lay on its side with its eyes half shut. Again it rose upon its legs merely to fall down in a different position, viz., on its belly, with its legs stretched out before and behind in a most unnatural way, only to be accounted for by their utter powerlessness. The respiration was very labouring, so that its nostrils were widely dilated for each inspiration.

At 10.38, the pupils were still natural, and the rabbit started when the candle was brought close to the eye. It can get up on its legs, but instantly falls again as if quite unable to stand. Its legs fold under it in any way and appear quite powerless, but it held its head curiously erect all this time.

At 10.43, it was breathing very badly and lying quite still. No threat could make it move, and physical motions were elicited feebly and with difficulty. It would not drink water. Pupils were quite natural. I thought the animal was dying at this time, it looked so very weak and breathed so ill. It continued in much the same state until 11.5, when it began to recover; first breathing more fully, and then exhibiting power over the muscles of the limbs; for, when touched, it could now get up on its legs, and very soon it ran nimbly about. But this was only on compulsion, and when permitted it stood still, leaning against the wall. By-and-by it became more natural, shook and licked its fore-paws, and by 11.13 it seemed quite recovered.

Exp. IV.—Performed on 29th December, on a small and active rabbit.

At 9.37 A.M., I gave it a pill, containing gr. ½ of the extract of the bean. At 9.55, it seemed to be a little frightened, but quite active. At 10.5, it ran about the room as if pursued, with great and unnatural activity. At 10.18, the same state continuing, I gave it a whole grain of the extract at once by the mouth.

At 10.24, it rocked itself to and fro from side to side, still standing on its legs, which seemed weak. When a sudden noise was made near it, it moved its ears and seemed startled, but did not move or try to escape. It soon fell on its belly, with its legs stretched out front and back. The pupils were rather small.

At 10.29, the only change was that it occasionally uttered cries like those of a peacock. It was very easily startled, and got up weakly on its legs, tottering for a few seconds and then falling down again. This was repeated several times till 11.10, when it seemed stronger. Then, for the second time, it became unnaturally active and lively, as if under some strong excitement, and by-and-by it became quite well. It was now put among some pet rabbits and fed, but it was found dead before night.

Exp. V.—Performed, 11th March 1867, on a middle-sized mongrel dog. This is the experiment referred to at page 1005.

At 9.45 A.M., I gave him the third part of the water which had been boiled on seven pounded beans. In one minute, he had lost the power of his hind-legs and fell down on his haunches. He trembled all over and panted in breathing, moved his fore-legs reluctantly, though he could by using them crawl along, dragging his hind-legs. He answered to his name and seemed quite sensible, but did not appear to suffer any pain. The pupils were about half contracted.

In about two minutes more, he kicked aimlessly with his fore-legs and then lost the power of them, falling down on his side. The respiratory movements were almost imperceptible. He passed urine and fæces and frothy saliva. Pupils the same as formerly. He soon afterwards lost the power of holding up his head, and finally ceased to breathe in six minutes after the administration of the poison.

The chest was immediately opened. The heart had ceased to pulsate. Its cavities were full to distention on both sides, the blood being of different colours. When this was allowed to escape by incisions, the wall of the heart began to contract, and continued to do so feebly but regularly for some minutes. The lungs contained very little blood, and were pale and crepitant.

In these experiments, as well as in Mr Thomson's cases, the two phenomena observed were, first, and most remarkably, the paralysis, and, second, a peculiar effect on the secretions.

I.—The paralysis has struck every one who has studied the actions of the Calabar bean as most extraordinary and unusual in many of its features. Let me mention the chief of them :—It is preceded by a more or less marked stage of twitchings or tremblings of the muscles of the whole body. Then it always commences in loss of the voluntary movements of the lower extremities (posterior in the inferior animals), and gradually extends upwards, first implicating the superior or anterior limbs, and then the chest and neck, when the respiratory movements cease, and the animal dies of asphyxia. In some cases, however, when the dose of the poison is strong, the paralysis affects the heart directly and thus causes death. When this paralysis is fully established, the physical or diastaltic movements can only be very feebly excited, and sometimes do not at all respond to their accustomed stimuli. Yet this paralysis seems to be intermittent in its power, for the animal sometimes rises out of the state of helplessness and moves for a short distance, again to fall down quite powerless; and this is repeated perhaps frequently till death happens. During the whole operation of the poison the sensibility of the body remains unimpaired, but no pain seems ever to be felt, and the mental faculties are quite unaffected.

Now, in investigating the physiology of this very extraordinary state in order to arrive at some knowledge of its proximate cause, it will be admitted without question that the poisonous agent enters the blood and then affects some of the nervous centres. The analogy of other poisons is enough to establish this point, but when we inquire more precisely into the kind of effect and the precise centre acted on, we meet with considerable difficulty. Professor Christison, in a private communication to me on this subject, has suggested four possibilities as to the cause of the paralysis, which are so exhaustive, and from the fact of their coming from him so worthy of consideration, that I shall state them in his words, and reason upon them from the facts before me.

1*st*, "*It may depend on reduction of the circulation and consequent great debility*;" that is, a general effect on the nervous system analogous to what we find in shock after a severe bodily injury. But it is quite evident that this is not the right explanation of the paralysis caused by the Calabar bean, for the paralysis always affects the posterior extremities first, and the rest of the body remains under the power of the will for sometime longer. The animal can hold his head erect, and walk with the fore-feet, dragging the posterior limbs behind. Besides, the arterial pulses may be felt beating strongly when the animal has lost all power to move; and even after death the heart is found to beat, sometimes vigorously for many minutes. These circumstances are obviously incompatible with such an amount of general debility as would account for the paralysis.

2*d*, "*It may be the result of coma, through action of the poison on the brain.*" But it is still more obvious that this is not the case of animals poisoned with Calabar bean; for nothing approaching to coma is ever noticed, except, of course, just when the animal is expiring. Then, certainly, as the circulation through the vessels of the brain is arrested, it ceases to perform its functions, but this is death, not coma. Indeed, in the case of one rabbit experimented on (Exp. IV.), the animal seemed to be even excited and to run about as if under the influence of a stimulant; and in the case of the woman (Case 5) observed in Calabar by Mr Thomson, the effect of the bean seemed to be to excite the cerebral functions. I suspect, however, that these were exceptional cases, and that in general the bean exercises no influence at all on the brain.

3*d*, "*It may depend on volition being suspended or impaired, though not necessarily, with the other phenomena of coma.*" This idea originated in Dr Christison's own feelings when under the influence of the poison. He then experienced great inability to move, but he found after several ineffectual trials that he really had the power still; that is to say, he could after a short time make the exertions in which he had failed just before. Now it is certain, that in order to a voluntary movement, the cerebral centre (corpus striatum) of the voluntary nerves must receive some sort of impression or impulse from the mind to be conveyed to the muscles; but if the mind be prevented from giving this impulse, no such movement can be produced, though the nervous apparatus is untouched by disease.

It seems to me, however, that the only mental state in which such communication between the mind and the nervous centre is necessary, is just the state of volition, and in Dr Christison's own case, the volition was strong, while the inability to move was nevertheless complete. Some three times he tried to raise himself on his elbow, but failed to do so. Surely this is proof of his will being strong enough,—so strong, indeed, that he did not cease to make an effort to obey it; and on the fourth attempt he succeeded, and did get up on his elbow to vomit.

Again, in Mr Thomson's cases, the poisoned persons showed strong will in obstinately refusing for a time the emetics offered by the missionaries; stating as their reason that the ordeal would then be unfulfilled and their characters still suspicious.

In my experiments on the lower animals, they could see and try to avoid a blow when threatened,—they winked, turned their ears to one side, and tried to run away, which, after several unsuccessful attempts, they often accomplished, tottering a few steps before they fell down unable for further exertion. They showed their will, however, in thus running as far as they could.

There seems, therefore, to be no failure of volition. The mind is clear and active in all its operations, and, what is very strange, it retains the power of willing to move the parts which are palsied. This is not usually the case in ordinary palsy of voluntary motion. The patient does not feel that he can even wish to move the affected limbs, but it is different in the palsy produced by Calabar bean. The person can wish to move his limbs which are lying apparently powerless, and though he fails to move them, yet he renews the attempt again and again, and often, by a strong effort of the will or under some powerful emotion as of fear, he overcomes the paralysis and succeeds in moving the limbs.

All this, I think, interestingly shows that the defect is in the nervous apparatus itself, for it is not in the mind, and it is not in the muscles, which I have stated already remain, even after death by poisoning with the bean, quite irritable on application of the ordinary stimuli.

4*th*, "*It may depend on complete annihilation or suspension of the action of the spinal cord.*" This I believe to be the true explanation of the phenomenon in question. *First*, Because none of the other explanations is applicable or satisfactory; and I believe that no other feasible explanation could be suggested. *Second*, Because the parts paralyzed by the influence of the bean are such as receive their nervous supply from the spinal marrow and its nerves. *Third*, Because this paralysis is never confined to one side, as one often sees, in cases where the affection springs from a local or a cerebral cause, but it is always a paraplegia which is produced by the bean; just as might be expected in the case of an agent affecting the whole of one segment of the spinal cord. *Lastly*, Because of the way in which the bean causes death. In almost all cases this is by asphyxia, brought on by paralysis of the external mechanism of respiration. This effect is produced more or less rapidly according to the dose of the bean administered, but it seems to be the universal mode of action of the poison that it affects the lowest segment of the cord first, then those above, and lastly, the part in the neck, soon after which the animal expires. If the chest be then opened at once, the heart will be found pulsating, but, of course, ineffectually. The circulation is arrested in the pulmonary capillaries by the stoppage of the respiration, and the enfeebled heart's force cannot overcome the vis inertiæ of the blood in them; hence we have the usual post-mortem appearances of asphyxia, viz., gorged right side of the heart and empty left side, while the lungs are rather more than usually bloodless.

There is no doubt, however, that when the dose of the bean is very powerful, the animal seems to die of paralysis of the heart, in which case the mechanism of respiration is paralyzed at the same time, and it is very difficult to tell which is the cause of death. In opening the body, however, in these cases, both sides of the heart will be found enormously distended with blood of different colours, the walls thin and flaccid, and no pulsation will be noted. But I have remarked, that after opening the cavities of the heart and relieving their over distention, the heart begins to pulsate feebly again, showing that it had been prevented from pulsating and emptying its left side by a paralysis of its walls occurring just previous to the systole, while its muscular fibres remain contractile. Now, this may be attributed to the sudden change in its nutrition and in its nervous supply caused by the complete cessation of the respiratory changes in the lungs, the effect of palsy of the muscles of respiration; or it may be ascribed to a direct influence of the bean on the cervical portion of the cord, and therefore of the sympathetic nerve, which is in close connexion with it in that region.

Another most remarkable phenomenon caused by the bean when given in poisonous doses is one which I have not previously adverted to, but will now shortly describe, as it is corroborative of the theory of the spinal origin of the paralysis in the cases under consideration. It is this, that just before the paralysis becomes universal, and therefore fatal, the animal often kicks out with its fore-legs, and, strange as it may seem, with its hind-legs too. The motions certainly are not voluntary, and partake of the nature of convulsions. They never accomplish any combined effect. The animal is never able to rise from the recumbent position, or even to scratch or kick an object presented to him. The movements are quite uncoördinated, showing, in my opinion, that first the power of voluntary movement is taken away by the influence of the bean, and then that the power of coördination in the muscles is annihilated; and, lastly, the animal dies utterly paralyzed, bereft of all the actions of the spinal cord.

The state of the iris produced by the action of the Calabar bean is an interesting but very difficult subject. When a square of gelatine, containing the active principle of the bean, is placed under the eyelid, it speedily causes contraction of the pupil and abolition of the accommodating power of the eye. The vision is only clear for a certain fixed and generally rather a short distance. The same effects, especially that on the pupil, have been observed by Dr Fraser in his experiments with the bean when acting generally on the nervous system. I must confess, however, that I have not observed this contraction of the pupil to be at all so remarkable in my experiments, or in the two cases of traumatic tetanus treated by me with the bean. From my experience, the effect on the eye is not nearly so great or so long continued when the bean is administered generally as when it is applied locally to the eye. I think the same thing might be stated with regard to atropine and belladonna. It is only when large doses or very frequent doses of any of these substances have been given that much effect is produced on the eye; and especially in my experiments on the lower animals, I have noted that the pupil was but slightly affected, even when the animal was under the full action of a poisonous dose of the Calabar bean.

It is not easy to account for this difference between what, for brevity, I may call the local and general action of the bean upon the pupil, for in both cases the action must be on the nerves and on them through absorption into the circulation. I can only suppose that in the local application more of the poison is carried directly to the ciliary ganglion than after the general administration, and that thus the effect is stronger as well as more lasting.

Then, how to explain the contraction of the pupil by the action of a substance causing depression of the spinal centres seems at first a deep mystery, and the only way of getting over the difficulty is by carefully applying the physiology of the nerves of the iris to its solution. It is well known that all the nerves of the iris pass through the ciliary ganglion, and that the short root from the third nerve, or motor oculi, is that which supplies the principal motor nerves to the iris. It has also been clearly made out, by cutting and irritating that nerve, that it supplies the sphincter muscle of the iris. So that irritation of it produces contraction of the pupil, and its section leaves it dilated. Again, it has been sufficiently demonstrated by Magendie that the ophthalmic division of the fifth nerve is a purely sensory nerve; but from recent experiments it would seem that some motor fibres are combined with its naso-ciliary branch; for irritation of that nerve produces motions in the iris with generally more or less dilatation. Now, MM. Budge and Waller have shown that irritation of the trunk of the sympathetic in the neck, or of the lower part of the cervical spinal cord, produces dilatation of the pupil by exciting contraction of the radiating fibres of the iris; while, on the other hand, sections of the sympathetic leave the pupil permanently contracted. The great probability therefore is, that the sympathetic root of the ciliary ganglion is that which supplies motor fibres to the radiating fibres of the iris, and that they are distributed along with the fibres of the naso-ciliary branch of the ophthalmic, which are themselves purely sensory.

The action of the Calabar bean on the pupil is then thus explained; for whenever it comes to affect the lower segment of the spinal cord in the neck, its influence immediately extends to the trunk of the sympathetic through its numerous branches of communication, and hence the motor nerves of the radiating fibres are paralyzed; while the third nerve, being of cerebral origin, is unaffected by this influence on the spinal centres, and so the annular fibres of the iris receive their usual nervous supply. In short, they are not antagonized by the radiating fibres, and therefore the pupil becomes contracted. Still farther, it seems to me that this theory explains the fact previously mentioned, that the local application of the bean produces more decided contraction of the pupil than its general administration; for, in the latter the animal experimented on dies very soon after the influence of the bean has reached the cervical region of the spinal cord, and in one of the two human subjects in whom I have watched its operation, a decided contraction of the pupil was only produced along with very unpleasant asphyxial symptoms. If these had not been speedily recovered from, I believe that death would have been the result, as it was in the case of the lower animals.

II.—The second physiological effect of the Calabar bean when given in poisonous doses is increase of the various secretions of the body. There is profuse perspiration, abundant flow of tears, a frothy mucous and salivary discharge from the mouth. This is sometimes tinged with blood. The urine is passed in considerable quantity, and there are fluid evacuations from the bowels.

I think that these phenomena are attributable, 1*st*, To the congestion of the secreting organs consequent upon the gradual arrest of the pulmonary circulation, which causes a temporary increase of their usual functions; and, 2*d*, It is in great measure due to the mere muscular relaxation of all the fibres of the body, the coats of the bloodvessels participating so as to permit of their great distention with slowly moving blood. Hence, there is a watery transudation from them mixed with the secretions, just as we have in cholera; and in some of the convulsive movements of the animal we are apt to have rupture of small vessels and blood escaping with the fluid, especially from the mouth and throat.

I now proceed to treat of the second section of my subject, viz., the antagonism of the Calabar bean to tetanus, and to the precisely similar state produced by strychnia in certain doses. The pathology of this state, if not quite satisfactorily explained, is, at all events, better understood by us since certain recent advances in the physiology of the nervous centres; for it was not to be expected that morbid anatomy alone could elucidate this, or, indeed, any but a few of the diseases of the nervous system. For the most of them the appeal must be made to physiology, to the otherwise known functions of the organs concerned, and to the clinical observation of their derangement during the lifetime of the diseased. Thus, since the discoveries and writings of the late Dr Marshall Hall it has come to be an established doctrine in physiology, that "physical" movements independent of both sensation and volition can be excited by stimuli applied to the surface of the limbs in virtue of a peculiar state thereby produced in the segment of the spinal cord into which the nerve is implanted. Whether Dr M. Hall's theory of a true spinal marrow and a special set of excito-motor nerves be adopted or not, there can be no doubt of the principal facts on which that theory was built, and of the general doctrine stated above which flows from them. This peculiar state of the spinal cord has been named a "polar state," or a state of "polarity," by Dr Todd; and it is well known that in certain diseases this state of the cord is greatly increased. Hence, *e. g.*, we have the convulsions of epilepsy, the startings of paraplegic and the occasional rigidity of hemiplegic limbs, all from some morbid influence, which it may often be difficult to define, exalting the polarity of the spinal cord, and affecting, through their nerves, the limbs of the patient. Now, the only way in which we can feasibly explain the phenomena of tetanus is by assuming that the irritation of the sensitive nerves of the wounded part (in traumatic cases) is communicated to their spinal centre, and that thus its polarity becomes greatly exalted, so greatly that the slightest touch of the surface, or the making an effort to move, or perhaps even the mere internal progress of the morbid action itself, is enough to call forth the most violent contractions of the voluntary muscles, and to maintain contraction in some of them uninterruptedly for days together.

In idiopathic tetanus the only pathological difference seems to be that the irritation originates not in a wound, but in the bowels or some of the internal organs.

Lastly, strychnia seems to have the power of directly producing the same effect on the spinal centres through the blood, and therefore of exciting an artificial tetanus of the severest kind.

The common cause of death in tetanus of all kinds is a more or less speedy asphyxia from fixation of the respiratory muscles by tonic spasm, which may be continued so long as to kill in the fit, or it may be repeated frequently at such short intervals as to have the same effect of stopping the respiratory changes. I can easily believe, however, though it has not occurred to me to witness such a case, that the glottis may be tetanically closed so as greatly to assist in this result, and I am quite sure that in poisoning by strychnia the heart is sometimes arrested in its action by tonic spasm of its fibres. But I think that all the morbid lesions, such as congestion and exudations in the spinal cord and softening of its tissue, observed by Mr Lockhart Clarke and others, after death from tetanus, are rather results of the primary morbific influence than its cause. I place them in the same category with the ruptured muscular fibres which have been likewise often observed in these cases. Besides, it must be remembered that none of these phenomena are constant. They have not been found by Todd and Bowman, and other equally good observers. I conclude, then, that the true pathology of tetanus is that it is due to a greatly exalted polarity of the whole or of part of the spinal cord, and that this may originate in causes acting first on the nerves, and then on the centre, or directly upon the centre itself.

Now, in the Calabar bean, as formerly shown, we have an agent which powerfully acts on the spinal cord, and that in a depressing or depolarizing manner. The animals, when under its influence, have no power of voluntary motion, and the physical motions are only feebly or not at all excitable. It is very remarkable, too, that it causes death proximately by asphyxia, no doubt just as do tetanus and strychnia, but more remotely the asphyxia is caused by the very opposite state of the mechanism of respiration,—viz., by paralysis instead of spasm; and when the heart is affected directly by the Calabar bean, which it undoubtedly is in some cases, the same thing is true. Its fibres are paralyzed, not spasmodically contracted, as in tetanus.

Thus the influence of the bean upon the spinal centres is seen to be directly opposed to that of tetanus and strychnia, and its secondary effects on the motor apparatus is also the very opposite. Instead of increased polarity of the centres we have diminished polarity, and instead of increased excitability of the motor organs we have paralysis. If, then, we could learn to use the one agent against the other we might produce complete neutralization. The following experiments, performed with the view of counteracting strychnia with Calabar bean, will be interesting in this point of view, and let me remark that there is no chemical action of the one substance upon the other. It is simply a physiological counter-effect that is produced.

Exp. VI.—Performed, on 21st December 1866, on a Scotch terrier, apparently strong and healthy.

At 3.2, I gave him a pill of bread-crumb containing a grain of powdered strychnia. He coughed it out after it had been held in his mouth for a few minutes, and it was thought he had got very little of it, but at 3.13 he exhibited all the usual symptoms of the drug in rapidly increasing intensity. He had a violent fit of opisthotonos before I interfered, but then I administered by the mouth the tincture of Calabar bean in ♏ x. doses, and increased them to twice that amount. I gave these doses every five minutes for nearly half an hour, and by that time he was much more relaxed, and was able to get upon his feet, but he immediately fell down again in a fit of opisthotonos. Still stronger doses of the bean were given, but had no effect, for the spasms continued uninterruptedly, and the dog died at 3.40, just after a fit.

On inspection, the left side of the heart was nearly empty and contracted, while the right side was distended with partially coagulated blood. The lungs were congested.

In the preceding experiment the dog was evidently killed by the strychnia, while the bean did not seem to have acted at all. The pupils were never contracted in the smallest degree, and any relaxation of the muscles that occurred was just in the intervals between the spasmodic fits. The experiment only teaches that strychnia acts much more rapidly than the bean, and that it counteracts the latter so powerfully as even to postpone its action perhaps altogether, as in the experiment, especially if it has begun to act before the bean has had time to do so.

In the following experiment the action of the bean was more obvious.

Exp. VII.—Performed, on 29th December 1866, on a large and strong rabbit.

At 9.58 A.M., I gave it ♏ v. of liquor strychniæ by the mouth, and in four minutes afterwards it had a tetanic fit with opisthotonos, in which it fell from the table. Still it was able to stand on recovering from the fit. I then (at 10.9) made it swallow ♏ xv. of the tincture of the bean. Within the next six minutes it had other two fits, the second one being very severe, and again causing it to fall from the table. I now (at 10.16) gave a second dose, the same as the preceding, and it soon afterwards seemed to be better, and was able to rise and walk a little. It had no other fit for nine minutes, and that was milder than formerly. A third dose of ♏ x. of tincture was then administered at 10.28, and soon after this the effects of the bean made their appearance. The rabbit lay down on its belly, and remained quite still unless touched, when slight spasms were produced. Then its breathing became panting and peculiar,—a constant effect of the bean,—and it was very sluggish. The spasms did not now seem to be produced by touching its sides as before; and therefore, to see how far it was paralyzed, it was lifted by the ears, which brought on a very violent tetanic fit, in which all the limbs, as well as neck and tail, were involved. After this it lay upon its side, its pupils were contracted, and its limbs quite palsied. In this state it died at 10.40.

On opening the chest immediately after death, the heart's action was found to have ceased, and its walls were not irritable. The lungs were empty and collapsed.

In this case the struggle between the strychnia and the bean was more observable. Although a very strong dose of the former was given, yet the bean seems to have in some degree checked its action, and to have almost antagonized it in twenty minutes after its first administration. From that period the symptoms were chiefly those of the bean, from which it might have recovered, had it not been rudely disturbed from its state of quiescence and held up by the ears. Such a stimulation broke through the depression of the bean, and excited once more the action of the strychnia, which the animal was not now able to bear. It was, however, my own impression, and that of my assistants, that the rabbit, if let alone, would have rested quietly till the effects of the strychnia had passed by, or till it had escaped from the system; and the lessons which we learned from the experiment were, the importance of quietness in the recovery from tetanus, even when the bean is fully acting on the patient, and also that the bean is a cause of great general prostration, in which an animal can ill bear the violence of a convulsion fit. Hence the necessity in the human subject for the exhibition of food and stimulants, and likewise for caution in the use of such a powerful agent. The great object during its administration should evidently be to give the smallest possible dose of the bean that will produce its physiological action.

In the following experiment I tried to anticipate the action of the strychnia by that of the Calabar bean.

Exp. VIII.—Performed on 29th December 1866. I took two rabbits, rather under the usual size, but very active. To one of them I gave ♏ v. of liquor strychniæ by the mouth, and it proved almost immediately fatal. To the other I gave the tincture of Calabar bean by the mouth.

At 3.5 P.M. I gave ♏ v., at 3.35 I repeated a similar dose, at 3.45 I gave a double dose, viz., ♏ x., still without any effect, and therefore a fourth dose of ♏ xv. was given at 3.52. In two minutes after this last dose was taken, the breathing became panting, and the animal shivered as if from weakness of the limbs. The pupils were still natural. In other two minutes it lay down on its belly, with its legs stretched out before and behind, apparently quite paralyzed. It was very unwilling to move, but could move its face and ears.

At 4.2, while it was in the state just described, I gave ♏ v. of liquor strychniæ, and again the effect was immediate, the animal being thrown into violent convulsions of the tetanic kind. After a short interval the fit was repeated, and then the animal died only four minutes after the administration of the strychnia. On inspection of the chest, the heart was found stiff and empty, and the lungs collapsed and containing very little blood.

In this case the life of the rabbit was no doubt prolonged by the influence of the Calabar bean, but the strychnia, in the dose employed, proved a much more powerful poison, and the result again indicated the inability of the animal to bear convulsions when its nervous power was diminished by the action of the bean. I have no doubt that the animal died of the strychnia, and that I had given too large a dose to be counteracted by the Calabar bean, before the animal's strength was worn out. I therefore performed the following experiment.

Exp. IX.—Performed, on 20th March 1867, on a middle-sized rabbit.

At 3 P.M., I injected, with the hypodermic syringe, ♏ vi. of the tincture of Calabar bean under the skin of the back, and immediately afterwards ♏ iij. of the liquor strychniæ two or three inches higher up the back.

At 3.8, the tetanic starts were very distinct, and I immediately injected other ♏ vi. of the tincture of Calabar bean under the skin of the opposite flank. Very soon after this, viz., at 3.10, a very severe tetanic convulsion occurred. Opisthotonos was quite marked, the very tail of the animal being bent back. Both fore and hind legs were affected with the spasms, but on coming out of it, which it did in two minutes, the hind legs lay behind the animal apparently paralyzed. At 3.14, it got up on its fore-legs, held up its head, and moved a few steps, dragging the posterior limbs behind it. At 3.16, it had partially recovered the use of the hind-legs, and ran off a short way, the hind-legs, however, being still feeble. Whenever it moved it was seized with violent shivering, which was of tetanic character. On one of these occasions (at 3.17), the shivering became a fit of rigidity, but the animal was not thrown down as before. It remained standing. The right hind-leg was exempted from the spasm, and lay out behind in a paralyzed state. It recovered from this fit in two minutes, but remained quiet till 3.21, when it got upon its legs and ran into a corner, where it sat down so as to be supported by the wall both behind and on the left side. Five minutes afterwards it moved on its fore-legs unsteadily, the hind-legs being again paralyzed, and in other two minutes (3.28) it started upon its fore-legs, with head erect, and screamed loudly, as if frightened or in pain. It then sat down in a natural position, but supported by the wall, and remained quiet till 3.55, when it turned the front of its body, using only the fore-legs. It had some tetanic starts after this, but remained still in the corner. The breathing was quite natural, and the animal seemed to be recovering. At 4·5 it ran across the floor, using the hind-legs a little weakly, and mounted on the top of an upturned basket in which it had been kept. It took food, and remained quite well during the afternoon and night.

At 8·43 next morning, the rabbit was quite well, and in every respect as before the experiment. I therefore injected ♏ iij. of liquor strychniæ under the skin of the shoulder, and left it to its fate. In two minutes there was violent tetanic convulsions, from which it only relaxed in death, four minutes after the injection had been performed.

In this experiment the proportion for safe antagonism was pretty exactly reached; and, by a moderate but well-timed administration of the bean, the strychnia was prevented from producing what was afterwards proved to be its natural effect, if unresisted, viz., the death of the animal. No experiment could better prove the power of the bean in counteracting the tetanus of strychnia, the usual slowness of operation of the former being greatly modified by the hypodermic method of administration. It produced its characteristic effects in six minutes in this case, though acting against the strychnia; and in Exp. I., previously related, in which the hypodermic method was used, and not the strychnia, the bean seemed to act immediately. It is worthy of consideration, if, even in the human subject, when the tetanus either of disease or of strychnia is very violent and acute in its progress, this may not be the proper way of administering the bean. I remark, also, that by the repetition of the injection whenever the tetanus became distinct, eight minutes after the first introduction of the tincture, its influence was prolonged over the entire period of the action of the strychnia, yet it never was so strong as to weaken the rabbit to any great or prejudicial extent. Lastly, the animal was intentionally kept from any start or rough handling which could excite the spasms, but was rather permitted to lie quiet till the effect of the strychnia had worn off. I cannot but think this experiment very encouraging, and very instructive as to the antagonism of Calabar bean to the tetanus of strychnia in poisonous doses.

Exp. X.—Performed, on 23d March 1867, on a large and strong rabbit.

At 3.3 P.M., I injected ♏ x. of tincture of Calabar bean, and in thirteen minutes thereafter it was found very excited, as if frightened. It breathed very rapidly, and, on being approached, ran wildly about the room. This continued till at 3.16 I again injected ♏ vi. of the tincture, and in two minutes afterwards the rabbit fell down on its belly, but with head erect. When I lifted him on to the table he started and kicked feebly. I now (3.22) injected ♏ iii. of liquor strychniæ over the shoulder. Soon afterwards the breathing became peculiar. It was accompanied with an audible blowing noise, and the abdomen moved very much, evidently performing the chief part of the respiratory acts. Very soon the rabbit lay down on its belly with its legs stretched out before and behind. It got up again, and stood shakily. A little frothy mucus came from its mouth. All this time the symptoms were entirely due to the Calabar bean, and not at all to the strychnia; but at 3.28, *i.e.*, seven minutes after the injection of that substance, the rabbit gave a slight spring forwards on all its legs. This was quite momentary, and it was difficult to tell whether it was the effect of the strychnia or a voluntary effort, for immediately afterwards it ran across the room, and jumped into its basket, the side of which was about a foot high. Its motions were, however, stiff and ungainly; and when in the basket, it stood on its four legs and screamed violently, but showed no other sign of pain or fear. It was quite sensible, and started naturally when the floor was stamped on, even at a distance. In a few minutes more, however (at 3.33), it had a decidedly tetanic start, and touching its sides or limbs produced similar slight contractions of the muscles. When it attempted to move, its limbs seemed to be stiff, and not fully under command of the will.

At 3.37 it gave another convulsive spring; and on being touched it had a violent tetanic fit. Believing that the effect of the Calabar bean had worn off, I injected ♏ vi. of the tincture once more below the skin of the back, and very soon after this the rabbit seemed wholly relaxed, lying on its side, and kicking especially with the fore-legs, as if to try to get up, which it was unable to do. At 3.41 it had another severe fit of opisthotonos, after which it was very weak, and breathed so ill, that if I had not assisted it by making artificial movements of the chest, it would have ceased to breathe. When it was restored it breathed with great rapidity (68 in a minute), and very imperfectly, the air passing through mucus in the trachea. It had frequent slight twitches of the limbs till 3.52, when it had another fit of opisthotonos, and died immediately after.

On opening the chest, the heart was found to have ceased to pulsate. Its cavities were all full of blood; those on the left side of a bright florid colour, and those on the right of dark venous hue. The lungs were collapsed, and nearly bloodless. The contractility of the muscles was apparently worn out, for they did not respond to the usual stimuli.

This experiment is the last and only additional one which I have performed with these two powerful poisons, in order to investigate their antagonism; and perhaps I might not have introduced it here were it not that it completes the series of my observations; and though at first sight rather contradictory of the immediately preceding experiment, yet it is not so in reality, but rather teaches a very useful lesson in regard to the physiological action of the Calabar bean. For it will be obvious, on careful perusal of the description of this experiment, that the rabbit was first brought under the influence of the bean, which occupied nineteen minutes before it was accomplished; then the strychnia was injected, and, instead of its acting immediately, as it would have done if the bean had not been employed, it took ten minutes before doubtful and, at all events, very slight tetanic symptoms were produced; and it was not till seventeen minutes after the injection of the strychnia, and thirty-six after that of the first introduction of the tincture of the bean, that general convulsions occurred. But by this time the action of the bean had become too slight to counteract that of the strychnia, and the animal was killed by the latter before the former could be renewed by further injections, for the one practised at 3.37 never acted at all. For it is well shown in this experiment that the antagonism of the Calabar bean and strychnia is mutual; that either of them prevents the action of the other, and that the most powerful will be the one which has the greatest hold on the system at the time. Thus, at the commencement of the experiment, the bean acted alone, and got its full power over the animal. Afterwards, when the strychnia was introduced, the bean would not let it act, and so its presence was not manifested till the influence of the bean diminished. Then it, in turn, took possession of the nervous centres, and though the bean was again introduced, it was excluded by the strychnia, which kept its power over the animal till death released it. Nothing could better show the mutual antagonism of these two substances; and the lesson, in regard to therapeutics, which flows from this physiological law is, that in the treatment of tetanus, we must never allow the influence of the bean to become weak, even for a time after the disease seems to have been overcome; for if we do, we may find it difficult or impossible to renew its force in the system, when the disease renews its attack on the patient, emboldened, so to speak, by the felt feebleness of its formerly successful antagonist. The proper medium must be struck between maintaining such an action of the bean as will diminish prejudicially the vital energies of the patient, and permitting it to become inadequate to the suppression of the morbid exaltation of the spinal polarity, which the disease seeks to perpetuate or to renew.

It must be noted, however, that in some of these experiments the poisonous influence of the strychnia was too strong as well as too rapid for the Calabar bean, and it is not difficult to foresee that cases of tetanus from disease will arise which will be able to resist entirely even the potent influence of that subtle agent. In different persons, likewise, the nervous susceptibility may be different; so that it is more than likely that, in some, the excitement may be more easily accomplished than the repression of morbid polarity of the spinal centres, and from this cause, too, failures may arise in the application of the Calabar bean to the treatment of tetanus. Nevertheless, I think it will be acknowledged by all, that it is by far the most promising counter-agent of the tetanic poison at present known to us. Woorara is the only other that will compare with it; but the enormous rapidity and energy of that poison, as well as our want of knowledge regarding its nature, and the absence of preparations of fixed strength, all combine to exclude that curious substance from therapeutic use.

I have only as yet had the opportunity of applying the Calabar bean in two cases of traumatic tetanus, and these I have fully recorded in the *Lancet* for the 2d of March, in this present year.[1] I may abridge these as follows.

Case I.—Annie W., age 11 years, admitted into the Royal Infirmary, November 12, 1866. Three weeks ago, she struck her toe against a stone, and it is still bruised, and its skin slightly ruffled. Six days ago, she began to have twitches, and now tetanus in its severest form is fully developed. The jaws are quite locked, and the body bent back in three-quarters of a circle. Chloroform was twice administered during the first night to relieve the violence of the spasms.

Such are the chief points of the report in the hospital journal as to her state on admission. After clearing out the bowels, I began to treat her with Calabar bean on the 15th. The paper used in ophthalmic surgery was first employed, and next day the extract in solution, one-eighth grain for a dose, was taken every half-hour. By the time she had taken two grains at this rate, she was found lying on her back in a semi-comatose state completely relaxed, with her mouth open, and her pupils moderately contracted. The breathing was quiet and regular, and the pulse rather hurried and full.

In this state of things the Calabar bean was stopped for two and a half hours, by which time the pupils were again dilated, and the spasms had begun to occur once more, when the patient was touched. Five drops of the tincture were given every hour during the night, and next day she was better in all respects, but relapsed in the evening, and seemed as bad as ever next day. I therefore prescribed one-half grain of the extract in pill, to be given every hour. Instead of this, a whole grain was given by mistake of the apothecary in making the pills. Nine pills were given before the mistake was discovered. By that time she was again thoroughly relaxed in all her muscles, except those of the back, which were still rigid; her pupils were contracted, and her heart's action was tumultuous. Her face was pale, and her eyes wide open. Her breathing was jerky, and a mucous rattle was in her throat.

From this state she recovered by the use of stimulants, vinum belladonnæ, and the extraction of the mucus from the throat—the first symptoms of restoration being that the respiratory movements were more freely performed, and that the pupils dilated.

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[1] Since these cases were published in the *Lancet*, another case of traumatic tetanus has been treated in the same method with Calabar bean, by Dr Andrew Campbell, formerly a pupil of mine, and now practising at Navenby, Lincolnshire. Though the case was a most severe one, the result has been perfectly satisfactory, and will, I believe, be published in detail by Dr Campbell at an early date.

In a few hours afterwards the spasms could once more be induced by touching the limbs, but generally they remained flaccid. The bean was not again administered for a fortnight, in order that she might thoroughly rally from its influence, and become stronger. During this time fits of tetanic rigidity did occur, but they never were so distressingly violent as they had formerly been, and, under the use of very small doses of the tincture for about another fortnight, she completely recovered. I had not the least doubt that the powerful influence of the bean, above described, checked the disease, and that its more continued and milder action accomplished the cure.

CASE II.—Alex. M'Ph——, aged 13, was admitted into the Royal Infirmary on 6th December 1866.

Three weeks before admission he had his finger bruised and torn, but very slightly, and two days before admission he became affected with lock-jaw. On the above date he had frequent and severe tetanic fits, with opisthotonos.

After opening his bowels I ordered him ♏ v. of the tincture of the bean every two hours. Each dose had a perceptible effect in relaxing the rigid muscles, and in rendering him less susceptible to the induction of spasms on touching the limbs. In three days he was able to sit up in bed, and in seven days more he was out of bed walking, though stiffly, through the ward. In a few days more he was perfectly well.

It should be noted that in this case the doses were not so large as in the former one, and that the pupils were never so much contracted as on the two occasions in that case, when the action of the bean was greatest. The breathing was never affected at all, yet the progress was steadily towards recovery, though no other treatment was employed but food and aperient medicine. On the other hand, it must be remembered that the girl's case was much more severe than the boy's, and therefore required larger doses to counteract it. On one occasion she took two grains of the extract in eight hours, and on another, no less than nine grains in nine hours. On both these occasions she was brought into much the same state of complete relaxation of all the muscles, but those at the side of the back-bone, and physical movements could not for a time be induced,—showing complete depolarization of the spinal marrow, or, in other words, precisely the opposite condition to that in which tetanus originates. Perhaps in the boy's case, without any such marked effect of the drug, it was more safely, and quite as efficiently, managed.

I do not think that the Calabar bean has, properly speaking, a cumulative effect, such as digitalis possesses; but, as it is necessary to keep up its effect in order to counteract the tetanus, so if the doses be rather too closely given, there will be a kind of cumulative result, for the action of the one dose will be added to that of the previous one. It is well to avoid this in the human subject, and to take care that the effects of one dose be fully worn off before another is brought into operation; and the secret of success in treating such cases will undoubtedly lie in seizing the proper moment for repetition of the drug, so that its action will never be altogether absent, and yet never allowed to become unnecessarily strong.

The time which the bean takes to act in the human subject depends on the dose. The stronger it is, the sooner it will act, and *vice versa*. Small medicinal doses, such as ♏ v. of the tincture, take about twenty minutes before their action is perceived, and then this action continues for about half-an-hour, after which the effects of the drug soon disappear; and therefore the doses must be repeated about every forty minutes, if their continuance in full force is desirable. On this point the judgment of the surgeon must be his guide, as no rules can be laid down applicable to all cases. I may remark, however, that as the Calabar bean is strictly an antidote to tetanus, the doses of it which will be required and borne in cases of that disease will be directly proportionate to its severity.

In conclusion, in the case of an overdose of the Calabar bean, I believe that the best measures to be used for restoration are the clearing of the mouth and throat, mechanically, of the mucus accumulated there, and to employ artificial respiration, by moving the arms up and down, as recommended by Sylvester, and also by alternately compressing and relaxing the ribs. Electricity applied to the course of the phrenic nerves may likewise be useful, and also mustard cataplasms along the region of the spine. As soon as the patient can swallow, some stimulant properly diluted with water should be freely given.

ARTICLE VI.—*On Ventilation and Cubic Space.* By Staff-Surgeon F. DE CHAUMONT, M.D., F.R.C.S.E., Assistant-Professor of Hygiene, Army Medical School, Netley.

ALTHOUGH the questions of ventilation and cubic space have long occupied the attention of scientific men, it is only comparatively recently that their paramount importance has begun to be appreciated by the public and, I may add, the great body of medical men. It is not my intention to enter into the history of the subject at any length; I shall merely remark, that for a considerable period the conclusions arrived at on these matters were only conjectural, and founded less on scientific data than on those of experience loosely brought together, and the significance of which was but partially understood. But when, later, the composition of the air and the law of diffusion of gases came to be better understood, and simpler methods of examination and improved instruments rendered *facts* more easily attainable, it was to be hoped that we should get gradually rid of mere conjecture, and be able to found our opinions more and more surely upon ascertained truth. This progress has, in spite of many obstacles, been slowly going on, and things had arrived at a very hopeful point at the time of the publication of the Report of the Sanitary Commission of the Army in 1857, and the subsequent Barrack and Hospital Commission of 1861. It may be worth while briefly to recall the recommendations made in those reports,—which are, indeed, interdependent,—the recommendations of the second being based, to a great extent, upon the evidence collected by the first.

The first important point was that, after eliminating every other cause, the greater death-rate of the soldier over the civil population of the same age was mainly due to overcrowding with deficient ventilation. This was observable even in the army itself; by comparing the death-rate before 1853—viz., 17·5 in the 1000—with three years' experience in Aldershot and Shorncliffe, ending 31st December 1859, where in camp the death-rate was only 4·7; the main cause of the improvement being undoubtedly a better supply of air, the necessary result of living in huts and tents. The recommendations of the Royal Commission of 1857 directed to give the following amount of cubic space:—

Barracks at home, . . .	600	cubic feet per man.
Hospitals at home, . . .	1200	„ „
Do. in the tropics, . .	1500	„ „

all net space, exclusive of furniture, etc., etc.

To this the Barrack Commission added the condition that the air should be changed at least twice in the hour, giving, for the barrack-rooms with 600 cubic feet, a delivery per hour of 1200 cubic feet of fresh air. At the same time they insisted strongly on the necessity of having the minimum space of 600 cubic feet as the only condition on which the delivery would prove sufficient. This amount exceeds some of the previous recommendations, but is less than others proposed of late years. Some of the remarks of the commissioners are very important; at p. 34 we have the following:—"It has been said that the question of cubic space is simply a question of ventilation; but it is rather a question as to the possibility of ventilation. The more beds or other encumbrances you have in a room, with a limited cubic space, the more obstruction you have to ventilation; the fewer the beds the more easy is it to ventilate the rooms. There are fewer nooks and corners, fewer surfaces opposed to the movement of the air, and less stagnation. Stagnant air, especially in sleeping-rooms, is poison. We have been in rooms, both in barracks and hospitals, in which the atmosphere was positively offensive, with the doors and windows open." And on page 35,—"The only safe principle in dealing with the subject is to leave a large margin for contingencies; and the question really is, not whether 600 cubic feet per man be too much, but whether 600 cubic feet per man be enough for all the purposes of warming, ventilation, and comfort." And on page

36,—"Just as people have died of suffocation from impure air in dense crowds out of doors, with the unlimited atmosphere above them, so men, sleeping for seven or eight hours at a time in wooden huts, have suffered from severe types of fever when the space allotted to them has been reduced below a certain amount."

All the arguments here adduced in the case of barrack-rooms, occupied for the most part for only eight out of the twenty-four hours, tell with triple force in the case of sick wards, occupied more or less continuously during that time.

Such being the views and recommendations of those Commissions, founded upon a careful consideration of both opinions and experience, it was a little startling to find Dr Edward Smith, in his Report on the Metropolitan Workhouses (1866), strongly advocating for the infirmaries a space of only 500 cubic feet per head, in opposition to Mr Farnall, who recommended 1000. Dr Edward Smith was supported by Dr Angus Smith, and, their names having justly great weight, these views were eagerly seized upon and commended by many, principally on account of the great saving (or imagined saving) of expense. They were felt, however, by many, on the other hand, to be a retrogression, and, in the army particularly, were viewed with distrust and disfavour; for, if they became generally received, it was clear that much of the improvement which was being gradually effected by so much time and labour would be brought to a standstill. A great deal still remained to be done in providing barrack and hospital accommodation up to the standard laid down by the Commissioners; and plenty of members would have been found in Parliament to support any motion whereby the expense of such provision might be saved. Personally, also, I felt that it, to a certain extent, stultified the instruction which Dr Parkes and I were actually giving to the candidates entering the medical service, whose duty it would be hereafter to see that the soldier did really receive the proper cubic space allotted. A short paper of mine was published in the *Lancet* (1st September 1866), in which I endeavoured to show that whilst the small cubic space (500 feet) recommended by Dr Edward Smith might be sufficient so long as the ventilation arrangements were in good order, the danger to the inmates from impeded or obstructed ventilation was greater and more rapid than it would be in a larger space; and also that with our ordinary appliances it was more difficult to ventilate, perfectly and equally, a small than a large space. Since the publication of that paper, the Report of the Committee on Cubic Space and Ventilation of Metropolitan Workhouses has appeared (February 1867), in which many of the papers given go far to support the views I have advocated, and to those papers I propose to refer a little farther on.

To illustrate this point, more especially with regard to the quantity of fresh air necessary to restore a vitiated atmosphere to a required standard of purity, the following table was given :—

Cubic Feet of Fresh Air Delivered per Hour.	Carbonic Acid brought in by Fresh Air.	Total Carbonic Acid, including ·4 in the original space (1000 cubic feet); ·6 given off by the occupant, and that brought in by fresh air.	Ratio of Carbonic Acid at the end of the Hour.	Diminution of Carbonic Acid Ratio.	REMARKS.
	Cubic Feet.	Cubic Feet.	Per Cent.	Per Cent.	
Nil.	Nil.	1·	·1000	Nil.	The original air-space is taken at 1000 cubic feet, containing ·4 c. ft. of CO_2, and occupied by one man, who gives off every hour ·6 c. ft. Every 1000 ft. of fresh air brings in ·4 of CO_2; therefore the ratio is got as follows: $\frac{\cdot4 + \cdot6 + \cdot4d}{1000 + d}$ which gives the CO_2 per cubic foot; *d* being the number of thousands of cubic feet of fresh air delivered.
1,000	·4	1·4	·0700	30·	
2,000	·8	1·8	·0600	40·	
3,000	1·2	2·2	·0550	45·	
4,000	1·6	2·6	·0520	48·	
5,000	2·	3·	·0500	50·	
6,000	2·4	3·4	·0486	51·4	
7,000	2·8	3·8	·0475	52·5	
8,000	3·2	4·2	·0467	53·3	
9,000	3·6	4·6	·0460	54·	
10,000	4·0	5·	·0455	54·5	
11,000	4·4	5·4	·0450	55·	
12,000	4·8	5·8	·0446	55·4	
13,000	5·2	6·2	·0443	55·7	
14,000	5·6	6·6	·0440	56·	
15,000	6·	7·	·0438	56·2	
16,000	6·4	7·4	·0435	56·5	
17,000	6·8	7·8	·0434	56·6	
18,000	7·2	8·2	·0432	56·8	
19,000	7·6	8·6	·0430	57·	
20,000	8·	9·	·0429	57·1	
25,000	10·	11·	·0423	57·7	
30,000	12·	13·	·0419	58·1	
40,000	16·	17·	·0414	58·6	
50,000	20·	21·	·0412	58·8	
100,000	40·	41·	·0406	59·4	
1000,000	400·	401·	·0401	59·9	

To ascertain the delivery requisite to reduce a vitiated atmosphere to a required standard of purity we may either use the above table, or a formula stated as follows:—

I. Let R be the ratio of carbonic acid in incoming air.
„ r' do. do. in vitiated air.
„ c be the capacity of original air-space in cubic feet.
„ r be the desired ratio of purity to which r' is to be reduced.
„ d be the delivery of fresh air in cubic feet.
„ v be the total volume of air, $= c + d$.

Then: $$\frac{r' - \mathrm{R}}{r - \mathrm{R}} \times c = v, \text{ and } v - c = d.$$

It will be at once seen by the above formula that when $r = \mathrm{R}$, that is, when it is wished to restore c to the purity of the external air, v and d become *infinity*, so that complete purification of c is, under those circumstances, theoretically impossible.

II. From this formula another suggests itself, which is indeed the one from which the fourth column of the table is compiled:

$$\frac{r'c + \mathrm{R}d}{v} = r.$$

III. To determine the number of men, n, a cubic space, c, will accommodate, we have the following, r and R being the ratio per cubic foot, e the CO_2 expired by one man in an hour (= ·6 cubic feet), and h the number of hours.

$$\frac{(r - \mathrm{R})v}{e\,h} = n.$$

IV. To determine the delivery of air required to retain an occupied space at a given ratio of purity, r, we have—

$$\frac{n\,e\,h}{r - \mathrm{R}} = v, \text{ and } v - c = d.$$

V. Lastly, to find the total outlet and inlet area necessary to supply the requisite delivery at a given velocity in feet per second at the point of entry, l, we have—

$$\frac{d}{3600\,lh} \times 288 = \left\{\begin{array}{l}\text{Total outlet and inlet area per head}\\ \quad\text{in square inches.}\end{array}\right.$$

This is founded on the consideration that one-half of the number of available ventilation openings is generally inlet, and the other half outlet, the functions of each being often variable.

Dr Parkes, in his second paper in the Blue-book (Report of the Committee on Cubic Space of Metropolitan Workhouses), quoted formula, No. I., to illustrate some of his remarks. Of this formula, Professor Donkin says, "that it appears inconvenient in form, but that it gives correctly the answer to a particular problem." Now this is all that was intended by the formula, which was only one of several that were all forwarded to the *Lancet* as an appendix to my paper, but which, for some reason unknown to me, never appeared. Had they done so, it would have been seen that I had provided for the case alluded to by Professor Donkin in his last paragraph. He states the problem thus—"Suppose a room of (c) cubic feet, containing initially (π') units of poison per cubic foot, to be shut up for (t) hours with a man in it, producing (P) units per hour. At the end of that time how much fresh air, containing (p) units per cubic foot, must be added to the air of the room in order to reduce the quantity of poison per cubic foot to (π) units?" And the formula is thus given—

$$\frac{c(\pi' - \pi) + \mathrm{P}t}{\pi - p} = \mathrm{A}.$$

Now this was the problem provided for by formula No. IV., namely—

$$\frac{neh}{r - \mathrm{R}} = v\,;\ \text{or, since } v = c + d,\ \frac{neh}{r - \mathrm{R}} - c = d.$$

If we transcribe this into the symbols used by Professor Donkin, we have the following,—

$$\frac{n\mathrm{P}t}{\pi - p} - c = \mathrm{A}.$$

This is virtually the same formula (with the addition of (n) for the number of inmates, and taking (c) as the total cubic space of a room with one or more inmates); but it is less complete than Professor Donkin's, because it starts from the basis of (c) containing *fresh air* initially; in such a case (π') was, of course, unnecessary, as it was identical with (p). In order to make it sufficient for a case where $\pi' > p$, we should only have to add the excess of $\pi' c$ over pc, and the case would stand thus—

$$\frac{n\mathrm{P}t}{\pi - p} - c + c(\pi' - p) = \mathrm{A}\,;$$

and it is evident that this, reduced to its simplest form, is,

$$\frac{n\mathrm{P}t + c(\pi' - \pi)}{\pi - p} = \mathrm{A}\,;$$

or identical with Professor Donkin's formula. This, expressed in the symbols I have before used, with the addition of (r'') for the initial CO_2 ratio of (c), stands thus,—

$$\frac{neh + c(r'' - r)}{r - \mathrm{R}} = d.$$

The advantage of expressing the formula, as Professor Donkin has done, is, that "it shows (as he says) that, if $\pi' = \pi$, that is, if the room is to be brought back to its initial condition, the quantity of air required is independent of c, that is, of the size of the room." This was not clearly seen from my original formula (No. IV.), because I started with the case of a room c filled with fresh air, and in such a case $\pi' = p$, which rendered π impossible, if it was required to $= \pi'$, for in that case $\mathrm{A} = \infty$, or an infinite quantity of air would have had to be supplied.

It would of course be necessary to modify in a similar way formula No. IV., in the event of the initial ratio of c (that is r'') being greater than the ratio R of the fresh air, and the formula would stand thus:

$$\frac{d(r - \mathrm{R}) - c(r'' - r)}{eh} = n.$$

There are two very important statements in Professor Donkin's paper,—

1. "It seems hardly conceivable, though it is mathematically possible, that the whole quantity of poison remaining permanently in the room could be reduced by any contrivance *below* that of uniform diffusion."

2. "The same supply of air will equally ventilate any space," or (what is the same thing) as much air is required for a small as for a large space. And this is mathematically proved from the algebraical demonstration given.

Let us now proceed to an application of the above principles, and see how far they lead us to a satisfactory practical result.

The question then narrows itself to this:—What is the minimum cubic space per head that would admit of the following desiderata being attained?

1. An adequate floor-space.
2. A change of atmosphere frequent enough for health, and not so frequent as to cause perceptible draughts.
3. Sufficient space to reduce to a comparatively small amount the danger from temporarily impeded ventilation.

1. As regards the area or floor-space, Dr Acland has stated the question very succinctly in his two short memoranda on the subject in the Report (1867). He comes to the conclusion of 72 square feet per bed as a minimum. This, with 12 feet of height, gives 864 cubic feet. With 14 feet of height, this would give 1016 cubic feet. As, however, it is doubtful whether much advantage is obtained from air above 12 feet, it would, I think, be better, if 1000 feet be necessary, to increase the area to 84 feet, or nearly that recommended by the Barrack Commission. This amount, given as a minimum, would probably be sufficient for practical purposes.

2. The second part of the inquiry is, What amount of space will admit of a change of atmosphere sufficiently frequent for health, and not so frequent as to cause perceptible draughts? Now, it has been clearly shown that, "*uniform* diffusion being supposed, the same amount of air will equally ventilate any space" (see Professor Donkin's paper); and I think it is also evident that, whilst uniform diffusion may not be, and probably is not, constant, we cannot safely legislate for anything *short* of uniform diffusion. The point now is to obtain a minimum of purity as a standard. It seems to me that this may be taken at the point where the smell of organic matter (*closeness*, as it is commonly called) ceases to be perceptible. Dr Parkes places this at about ·06 per cent. of *carbonic acid*, which is the most convenient measure of purity, as the organic matter generally increases and diminishes in the same proportion. From my own observations, I find the following remarks on different degrees of impurity:—

At ·1408	per cent.	Extremely close and unpleasant.
„ ·1200	„	Very close,
„ .1154	„	Extremely close and unpleasant.
„ ·1090	„	Extremely close.
„ ·1072	„	Very close.
„ ·0962	„	Very close.
„ ·0921	„	Close.
„ ·0860	„	Rather close (day-time).

At ·0843	per cent.	Not very foul.
„ ·0820	„	Not very foul.
„ ·0804	„	Very close.
„ ·0804	„	Close.
„ ·0759	„	No unpleasant smell perceptible.
„ ·0697	„	Rather close.
„ ·0658	„	Not very close.
„ ·0642	„	Not very close.
„ ·0568	„	Not close.

Although there are a few slight discrepancies, the sensations appear to proceed very regularly, considering that the observations were made at different times, and all recorded *before* the amount of carbonic acid was known. Some of the discrepancies may have arisen from the rooms not being entered directly from the open air, which should invariably be done if possible, as the sense of smell is in this particular speedily blunted. I think it would be safe, then, to conclude that at ·06 per cent. the smell of organic matter becomes imperceptible in most cases, and that this may be confidently taken as a minimum of purity. Now, to attain this minimum, that is, to insure the carbonic acid ratio never rising above ·06, we must supply 3000 cubic feet per head per hour; for, taking Professor Donkin's formula,[1]

$$x = p + \frac{P}{A};\ \text{when } x = \cdot 0006,\ A = 3000.$$

Here p = carbonic acid per cubic foot.
P = ·6, carbonic acid given off by one adult in an hour.
A = delivery of fresh air in one hour.
x = ratio of carbonic acid (in this case ·0006 per foot) desired.

This being the quantity, we must now consider how often we can change the air of a room without producing a draught. This must depend upon varying conditions of temperature, etc., but it will be difficult to effect it oftener than six times in an hour at ordinary times, and, indeed, this is what the experiments of Pettenkofer led to. It is sometimes possible to do it oftener, for example, at Hilsea Hospital I found the air to be changed twelve times in an hour, but this was on a mild night with a slight breeze, and with a number of the windows open. This could not have been borne had the external temperature been lower. Moreover, we must provide for a case where the ventilation can go on at all times, whether windows be open or shut, although we can never entirely

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[1] The complete formula is as follows:—

$$x = p + \frac{P}{A} - \frac{P}{A}\epsilon^{-\frac{At}{c}};$$

but as the last term becomes rapidly inappreciable, it has been omitted.

do away with the necessity of opening windows from time to time, when circumstances permit. Now if the original cubic space allowed be only 500 cubic feet, we shall find that we have already reached our limit of change, if we seek to keep the air at ·06 per cent., for $\frac{3000}{500} = 6$, that is, the air must always be changed at the rate of six times an hour, as long as the space is occupied,—a condition which will be admitted to be barely possible by all persons who have examined the subject carefully. At Aldershot, the following results were obtained in barrack-rooms ventilated on the plan proposed by the Barrack Commissioners. I select them because the cubic space nearly approaches 500 feet, and because they show the *most favourable* side of the question.

A room on the ground-floor, furnished with a ventilating stove, extraction shaft, and louvred inlet ventilators, had the air changed during the night, from 9 P. M. to 5 A. M., only 14 times, or a little more than 1¾ times an hour. A room in the first floor (with similar arrangements) during the same time had its air changed 13 times, or less than 1¾ times an hour, the number of men being fewer than in the former. A similar room on the second floor during the same time had the air changed only 9½ times, or less than 1¼ times an hour. In each of these cases the rooms were calculated for 24 men, having each 540 net cubic feet of space, but none were full; the first had 18 men, giving each 722 feet, and about 1200 feet of fresh air per hour, and yet the carbonic acid at 5 A. M. was ·0855 per cent.; the second had 13 men, giving each a space of 999 feet and a delivery per head, per hour, of about 1700 feet, the carbonic acid at 5 A. M. being ·0759 per cent.; the third had 22 men, giving each a space of 585 feet and a delivery of about 765 feet per head, per hour, the carbonic at 5 A. M. being ·12 per cent. If now we take a room ventilated on the ordinary principle (for the three above mentioned are the only ones where an attempt has been made to carry out the recommendations of the Commission), we find that in six hours the air was changed only *five* times, or only $\frac{5}{6}$ times an hour. In this case the only real ventilator was the open fireplace, the gratings intended for ventilation being too small to be of any practical utility.

Bearing in mind these facts, it will be obvious how difficult it is to ventilate effectually so small a space as 500 feet per head with our present means of ventilation.

On the other hand, if we give 1000 cubic feet per head, the problem becomes much simplified. We shall still require to supply 3000 feet per head, per hour, but this will be equivalent to a change of air only once in twenty minutes, and will leave a wide margin for contingencies between that and the proposed maximum, six times an hour or once in ten minutes. In addition to this, the maximum of impurity is sooner reached with 500 feet than with 1000, but the difference is too small to be of practical value.[1] It is different, however, when we come to consider the consequences of impeded or arrested ventilation. Let us suppose two occupied spaces, one of 500 and the other 1000, ventilated so that the ratio of carbonic acid is ·06 per cent., and that from some cause or other the ventilation is arrested in both, the condition will then be as follows:—

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[1] See Professor Donkin's complete formula in note to page 1031.

1000 feet. Ratio of impurity:		500 feet. Ratio of impurity:	
After one hour	·12 per cent.	After one hour	·18 per cent.
„ two hours	·18 „	„ two hours	·30 „
„ three „	·24 „	„ three „	·42 „
„ four „	·30 „	„ four „	·54 „
„ six „	·42 „	„ six „	·78 „
„ seven „	·48 „	„ seven „	·90 „

There is some difference of opinion as to the amount of carbonic acid that can be borne, but when conjoined with fetid organic matter, as in the products of respiration, it is pretty generally agreed that ·3 per cent. can hardly be supported, and that at ·5 per cent. the atmosphere is unendurable. From the foregoing table it will be seen that ·3 per cent. is reached in the case of 500 feet in *two* hours, but in the case of 1000 feet only in *four* hours, whilst ·5 per cent. would be reached in the former case in about 3½ hours, in the latter not until 7 hours had elapsed. Of course, this is an extreme case, but it is applicable, in the due proportion, to other cases. This point brings me to consider the remarks of Dr Markham (whose paper throughout shows a bias towards Dr Edward Smith's view of the question), where he says (Blue-book, p. 8), "In fixing, therefore, the space required at 1000, 1500, or 2000 cubic feet, men of science, I think it may be fairly said, have been guided rather by the idea of making a wise and ample provision for the *possibilities* of the case than by any positive demonstration of the exact *necessity* of it." This would, of course, apply to the latter part of the argument I have used, referring to the contingency of impeded or arrested ventilation; but, in the former part, I believe I have given satisfactory proof of the "exact *necessity*" of the quantity advocated. The Report of the Committee "on Cubic Space of Metropolitan Workhouses," is a document of great importance, and will, in all probability, be frequently referred to hereafter in discussions on the subject. It is therefore to be regretted that—with the evidence before them, as printed in the appendix—their recommendations, good in some points, were not more satisfactory and decided. They appear to have been too much inclined to consider the whole as a matter of opinion, whilst there was, to my thinking, ample evidence for proceeding on a foundation of facts. They would certainly have been justified in making an authoritative statement, which would have served hereafter as a trustworthy guide to practical men, on the following fundamental points:—

1. A minimum cubic space for buildings permanently occupied.
2. A fixed standard of purity (say ·06 per cent. of carbonic acid).

3. The definite amount of air required to keep the space at the required standard of purity (*e.g.*, 3000 cubic feet per head, per hour, for ·06 per cent.)

Instead of this, a great opportunity was lost, particular cases were provided for, and the great questions of ventilation and cubic space were left as vague and unsettled as ever.

One objectionable thing in the Report is the expression made use of, "on the average," and to this Dr Edward Smith very properly demurs, remarking that the *average* contains the *less* as well as the *greater*. The quantity ought in each case to have been laid down as an *absolute minimum*, let the average be what it might.

There is a point which seems to have influenced the Committee considerably, and which is specially referred to in Dr Markham's paper, viz., the want of evidence of the bad effects of overcrowding in the replies on the subject obtained from the medical officers of the different workhouses. Dr Markham appears to accept this as positive evidence of the absence of such bad effects. I hope I shall not be considered as casting a slur on my confrères if I decline to coincide with Dr Markham, and accept it only as evidence of the bad effects *not having been observed*, which is a very different thing. We all know that many things escape us unless our attention has been specially called to them, and it may have been so in the cases in question.

I will now point out how far I think we are warranted in coming to definite conclusions on this important subject.

1. We cannot safely accept a lower standard of purity than ·06 per cent. of carbonic acid.

2. We cannot safely legislate for anything short of uniform diffusion in an air-space.

3. Uniform diffusion being supposed, we cannot preserve our minimum standard of purity with a less delivery of fresh air than 3000 cubic feet per head, per hour.

4. We cannot safely change the air, on an average, oftener than *six* times in an hour without producing draughts.

5. With ordinary means of ventilation (artificial, involving current expense, being out of the question), we can seldom hope to succeed in changing the air even *six* times in an hour.

6. We must provide an air-space which will admit of the delivery of 3000 cubic feet per head, per hour, and at the same time preclude the necessity of changing the whole air so often as *six* times per hour.

7. To fulfil the above conditions, a minimum of 1000 cubic feet per head is absolutely necessary.

8. To provide the supply of 3000 cubic feet per head, per hour, so that the velocity of the current at the point of entry shall not exceed 5 feet per second, 48 square inches per head of total inlet and outlet area ought to be provided, and this independent of the chimney, if there be an open fireplace.

Part Second.

REVIEWS.

On Epilepsy, Hysteria, and Ataxy. Three Lectures by JULIUS ALTHAUS, M.D., Member of the Royal College of Physicians; Physician to the London Infirmary for Epilepsy and Paralysis, etc. 8vo, pp. 126. London: Churchill and Sons: 1866.

Hysteria: Remote Causes of Disease in General; Treatment of Disease by Tonic Agency; Local or Surgical Forms of Hysteria, etc. Six Lectures delivered to the Students of St Bartholomew's Hospital, 1866. By F. C. SKEY, F.R.S., Late Pres. and Memb. of Council and Court of Exam. of Royal College of Surgeons; Consulting Surgeon to St Bartholomew's and Great Northern Hospitals, etc. Crown 8vo, pp. 107. London: Longmans: 1867.

THE purpose of Dr Althaus's Lectures on Epilepsy, Hysteria, and Ataxy is to inculcate the author's views on certain points of the pathology and treatment of these diseases, in regard to which he is of opinion that considerable misconception has prevailed. Each lecture is a short sketch or essay on its own subject, and, as might have been expected from Dr Althaus's previous writings, contains the result of much reflection and experience. The statements made are eminently suggestive, and, what is especially remarkable, is the hopeful tone in which the author speaks in regard to the curability of the more obstinate forms of these usually intractable affections.

The first lecture, on the prognosis of epilepsy, is the most important. Its aim is to prove that epilepsy is a curable disease, and it is to be observed that in making this statement, the author refers only to the true, centric, or idiopathic epilepsy, which is usually considered least amenable to treatment. The reason for omitting the eccentric forms of the disease, in which prognosis is generally considered more favourable, is that the author regards them as essentially different from true epilepsy. Symptomatic convulsions are also excluded for the same reason. Confining his remarks, therefore, to idiopathic epilepsy, the author's main argument for its curability is derived from its pathological nature,—a basis of reasoning which he thinks is at present too much neglected in favour of the mere results of experience. In accordance with recent views, Dr Althaus regards idiopathic epilepsy as consisting in a state of undue excitability of the medulla oblongata, and an irritation of the vaso-motor nerves of the encephalon. The sequence of morbid phenomena is thus explained. The tonic contraction of the cerebral arteries is presumed to cause the unconsciousness and the tetanic rigidity of the muscles with which an attack begins; the succeeding clonic convulsions of the body are ascribed to the clonic contractions of the cerebral vessels; and the state of exhaustion and prostration in which a fit ends corresponds to the paralysis of the arterial coats, which is the consequence of their previous spasm. The various lesions of structure found in the nervous centres in cases of epilepsy, the author regards as complications, foreign to, or at least not essential in true epilepsy, and he accordingly concludes that "idiopathic epilepsy is a functional disorder due to certain finer changes in the nutrition of the brain imperceptible to our senses." On the conclusion, thus arrived at, as to its essentially functional nature, Dr Althaus founds his conviction of the curable nature of the disease. So far as these theoretical considerations go, we believe that little difference of opinion will be found to exist among medical men acquainted with the actual state of medical science. That, in a large majority of cases of centric epilepsy, no lesion is to be discovered after death, no one will deny; and the pathological doctrine, which ascribes the occurrence of the paroxysm to constriction of the cerebral arteries, has also for some time past been generally allowed. What the author does not explain, however, is the cause which induces this inordinate excitability of the medulla oblongata and spasm of the bloodvessels in the nerve centres. In this respect, we are of opinion, that, in order to arrive at a sound pathology of even idiopathic epilepsy, the eccentric and symptomatic forms of the disease ought not to be omitted from consideration. We are inclined to think that Dr Althaus undervalues the influence of peripheral causes in the production of disorder of the nervous centres. But, at the same time, we willingly admit that the present pathological views of the nature of epilepsy afford a strong *à priori* probability in favour of its curability, and that any want of success of present therapeutical methods should not be allowed to check practical endeavours, or obliterate the hopeful anticipations which are legitimately founded upon scientific pathology. It should not be forgotten, also, as evidence of curability, that a small proportion of epileptics spontaneously recover.

Admitting with the author the theoretical probability of cure, we naturally look for results confirmatory of it. And we regret that Dr Althaus has not more specifically informed us how far his experience bears out the hopeful prospects which he founds on the functional nature of the disease. Until experience endorses a favourable prognosis, medical practitioners must necessarily maintain their present reserve. It is cruel to give rise to expectations which the issue does not justify. We presume, however, that the author has met with some amount of encouraging success. One case which he relates is equally remarkable for its severity and for its speedy recovery. His remarks on treatment are well worthy of attention. He attaches great value to tincture of henbane in large doses for the suppression of epileptic auræ and paroxysms. Perseverance in treatment, and the necessity of securing a devoted attendant, are particularly insisted upon, in addition to the appropriate remedies to strengthen and modify the nerve centres, and improve the general health.

In the lecture on hysteria, Dr Althaus dwells particularly on the emotional character of the disease, and discards the notion of its dependence on uterine disorder. We cannot, however, at present follow the author in his observations on the nature and treatment of hysteria, but must content ourselves with recommending this lecture, as well as the concluding one on locomotor ataxy, which contains a succinct account of what is known of that disease, to the attention of our readers.

Mr Skey's lectures are Brunonian both in tone and subject-matter. They consist essentially in a laudation of the tonic, or we should rather say the stimulant method in the treatment of disease. Port-wine and bark are the remedies which he advocates; and it is curious to note what he says of the change in this respect which, within his experience, has taken place in the practice of London hospitals.

> "During the last forty years the treatment of disease has undergone great and important changes, and up to a later date the use of wine was very exceptional. I do not overstate the case when I affirm that within the period I have mentioned the consumption of wine and brandy in the London hospitals has increased at least fourfold; and I may here relate an anecdote which confirms the above statement. In the year 1848 the treasurer of the hospital commented on the quantity of wine I ordered for my patients. He said the hospital could not warrant the large expense. I inquired the number of patients—550; and the wine consumed was three pipes per annum. I told the worthy treasurer that the consumption of wine quite surprised me; that I could not understand how my colleagues could manage their cases, for that I could not treat hospital cases without wine; and I assured the treasurer I would do my best, for the credit of the hospital, to raise the consumption of port-wine from three pipes a-year to thirteen, and that nothing less would satisfy me, or my conviction. Twelve years elapsed, when I was again addressed by the successor of the then treasurer on some matters connected with the diet scale of the hospital. On inquiry into the consumption of port-wine, he appealed to the apothecary, who, referring to his wine-book, announced the quantity annually consumed to be thirteen pipes! I was on the previous occasion the great delinquent, but now, as the treasurer declared, 'You are all nearly equally bad, although *you* still head the list.' And the same change in the treatment prevails more or less in every hospital in London."

In this particular, however, the London hospitals form no exception. In most, if not all those in Great Britain, the treasurers have, no doubt, been in the habit of objecting to the expense of the wine roll.

For the purpose of enforcing his views, Mr Skey divides all diseases into true disease attended with structural changes, and unreal disease, which is the mere shadow or semblance of the former.

True disease he regards as an affection of the vascular system, and inflammation is the principal form of it; simulated disease he ascribes to the functional disorder of the nervous system, or, in one word, hysteria. It would, perhaps, be unfair to criticise strictly this pathology, which is probably meant to apply only to the subject of the lectures, viz., hysterical as distinguished from inflammatory or organic disease. At the same time, as addressed to students, such sweeping statements are very liable to create erroneous impressions. Inflammation is not simply a disease of the vascular system, nor is functional disorder synonymous with hysteria any more than bark and wine are a panacea for all diseases where bloodletting is not admissible.

In his observations on hysteria, Mr Skey essentially agrees with the views so well laid down by Sir Benjamin Brodie, to whose memory these lectures are dedicated. The only points of more recent interest relate to the question at present exciting some degree of public attention as to the ultimate effects of railway accidents. What Mr Skey's experience on this subject has been will be apparent from the following among other similar passages. Speaking of hysterical symptoms, he says,—

> "Such is the nature of the large proportion of cases of persons who come into courts of law for compensation for injuries erroneously deemed to be permanent, bringing with them headaches, spinal pains, tingling of the extremities, impaired vision, loss of memory, and many other symptoms of an unstrung nervous system—a series of grievances of the incurable nature of which an acute lawyer takes care to provide himself with ample testimony, and which will always be obtained so long as the diseases of the vascular system and their consequences monopolize a too prominent share of the attention of our profession. I have traced several of these persons in their after-career, the large majority of whom entirely recover. I believe it is to the prevalence of error in the early management of these persons, who are almost invariably subjected to depletive treatment, and to the imperfect knowledge of nervous diseases which prevails in the profession, that large sums are awarded for injuries erroneously supposed to be permanent and incurable."

And again,—

> "One of such cases I will give you as an example:—A man without property or profession brought an action against a railway company for injury to his spine. This statement, on the face of it, is an absurdity. How can a man without property bring an action at law? Well, he applies to a lawyer, who undertakes the case on his behalf, with a certain compact and understanding as to the question of future payment! Thus the lawyer becomes the plaintiff, and the plaintiff the witness in his own case. The man's injury was made out to the entire satisfaction of the jury, and very heavy damages were awarded by them, coupled with severe comments on the negligence of the railway directors.
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> "It was positively known, at the time, by several persons engaged in the action, among whom was a detective officer, that within a few days of the trial the plaintiff, or the witness, whichever you please to term him, *had walked a race* against another man! Yet this man was declared on authority to have sustained a permanent injury of his spinal column!

As the results of observation of an eminent surgeon of large experience, these lectures afford additional testimony in favour of the tonic system of treatment, which, since the days of Brown, has been progressively taking the place of the older depletory methods. And they may be of service in certain quarters where venesection and mercury still linger as the daily weapons of a dangerous routine practice. But we do not agree with Mr Skey when he alleges that the actual teaching in the schools, as a rule, is opposed to the tonic line of practice; and we imagine that most practitioners are sufficiently familiar with the use of stimulants in medicine and surgery,—and we may also add with their abuse, a danger to which Mr Skey does not seem to be sufficiently alive.

Traité expérimental et clinique de la Régénération des Os, et de la Production artificielle du Tissu osseux. Par L. OLLIER, Chirurgien-en-chef de l'Hôtel Dieu de Lyon. Avec 9 planches gravées sur cuivre, et 45 figures intercalées dans le texte. Paris: Victor Masson et fils: 1867.

Experimental and Clinical Treatise on the Reproduction of Bone and the Artificial Formation of Osseous Tissue. BY L. OLLIER, Chief-Surgeon to the Lyons Hospital. With 9 copperplates and 45 woodcuts. Victor Masson & Son. Paris: 1867.

[FIRST NOTICE.]

To do anything like justice to a work of such originality and importance, as this monument of experiment and observation really is, a pretty full analysis of its contents is required, though careful study both of the results of experiments and the criticisms and applications of these results will be needed by any one who wishes thoroughly to appreciate the value of M. Ollier's great labours.

The work is divided into two volumes; the first is experimental, and analytic, being an account of experiments made by many surgeons in different ages, and chiefly those of the author himself on the subject of regeneration of bone; the second is synthetic, and clinical, describing the practical value and possible application of the results of experiment, and detailing numerous cases in which these results were obtained in man.

An introduction of sixty-one pages is devoted to an elaborate defence of the experimental method of research, and an inquiry how far experiments on animals are to be trusted, as explaining similar phenomena, and warranting similar treatment on man. After telling us that "the nature of the different lesions can only be determined by the study of the mode of development of bone, normal osteogeny giving us the key to the production of most of the alterations by which the tissues of bone may be attacked," M. Ollier refers to the many difficulties by which such experiments are surrounded. These are numerous. *First*, The very fact that we must inflict a wound in our experiment—not wait for the onset of disease—makes it difficult to separate the effects of the wound from the reparative efforts of nature; again, the differences in animals, and their varied nearness or distance from man in the animal kingdom; the variations in the age, bodily vigour, and hygienic condition of the animals experimented on, are all of great importance, and have introduced many fallacies. The enormous length of time required for certain of the experiments—one, two, or more years, and the necessity of repetition for their verification, is also an element of difficulty and delay. The author then gives a history of the theories regarding the regeneration of bone, and experiments on the osseous system. This is divided into two periods or eras, which meet at Duhamel's researches.

A. Before Duhamel: period of pure observation. In this there are few details of cases, and *no* experiments properly so called, if we except a paper by Antonio de Heyde, in 1684, on the formation of the callus in frogs, to which M. Ollier alludes in a foot-note. Hippocrates, Galen, Celsus, down to Parè and Scultetus, had done nothing more than observe cases in which bone had been regenerated, and record observations on them, but made no experiments. In one most interesting case, which is detailed in a foot-note, Scultetus seems to have been on the eve of discovering the practical value of the periosteum. Delamotte, in 1771, published a case in his large Treatise on Surgery, in which he had performed with success a true subperiosteal resection.

B. Period of experiment. The opinions of Duhamel, Fourgeroux, Haller, Dethleef, and Bordenave are briefly discussed, and their experiments contrasted. It is exceedingly interesting to find how age after age controversies repeat themselves. Duhamel held that the periosteum was the tissue which was converted into, and could reproduce bone; Haller and Bordenave told of osseous juice, fluid oozing from the vessels or from the old bone; Troja speaks of "la gelée." A century later, with our improved microscopes and altered nomenclature, we have still the school of the exudate and the blastema waging war with the cellular pathologists who believe in the proliferation of nuclei, the cells of the connective tissue, and the ossific layers of the periosteum; the names of Haller and Duhamel are now changed into Rokitansky and Bennett, Virchow and Goodsir. We are next introduced to the remarkable experiments and theories of Troja, who held that, by destroying the medulla in cases of disease, the old bone might be destroyed and extracted, a new shell having been formed over it. The papers of Coutavoz (1752), David (1770), and Vigarous (1778) are quoted to show the progress made in France in the appreciation of the powers of the periosteum. Vigarous especially seems to have been remarkably in advance of his time; and our author claims for him priority by one year over White of Manchester in excising the head of the humerus. The opinions of Scarpa and Bichat, the former undervaluing the periosteum and limiting its function simply to that of acting as a mould or limiting membrane, are then briefly described, with the attempt by Dupuytren to revive the theories of Duhamel, and that of Cruveilhier by an eclectic theory to unite the most opposite views. After this, experimenters for a time seem rather to increase in number, but decline in importance till, in 1830, Bernhard Heine of Wurzburg commenced his experiments, so celebrated in Germany. He gave the periosteum the first place in the repair of fracture. After this, well-known names multiply. Syme, Stanley, Jobert, Malgaigne and Velpeau all contributed; and in 1847, Flourens published his important work, "Theorie experimentale de la formation des Os," of which the following six propositions give the leading conclusions, and may serve to indicate the *status quaestionis* at that date.

1. Bone forms itself in the periosteum.
2. It increases in thickness by layers laid one upon the other.
3. It increases in length by layers laid side by side.
4. The medullary canal increases by re-absorption of the inner layers of the bone.
5. The heads are successively laid down and re-absorbed, to be again laid down as the bone grows.
6. The continual change of the material is the great and marvellous secret of the development of bone.

In summing up this most able and learned introduction, M. Ollier invokes the aid of experiments conducted on better plans than those of his predecessors to prove his points, and says that his adversaries, who must be convinced, are the very same ones who had to be fought with when this century began.

1. Those who believe that the periosteum is useless, and that regeneration of bone can take place without its aid.
2. Those who deny that regeneration of bone can take place even with the aid of the periosteum.

The second or experimental division of the work commences by stating in a single paragraph the constituents of bone, periosteum, marrow, and osseous substance, to which is to be added cartilage, which, however, is in some bones only a temporary developmental constituent. Each of these must be studied separately to ascertain its relative importance in normal and accidental ossification.

1. The periosteum and its *rôle* in ossification are discussed in various series of experiments, of which we have room only for a brief account. Ollier has succeeded in forming artificial bone by the displacement and even transplantation of periosteum. Thus, A: In this series he cut an oblong flap of periosteum, perhaps two inches long and half-an-inch broad, raised it from the subjacent bone (generally the subcutaneous surface of the tibia), and, leaving it attached by its upper end only, twisted it across the muscles so as to bring it nearly or altogether into the form of a ring surrounding

the limb, and secured it in its new position with a stitch. In six or seven weeks the flap was changed into bone, thick, strong, and exhibiting the microscopic characters of true bone. These experiments were made frequently on rabbits, cats, and dogs.

B. In this second series, the flap was raised and fixed as above, but the pedicle of connexion was cut in three or four days, still the bone developed in the flap as before.

C. This is a still more remarkable series: flaps of periosteum were at once raised and transplanted to different parts of the same or different animals, or even animals of a different genus. In almost every case, where the animal was of the same genus, bone formed in considerable quantity in the new position.

The osteogenetic power of the periosteum being thus thoroughly proved, the author next proceeds to inquire in what part or layer of the periosteum this power resides. Though the scalpel cannot define two distinct layers, the microscope shows that the periosteum really does differ in structure in its deep and superficial aspects. Examining the periosteum from without inwards, we find—

1. A thin layer of connective tissue containing fat cells; this hardly belongs to the periosteum at all, but is rather the result of the imperfect separation of the neighbouring cellular tissue. We next come upon a fibrous tissue, closely compacted, formed by small corpuscles of connective tissue united by an intercellular substance in the form of fine wavy fibres, and containing numerous elastic fibres.

2. Turning the preparation and examining it from its inner surface, we find elements entirely differing in appearance, namely, oval or spindle-shaped cells, enclosed in a granular or fibroid intercellular substance. Most of these cells have only one nucleus, but a certain number are in a state of proliferation; those nearest the bone resembling the cells of the marrow with their numerous nuclei.

It must be admitted that the elements of the deep layer, although of similar nature, are at a period of evolution quite different from those of the superficial one. This deeper layer has been described by various histologists,—it is the blastema of ossification of Kölliker, the proliferating layer of the periosteum of Virchow.

The above is a brief resumé of the author's interesting account of the structure of the periosteum. His experiments on the functions of the different layers may be merely noticed.

1. By scraping this inner layer from the outer for a portion of the length of one of his periosteal flaps (left attached at one end and twisted round the muscles), he found, after fourteen days, that the portion which was left unscraped had been ossified as usual, while the scraped portion *was only fibrous*, though still alive and traversed by vessels extending from the new bone to the old. In other experiments he destroyed the inner layer by cauteries, both actual and potential, and found that, notwithstanding the thinness of the layer left, it still continued to live as a fibrous membrane, but it had quite lost the power of forming bone.

2. Not satisfied by these indirect or negative proofs of the bone-forming power of the inner layer of the periosteum, the following positive experiment was made. The inner layer was scraped from a periosteal flap raised from the tibia of a young rabbit; it was obtained as a whitish semi-liquid matter, containing certain small fragments of membrane. This was sown or planted underneath the skin of the animal's groin. Three weeks afterwards, numerous little solid grains were found in the groin, the largest about the size of a pin's head. Under the microscope some of those were found to be merely calcified, but in others there existed distinct evidence that true bone had been formed.

The author's conclusions up to this point are given in the following sentence:—

> "If the periosteum possesses the power of producing bone, it owes this power to its deep layer. This deep layer is the immediate agent of ossification; if the periosteum is deprived of it, it loses its power of ossification. It may, however, perhaps regain at length a portion of its activity, but will never return to its original condition."—P. 94.

A series of experiments are next given, which prove that other fibrous tissues, such as tendons, may possibly calcify, but are not capable of real ossification.

The mode of development and structure of the new bones which the periosteum forms is then described and illustrated, at the end of which M. Ollier, in a few trenchant sentences, expresses his conversion from the blastema theory to cellular pathology.

> "We have now recognised that that which we had taken for a new product, an exudation, a blastema, is nothing else than the normal osteogenetic layer, the deep layer of the periosteum, the primitive formative elements of which have become greatly multiplied. . . . The periosteum does not form bone as the liver secretes bile; one cannot make the least comparison between the two phenomena. At the epoch when an osseous juice or fluid, passing successively through various stages of development, was admitted, the confusion was possible; it is so no longer."—P. 110.

Chapter II. discusses the marrow from the same point of view as the periosteum has been discussed. *First*, In a brief history of previous experiments and observations, the few regarding its anatomy, and especially regarding the existence of a true medullary membrane. M. Ollier doubts the existence of the latter, except in the hollow bones of birds. *Second*, In reference to its function, he holds that it is one of absorption, not of growth; that the marrow-cells tend, by absorbing the inner lamella of the bone, to increase the size of its *lumen*, as the periosteum, by adding layer after layer, tends to increase its thickness. He thus objects most strongly to the name, "*internal periosteum*," as applied to the marrow,—first, as *inadmissible anatomically*, as we do not admit the existence of a medullary membrane at all; second, as *physiologically vicious*, on account of the great difference between them in function. *Thirdly*, Experiments prove that the transplanted marrow does *not* ossify. If a limb be amputated, and a metallic tube be pushed up the hollow of the bone so as to contain within it the marrow, and separate it from the bone, in some rare cases the experimenter has succeeded in obtaining a slight ossification. This may appear to militate against the results of the preceding experiments, but ossification so seldom occurs, and, notwithstanding the great degree of irritation present, it is really so much less in amount than the periosteal ossification, that M. Ollier does not lay much stress on its occasional occurrence. In one of his brief axiomatic sentences, he says,—"*The ossification of the periosteum is a normal fact, that of the medulla an accidental one.*" "Médullisation," which is the name given by Ollier to the process of formation of a medullary canal in a new bony formation, or in the callus of a fracture, is fully described in a very interesting section. The final stage of medullisation in mammals is conversion into fat, in birds into a hollow cavity containing air. The last section of this chapter contains some experiments (necessarily cruel ones) on the sensibility of the medulla, its vascularity, and its power of absorbing liquids injected into the canal.

Chapter III. is devoted to the bone itself, its structure, and properties. The description of its structure differs little from that given by Virchow in his Cellular Pathology, from which M. Ollier quotes. He experiments,—1. By removing the periosteum. If the wound heals by first intention, it is found that the periosteum is soon reproduced, partly from the periosteum at the edge, partly from the very surface of the bone itself. As might have been expected, if the superficial layer of the bone at the denuded part is destroyed by scraping it, the reproduction of the periosteum is very considerably delayed. But if the wound suppurates, it is generally found that partial or complete necrosis of the denuded portion results. This delays the reproduction and deteriorates the character of the periosteum, which eventually fills up the gap left by the separation of the sequestrum. 2. By destroying and removing the marrow. This operation, which is borne well by birds, is very dangerous to mammals, as might have been expected from the well-known great danger in man of osteo-myelitis. When the animal does survive the operation, the marrow is very rapidly reproduced, and no necrosis need result; indeed, even after total destruction of both periosteum and marrow of a portion of a long bone, necrosis does not necessarily result, the bone being nourished by its attached extremities. In such an injury in man, necrosis would be inevitable.

Chapter IV. describes cartilage, and its rôle in ossification. Several experiments on the transplantation of cartilage gave different results, according to the particular cartilage, the relation of its perichondrium, and its new site; as a general rule, however, very little ossification resulted, and in many of the experiments the cartilage was rapidly absorbed.

Chapter V. seems to us a most valuable one; it records comparatively few experiments, but some most striking observations, and is throughout full of the most philosophic generalization. It discusses the effects of traumatic irritation on the different parts of the bone and on the adjacent tissues. We can here only briefly detail the chief heads of the observations. 1. Irritated periosteum forms new bone even in aged animals, where the natural bone-forming process of the periosteum had ceased. 2. Irritated marrow, though exceedingly apt to pass into suppuration, sometimes fulfils its rôle of medullisation, absorbing layer after layer of the bone. In young animals, M. Ollier succeeded occasionally in obtaining complete reabsorption of the bone by irritation of the marrow, so that only marrow and pus remained enclosed in an external envelope of new bone, which in the meantime the periosteum had been forming. Such experiments explain the not uncommon cases of almost complete reabsorption of inflamed phalanges; and also the peculiar soft condition of the long bones so often seen in the neighbourhood of carious joints, even where there has been no suppuration. The same amount of injury seems to produce very different effects in different animals, according to their state of health and kind of constitution. What will cause suppuration in one, will produce only a thickening of the periosteum in another, or a layer of new bone in a third.

The neighbourhood of the periosteum is then shown to have an effect in determining the results of irritation in other tissues, such as the fibrous or synovial, and this explains the new formations in and about joints after irritation, whether traumatic or rheumatic. A most interesting section follows, which traces the relation between ostitis and necrosis. Ollier proves that mere destruction of the nutrient vessels of a portion of bone, by isolating it simultaneously from the periosteum and marrow, need not result in necrosis, so long as the bone does not inflame. Inflammation of the bone at once obliterates the capillaries, for, enclosed in narrow canals which are incapable of dilatation, the least inflammatory thickening presses on the capillaries, cuts off the supply of blood, and then, and not till then, the bone dies. This it is that best explains the far greater frequency of necrosis from similar injuries in adults than in children. For an interesting discussion of the various opinions on the possibility of absorption of dead bone, we must simply refer to the rest of this most valuable chapter (pp. 192–199).

Chapter VI. is on the repair of wounds of bone, and the formation of callus. A large number of experiments on wounds give results so thoroughly consistent with the foregoing chapters as to require no detail. The subject of reunion of fracture, so much discussed, and enveloped in such a mist of words, seems to M. Ollier and those who follow him to be not so difficult after all. He briefly describes certain varieties depending on the nature of the injury. 1. Subcutaneous fractures with or without tearing of the periosteum.

Subperiosteal fracture is not uncommon, especially in the clavicle and tibia, when the injury has been direct, and there has been little or no displacement. There is little irritation, a prolonged spindle-shaped swelling occupies the neighbourhood of the fracture, and the periosteum is the chief agent in repair. 2. Where the periosteum has been ruptured and the fragments project through it, the flap or bridge which remains connecting the two ends plays a most important part; it ossifies, joins the ends, and by degrees the corners of the shaft which remain connected to this bridge are gradually rounded off and worn away by the efforts of the marrow, and the continuity and shape of the limb restored. 3. The marrow has its work too, chiefly in old subjects, in ossifying after a fracture and forming a bolt or pin which aids in fixing the ends. This is speedily reabsorbed in young animals or children, in adults it may remain unchanged. The work of the soft parts and the specialties of compound fractures are then noticed; and all is summed up in a section entitled "The Theory of Callus," which seems a very fair exposition of the present state of the question. A curious experiment gave the most natural results. M. Ollier deprived the tibia of a rabbit of its periosteum, and allowed the place to heal. Three weeks after, he fractured it at the same spot. The fracture reunited, but very much slower than it should have done. Certain results of experiments on fractures can here only be noticed. Continued irritation by rubbing, pricking, inserting setons, etc., seems to delay union and even cause absorption of the callus. Weak health retards—gestation seems not to affect—and phosphated salts do not hasten—the union of a fracture. Section of the nerves of the limb seems to have little or no effect on the repair. A notice of wounds of cartilage and their mode of union concludes the chapter.

Chapter VII. treats of the regeneration of bone in general. The chief experiments on this subject were made on the radius of young rabbits. There were four series: briefly, the results were as follows:—1. The whole of the periosteum preserved, and bone removed, resulted in complete restoration of the bone. 2. Removal of the bone with most of the periosteum, but leaving here and there fragments of periosteum, resulted in bony fragments corresponding exactly to the portions left. 3. Removal of the periosteum and bone entire, leaving carefully the cellular layer above the periosteum, and cutting the tendons as close to the bone as possible, resulted in a band of fibrous tissue. 4. Removal of the periosteum and also the neighbouring soft parts, including a layer, more or less thick, of the tendons, resulted in no reproduction of the bone at all. These all coincide in showing the immense importance of the periosteum, and the only exceptional powers of the soft parts. A series of similar experiments on the powers of the marrow in regeneration give almost negative results.

Chapter VIII. gives some most curious and surprising results of experiment on the reproduction of different bones after their complete removal. We have no room for a resumé of the various experiments; suffice it to say, that in young dogs Ollier removed the whole humerus in one, the whole scapula in another, the vault of the palate in a third, with complete reproduction of new bones with all the attachments of muscles complete, in periods varying from one to five months. Less remarkable at first sight, but still very interesting, are similar experiments on the cranium, by trepanning, and on short bones by excision.

Chapter IX. opens a new and very important practical question, which, more even than the preceding chapters, has a bearing on many most frequent surgical operations. It discusses the "Formation of new joints between the reproduced articular extremities," or, in other words, the formation of false joints after resections. M. Ollier naturally strongly supports the preservation of the periosteum and the capsular ligaments, as far as this may be done, in resections. He thus contrasts the results of the two methods.—1. In resections in which the capsular ligament and periosteum have been preserved, we have "reproduction of the heads of the bones, preservation of the joint, and separation of the extremities, or else a union by a mere cellular tissue, in which may be traced some of the characters of a synovial membrane, occasionally even an attempt at an inter-articular fibro-cartilage may result." 2. In ordinary resections where the periosteum and capsular ligament have been removed, "the heads of the bones are not reproduced, no joint remains, the ends of the bones are united by a fibrous band varying in thickness."

We must own that our experience of resections, specially of the elbow-joint, in Edinburgh, does not bear out M. Ollier's results. We find that the chief risk is not that too much be removed, but too little, and that it is quite unnecessary to leave the periosteum. Many cases have shown the admirably useful joints which may be obtained, and one or two dissections have proved the nearly perfect reproduction of joints in which no care was taken to retain periosteum, and in which not only all the synovial membrane, but the ends of the bones were very freely removed.

Some of M. Ollier's experiments on animals bear out his view, that so far a better result can be obtained *in them* when the periosteum is retained than when it is removed. For in young dogs, when he removed the whole articulation, the elbow remained useless; the fore-arm, at the end even of six months, remained atrophied and flapping; while, when he retained the capsule, the ligaments, and periosteum, removing only the bones, he obtained joints which, while strong to resist lateral movement, were able to be flexed and extended.

Chapter X. briefly groups the results of experiment on the mode of development, and the structure of reproduced bones and of new joints.

Chapter XI. details the conditions of age, method of operating, and general hygiene, which are needed to ensure success in the experiments; and also describes a few of the artificial methods which may be used to stimulate and increase reproduction of new bone.

Chapter XII. is of very great value in a physiological point of view, as it enters very fully into the interesting question of the manner in which bone grows, and especially into the laws which govern the growth of the long bones of the limbs. The author sums up in the following brief sentence the broad differences which exist between the growth of soft parts and that of bones:—

> "Growth is in fact quite different in the soft tissues from what it is in bone. In the former it is interstitial—in other words, it acts by the interposition of new anatomical elements *between* the ones already existing. In the latter, on the other hand, growth takes place by the deposition of new elements—the osteal cells, fixed at regular distances by the want of extensile power in the calcareous substance, cannot separate to give place to new elements, which must arrange themselves either inside or outside of the layer already formed. We may contrast the growth of bone with that of the soft parts, by saying in a single word, that the former is peripheral, the latter interstitial."

The well-known experiments of Duhamel, Flourens, and others, of obtaining variously coloured layers, by including or withholding madder from the food of animals, are then referred to; and then the more accurate ones, by the insertion of nails, are recorded and repeated.

The latter kind were thus performed. Two small nails were inserted into the tibia of a rabbit ten days old, at a certain measured distance from each other. Ten months afterwards, on the animal being killed, the bone was found at least three times its former length, and yet the nails preserved the same original relation to each other, though the epiphyses had separated so far. The next difficulty is to explain the changes in form which bones undergo during their growth—the jaw of an adult being not merely the enlarged jaw of a child, but having a different shape. For this purpose M. Ollier cites and adopts John Hunter's term of "modelling absorption" for want of a better term, but explains that the changes in form are the result not only of absorption of bone at one place, but also of accumulation of new bone at another part.

The remarkable facts regarding the inequality of the increase of different ends of the long bones of the limbs lately discussed by Dr Humphrey of Cambridge (Med.-Chir. Transactions, vol. xliv.) are confirmed by M. Ollier. These variations have an important practical bearing on the subject of resections on infants, and are well worth the careful study of surgeons. Experiments, and even conclusions, are far too long to give here; but we may quote the two propositions with which the section closes:—

1. In the upper limb the extremities of the arm and fore-arm, which together form the elbow-joint, grow *less* than the others do.

2. In the lower limb the extremities of the thigh and leg, which together form the knee-joint, grow *more* than the others do.

This explains, what every practical surgeon must have observed, why excisions of the elbow in young children are much less likely to be followed by arrested development of the limb than those of the knee, while resections of the shoulder and wrist are more likely to cause arrest than those of hip and ankle.

It also explains in a most interesting, and, we believe, thoroughly scientific manner, the frequency of malignant tumours of the lower end of the thigh and head of the tibia, compared with the rarity of such near the elbow. It also bears a close relation to the specialties in the development of rickets.

Chapter XIII. describes experiments on the influence of irritation and removal of different portions of bones on their subsequent growth, and on the effect of dividing nerves.

Chapter XIV. recounts some curious experiments made on the possibility of grafting bones (*a*) by *transplantation of the periosteum*:—

1. From one part of the same animal to another.

2. From an animal of one species to one of another. Out of sixty experiments only one succeeded, and that partially.

3. From dead to living animals. Some experiments succeeded twenty-four hours after the death of the animal.

And (*b*) *by actual transplantation of bones*, or pieces of bones, still surrounded by periosteum, and enclosing marrow.

Our analysis of this first volume has already run to such a length that we can quote no more of these very curious experiments, but must simply refer the reader to the original. For the present we have left ourselves no room for criticism. Indeed, so far, there is little need for it; the experiments are so numerous and precise, the results tally so well with each other, and leave the impression of so harmonious a whole, that really, unless by a similar series of experiments, with different results, M. Ollier's facts could be controverted, there would be little use assaulting his *theories*. And even were this done and negative results obtained, it would still remain for the dexterous French surgeon to say, "Your results are not like mine, because you have not proved yourself so neat-handed an experimenter, nor so close an observer."

We reserve for another occasion a notice of the second volume of this monument of industry.

Chemistry, Inorganic and Organic. By Prof. Bloxam. London: Churchill and Sons: 1867.

This is a Manual of Chemistry written on the old plan, and although exception will no doubt be taken to it by advanced theorists on account of its antiquated notation and want of systematic arrangement, every one who takes the trouble to read it will admit that it is an excellent book. The science is at present in a transition state, and chemists are apt to forget that there is a large class who take no interest in our theoretical discussions, but who are practically interested in chemistry. For this class of readers the book before us is specially suitable. A large proportion of it is devoted to the applications of chemistry, and metallurgical and manufacturing processes are fully and clearly explained. In his short theoretical remarks the author has, on the whole, very fairly represented the views now generally held; the only exception we have observed is in his notice of the doctrine of atomicity, p. 153, where he gives peroxide of hydrogen the graphic formula $H—O''—H—O''$ (instead of $H—O''—O''—H$), thus making one atom of hydrogen diatomic. An error of this kind is not of much consequence in a book so purely practical as this, still, if the subject was to be alluded to at all, the author might have taken the trouble to make the allusion accurate.

The book is carefully printed and is illustrated with a large number of new and instructive woodcuts. We can cordially recommend it to all who prefer what is generally called the "old system of notation," and believe that even the more "modern" chemists would not find the time spent in its perusal altogether thrown away.

Erkennung und Behandlung der Prostata-Krankheiten. Von HENRY THOMPSON, Artz am Londoner Universitats-Hospitale, so wie in St Marylebone, Mitglied der Société de Chirurgie in Paris, etc. Autorisirte deutsche Ausgabe, mit 25 in den Text gedrucken Holzschnitten und 2 Farbendrucktafeln. Erlangen: verlag von Ferdinand Enke: 1867.

Recognition and Treatment of Diseases of the Prostate. By HENRY THOMPSON, Surgeon in University College Hospital, and to the Marylebone Infirmary, Fellow of the Société de Chirurgie in Paris, etc. Authorized German edition, with 25 woodcuts in the text, and 2 chromolithographs. Erlangen: Ferdinand Enke: 1867.

DR THEILE, the translator, expresses his sense of the great value of British contributions to the subject of Diseases of the Prostate, and implies that this may be in part due to the greater frequency of the disease in this country.

Sprengel and Keil had already done for Home and Adams what Theile has now done for Mr Thompson, whose careful and original observations well deserve the honour of a good translation. The translation seems close and sufficiently accurate; a trival alteration is made in the arrangement of the chapters; and only two of Mr Thompson's thirteen plates are reproduced, and these in a style very much inferior to that of Mr West.

Part Third.

MEETINGS OF SOCIETIES.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XLVI.—MEETING IV.

6th March 1867.—Dr MOIR, *President*, in the Chair.

I. FATTY GROWTH FROM THE INTESTINAL CANAL.

Dr Grainger Stewart, who showed the specimen, said that it had been sent for examination by Dr Davidson, of Berwick-on-Tweed. It consisted of a fatty tumour of the size of a goose-egg, imperfectly surrounded by mucous membrane. It was passed from the bowel of a lady who had enjoyed good health till the end of 1864. She then suffered from dyspepsia and considerable irritation of the intestinal canal. Towards the end of 1866, she had severe "spasms," more particularly a few hours after eating. The pain was located in the region of the ileum, but sometimes darted across the pelvis. She was recommended to take food in small quantities at a time, and sedatives of various kinds were occasionally employed. Suddenly, after severe pain, she experienced complete relief. For twelve hours afterwards she had a feeling as if hardened fæces were passing along the gut, and during that time nothing but pure mucus was passed by stool. At length, after considerable straining, the fatty tumour was voided. Since then the patient has steadily improved. Microscopic examination showed the tissues to be fatty, with numerous vessels, and an incomplete layer of mucous membrane.

II. RUPTURE OF ARTERIES AND HÆMORRHAGE INTO THE PERICARDIUM.

Dr Grainger Stewart gave the following outline of the case. A man, aged 60, was employed in Bonnington Chemical Works. It was stated that his employment was pumping sulphuric acid into a receiver containing coal, and further, that, while he was at work, fumes of muriatic acid were thrown off. He had been in general healthy until about six months before his death, when he had paralysis of one side of his face, and had only worked for three weeks in the chemical manufactory. On April 10, 1866, he had been employed from eight to nine in his usual way, and was afterwards sitting down to breakfast when he complained of a feeling of faintness and sickness, followed by rigors. He was brought to the Infirmary in the forenoon, and was put under the care of *Dr Sanders*. On admission, he was pale, and shivered occasionally, and the pulse was very feeble. In the course of the evening he vomited, and soon became dull and stupid; the hands were cold, the left leg and foot very cold; he passed blood from the bowel, and, according to the nurse, took three or four fits, with slight frothing at the mouth. About 11 P.M. he became much worse, and died before 12. A ruptured aneurism was conjectured to be the cause of death. The body was well built and moderately well nourished, the lungs were congested, their margins emphysematous. The pericardium contained 12 oz. of clotted blood, which had escaped into the cavity from a small tear in the pericardium between the pulmonary artery and the aorta. The left side of the heart was hypertrophied, the right dilated; the valves were thickened, atheromatous but competent; the aorta was dilated and atheromatous. About one-and-a-half inch from its commencement there was a transverse tear on its anterior wall about one-and-a-half inch in length through the inner coat, and a corresponding tear, but somewhat less in size, through the middle. Blood was infiltrated between the inner and middle, and between the middle and outer coats; it had passed forwards between the pulmonary artery and the aorta; the blood had burst through the visceral layer of the pericardium. The superior mesenteric artery was occluded about half an inch from its commencement by blood extravasated between its middle and outer coats; the left renal artery was similarly occluded. In the left iliac there was an extravasation about four inches in length from a small rupture about the middle of the bowel. There was atheroma of many of the smaller arteries; no disease of the brain, nor any important disease elsewhere.

III. ATROPHY OF RIGHT OPTIC NERVE IN FRONT OF THE COMMISSURE.

Dr Stewart also showed a preparation of atrophy of right optic nerve in front of the commissure from a patient who had lost the sight of the right eye in infancy. Beyond the commissure the nerves were equal.

IV. GANGRENE OF LUNGS FROM EMBOLISM, A RESULT OF THROMBOSIS OF THE FEMORAL ARTERY.

Dr Stewart exhibited the femoral vein, and part of one lung. The vein contained a decolorized thrombus which completely occluded it, and from which a coloured coagulum extended upwards to near the commencement of the cervix, and downwards to the popliteal space. In the upper part of the lung there were tubercular deposits and cavities, and in the lower part gangrenous cavities. In the arteries corresponding to the gangrenous districts embolism was seen, and in the other lung a similar condition had existed.

The patient (who had been under the care of Professor Maclagan) was a woman suffering from advanced phthisis. In the course of this affection she became greatly exhausted, and one of her legs was observed to be swollen. This was referred to thrombosis of the femoral vein. Sometime afterwards, symptoms of gangrene of the lung set in, which proved speedily fatal. Exhaustion, and perhaps pressure, caused the thrombosis, which led to embolism, and that to gangrene.

V. CANCER OF THE LIVER LOCALLY CURED.

A portion of liver was shown by *Dr Stewart*, in which were seen several large cancerous nodules completely cured, the bulk of the mass having undergone complete fatty degeneration, and become encapsuled by fibrous tissue. Notwithstanding the local cure in several places, the patient had died of cancer, complicated with ascites, a result of pressure on the portal vein.

VI. RUPTURE OF THE HEART.

Dr Stewart stated that the patient from whom the specimen was obtained had died suddenly while in a state of excitement. The heart was fatty and somewhat hypertrophied, and an aneurism existed in its left ventricle. From this aneurism the rupture had extended, and at the sectio several pounds of blood were found in the pericardium. The rupture at the pericardial surface measured about half an inch in length, and had apparently become closed before death took place.

VII. NON-MALIGNANT ULCERS OF STOMACH.

Dr Stewart exhibited two stomachs with large non-malignant ulcers. In one of them, the organ was greatly enlarged, in consequence of the ulcer having led to a constriction of the pylorus; in the other, adhesions had been formed between the stomach and liver, so that the latter organ formed to some extent the base of the ulcer. One margin of the attachment appeared to have become torn, and peritonitis had been lighted up in consequence. This peritonitis was limited by the dense adhesions, but proved fatal about three weeks after its commencement.

VIII. TUBERCULAR DISEASE OF THE TESTIS.

Dr Watson showed a specimen of tubercular disease of the right testicle, which he had removed a week previously from a patient in Chalmers' Hospital. The disease had existed for more than a year, but had latterly increased rapidly in size. The great bulk of the tumour was developed in connexion with the tubular structure of the rete testis, and had undergone a process of softening in the centre. There were smaller cheesy masses in connexion with the globus minor and body of the epididymis. The great mass of the glandular structure of the testis was aggregated towards the upper part of the tumour, and presented a perfectly normal appearance. The tumour was removed, as a bloodless operation, by the use of acupressure to the cord at the external abdominal ring, and the lateral application of cork splints to support wire sutures, whereby the tissues of the scrotum behind the bulging tumour were brought into linear apposition. The cutaneous tissues were then divided over the tumour an inch beyond the margin of the cork splints, the testicle was enucleated with a sweep of the bistoury, the cord at the same time being divided. The introduction of marginal sutures through the edges of the divided skin completed the operation. The splints and sutures were removed in two days, and the patient had returned home with the wound entirely healed.

IX. EXCISED KNEE-JOINT.

Dr Watson showed the osseous surface of the femur and tibia composing the knee-joint, which he had on the same occasion removed from a young married female who for seven years had suffered from disease of the left knee. The disease, from the history, apparently commenced in the synovial textures, but latterly the affection was manifestly chiefly progressing in the articular ends of the bone. The amount of synovial disease present rendered anything short of amputation or excision out of the question. The operation was performed in the usual manner, retaining the patella, the limb being supported by means of the apparatus Dr Watson constantly employs, and which he had recently exhibited to the Society. The case had progressed most favourably, not above ten drops of discharge escaping daily since the operation, while the patient was perfectly comfortable, enjoying sound sleep and good appetite, to which she had for months been quite a stranger. The diseased structures showed unusually well the appearances of ulceration of cartilage due to disease in the articular ends of the bones.

X. BLADDER AND KIDNEYS IN A CASE OF LITHOTRITY.

Dr Gillespie exhibited the bladder, kidneys, and ureters, removed from a man, who died of acute bronchitis some days after a small stone lying in the bladder had been crushed. The preparation showed great dilatation of the left ureter throughout its whole extent, with considerable atrophy of the left kidney. The orifice of the ureter in the bladder was enlarged and ulcerated. It was evident that the stone passing from the kidney had stuck in the orifice, causing obstruction and dilatation of the canal, with atrophy of the kidney. The right ureter was natural, but the kidney hypertrophied, so as to be about a third larger than the other, evidently to compensate for the loss of action in the left kidney. The mucous membrane of the bladder was natural, not inflamed nor thickened, and about eighteen pieces of a stone, which had been nearly the size of an almond, were found lying in the cavity. These fragments had been caused by one crushing of the lithotrite.

XI. DESCRIPTION OF A CYCLOCEPHALIAN FORM OF THE ETMOCEPHALOIDS.

Dr Handyside communicated to the Society the description of a male cyclocephalian infant, born alive in the valley of Jauja and village of Huaripampa, Peru, in 1860, as recorded in the *Gaceta Medica de Lima*, by Dr C. Bambaren, and to which his attention had been drawn by Dr Archibald Smith of this city, who had recently returned from Lima. Dr Handyside exhibited to the Society two representations, life-size, of this Cyclopean monstrosity, which is reported to have lived some hours after birth, and to have been baptized. With the exception of the fore-part of the head, the face, and the external genitals, the organs of the body were fully developed. Owing, however, to the absence of the nasal fossæ, the infant's respiration had been difficult, and the act of sucking was rendered impossible. Indeed, Dr Bambaren suspects that at birth the child was not perfectly viable. The father of the infant was 35 years of age, and the mother 30 years; both of the Indian race, and whose occupation was in excavating "guacas" or ancient pottery. The colour of the hair of the infant was black, and that of the skin generally a pale brown.

The fronto-parietal suture projects in a broad ridge-form, raising the integument; which, along this ridge, is uncovered with hair. The frontal bone is imperfectly developed, and in the mesial line is deeply grooved. There is no glabella over it; at the limit of the hair of the forehead is a cutaneous rounded proboscis or trumpet-shaped snout, which projects horizontally forwards from the plane of the face, and overhangs a wide orbito-nasal aperture or fissure of the face. This tubular process of skin, five-eighths of an inch long and three-eighths broad, is dilated anteriorly as the congener of the nasal apertures; its superior border represents the nasal dorsum, while its inferior border may be traced backwards to the upper border of the orbito-nasal cavity. This cavity is elliptical transversely, and is bounded by the four palpebræ, their free borders serving to circumscribe the space: thus, the external palpebral commissures terminate the ellipse laterally, while the internal ones bound it in the mesial line. The left upper palpebra alone is provided with cilia. The eyebrow on both sides is prominent and rugose; very obliquely directed from below upwards, to be lost in the rugose skin and hair at the root of the pyramidal snout. Within the common cavity of the orbits and nares may be distinctly recognised both eyes, in which are perceived the sclerotic, cornea, iris, and pupil; but all in a state of atrophy. Between them is a thin flat plate of fibro-cartilage, representing the element of the entire septum nasi, and of breadth equal to one eyeball. It is in the middle line, broader above than below, and covered by the common conjunctiva of the eyes. At the point of union inferiorly of the inner canthi of the lower palpebræ, the two lachrymal tubercles and canaliculi may be observed. The mouth is small, semilunar in form, and with the convexity upwards; and the upper maxilla and lip are very prominent. The philtrum is deeply marked. The lower maxilla is both small and short.

The genital organs are poorly developed; the penis is quite rudimentary, and nearly hidden within its prominent cutaneous folds.

The cerebro-spinal substance was too much liquefied to admit of any correct observations being made on it by Dr Bambaren.

Dr Handyside regarded the abnormal forms in this case of cyclopia as analogous somewhat to Vrolik's fourth variety of that species of monstrosity. There the two eyeballs are separated, but are within one orbit, and in contact, presenting a narrow partition between them; and above is a proboscis or snout curved either upwards or downwards. In the Lima case, however, there exists no true partition, and the snout projects forwards. Dr H. thought that this case helped to confirm the view of Bischoff, seeing that the atrophy of the eyeballs had arisen probably from arrested development in the brain, and especially in the separate and double optic lobes; and it helped too to support the conclusion of James Paget that the olfactory apparatus and nasal cavity had become displaced as a result of a preceding fusion and disappearance of the orbits, which, as opening together into a single shallow cup-like groove took the place finally of the nasal cavity.

Dr Handyside farther recognised, in this remarkable congenital deformity, *first*, an abnormal condition of the nasal segment of Huxley (nasal vertebra of Oken); and, *secondly*, traces of the embryonic common oral and nasal cavity (according to the researches of Rathke and J. F. Meckel)—the incomplete closure of the rudiments of which had, in Dr H.'s opinion, produced this, a permanent mesial fissure or orbito-nasal ectopia; a subsequent mutual approach and fusion issued in Vrolik's state of cyclopia of the nose, but not of the eyes.

Lastly, Dr H. drew the attention of the Society to the existence, in this case, of a *too short and very small lower maxilla*, a condition which had given rise to a subdivision of this fissure of the face into what Vrolik has called monotia.

Dr Handyside exhibited to the Society copies of treatises on physiology, surgery, materia medica, natural science, and natural history, in the Chinese language, which had been prepared under the superintendence of Dr Hobson, medical-missionary. These several works are of a folio size, and are amply illustrated by wood engravings, copied from our best British authors.

Part Fourth.

PERISCOPE.

MEDICINE.

CYLINDROTÆNIUM CHOLERÆ ASIATICÆ: A NEW FUNGUS FOUND IN CHOLERA EVACUATIONS. BY DR OTTO WILH. THOMÉ, COLOGNE.

THE author states that he has succeeded by microscopic examination, and by cultivation of cholera dejections, in discovering a parasitic plant, which was very abundant in all the cases he examined, and which exerted a peculiar disorganizing influence on the epithelial cells. He thinks it not too bold to suppose that *this is the cholera poison;* although he admits that this supposition is not yet confirmed by experiment,—as by introducing the plant into the food of animals and observing the effects. He conceives it impossible to do this satisfactorily, as we possess no means of separating the minute organisms from the fluid in which they are present, and we could not be certain that the poison was not in the fluid rather than the organism.

The plant belongs to a genus and species hitherto undescribed. The characters are as follow:—Cylindrotænium nov. gen. m. Mycelium filamentosum, achroum, ramosum, in substratum superficiale effusum. Hyphæ fructiferæ, mycelio simillimæ, parcæ, prostratæ interdum adscendentes, ramosæ, septatæ sporocladiis, vel sporophoris non distinctis. Sporæ concatenatæ cylindriformes. Sporarum evolutio succedanea.—Spec. nov. choleræ asiaticæ m. Mycelio parco, vix conspicuo, septato, repente, hyphis fructiferis ramosis, rarius simplicibus, sporis longitudine ca. 0·012—0·016 Mm., latitudine ca. 0·007—0·009 Mm. Variat ex substratorum natura, mox Hormiscii, mox Hyphomycetis habitum præbens, mox sporulas locomotione præditas, magnamque muci vim proferens. Invenitur in choleræ asiaticæ dejectionibus.

The author gives numerous drawings illustrative of his observations.—*Virchow's Archiv*, February 1867.

THE TEMPERATURE IN CHOLERA. BY DR L. GUTERBOCK, BERLIN.

As the result of his observations on this point during the recent outbreak, the author gives the following conclusions:—1. In the cold stage the temperature is lowered in the head and extremities to a degree scarcely equalled in any other disease. 2. In the cold stage the temperature of the cavities of the trunk (vagina, rectum), the highest in the body, are alone worthy of trial in determining the general temperature. 3. In the cold stage in most cases (whether it be followed by death or recovery) the internal temperature of the body is elevated, rarely normal, still more rarely diminished, without causes as yet discerned during life or in the dead body. 4. In the cold stage the temperature rises usually as death approaches; an increase after death does not appear to occur. But cases occur in which there is no increase of temperature in the agony, the cause of this difference not being explained. 5. With the occurrence of simple reaction there is no elevation of temperature; rather usually a slight diminution of the internal parts, while the external parts get warmer. 6. In the cases of protracted reaction (protracted asphyxia) the whole temperature sinks usually below the normal. 7. The inflammatory sequelæ induce usually a marked increase of the temperature. 8. During complete re-convalescence the temperature is sometimes high without apparent cause.—*Ibid.*, Jan.

ON THE PATHOLOGICAL ANATOMY OF THE BRAIN. BY PROFESSOR VIRCHOW.

1. *Congenital Encephalitis and Myelitis.*—Virchow shows that in the fœtus of women affected with acute exanthemata (particularly smallpox), and syphilis,

a peculiar interstitial encephalitis or myelitis occurs, which has its seat in the white substance, and chiefly in the hemispheres of the cerebrum and the columns of the cord; that it consists mainly in a fatty metamorphoses of the cells of the neuroglia. He has found this condition in a considerable proportion of still-born children, and in a not inconsiderable proportion of those dying soon after birth. The condition is often such as to be unrecognisable by the naked eye, but distinct when examined microscopically. Virchow thinks the observation may prove important in medico-legal questions of child-murder.

2. *Heterotopia of Grey Substance.*—Virchow records a case of malformation of the right cerebral hemisphere, whereby masses of grey substance appeared where no such should be found.—*Ibid.*

ON OSTEOMALACIA, PARTICULARLY THE SENILE FORM, AND THE OCCURRENCE OF LACTIC ACID IN OSTEOMALACEOUS BONES. BY PROFESSOR OTTO WEBER, HEIDELBERG.

THE author gives details of two cases of senile osteomalacia and of the chemical examination of the bone. In both cases the phosphate of lime was markedly diminished, and at the same time lactic acid was present in considerable quantity.—*Ibid.*

ON INTERSTITIAL PLACENTITIS AND ITS INFLUENCE ON PREGNANCY AND BIRTH.

BY PROFESSOR A. HEGAR AND RUD. MAIER OF FREIBURG.

THE authors record a case in which inflammatory condensation, followed, by fatty degeneration, existed in patches here and there throughout the organ; nevertheless, a healthy child was born at the full time, and the mother made a good recovery. The paper is intended as a contribution to our knowledge of the subject of retention of, as well as of the pathological changes in, the placenta.—*Ibid.*, March.

SURGERY.

ACETIC ACID IN EPITHELIAL CANCER.

IN a leading article of the Gazette des Hôpitaux, after briefly noticing the purely negative results obtained by the injection of acetic acid into true cancers, a notice is given of a series of cases reported by Dr Dieu of the Hôtel des Invalides, in which he had used acetic acid with the happiest results on those tumours and ulcers of the skin which are classed under the heads of "cancroide," "noli-me-tangere," and "cutaneous epithelioma." In one of eleven years' standing, the ulcerations of the lower eyelid and angle of the eye was as large as a franc-piece, yet after ten external applications of the concentrated acid during one month only, cicatrization was complete. Another in an old soldier (æt 77), a tumour of twelve years' standing, as large as a filbert, with an indurated base, was completely removed after two injections, and four applications, of the concentrated acid. Three other cases were similarly treated with similar happy results.

ACETIC ACID INJECTIONS IN CANCER.

DR BROADBENT'S method of treating cancer by injections of acetic acid has been tried in several hospitals in Paris with very "mediocre" results. At the Bicêtre, M. Tillaud employed it in the case of an old man whose face was in part destroyed by cancer. For some days the progress of the disease seemed retarded, the tissues were less swollen, and the acetic acid appeared to be of use; but soon the ulceration resumed its invading march, and the acid seemed to have lost its effect.

At the Hôtel Dieu, M. Maisonneuve, having tried the same plan in a female whose nose and cheek were eroded by a malignant ulcer, did not obtain from it the least result.

Again, in a most favourable case, M. Laugier produced a considerable diminution in the size of a cancroid tumour of the lower lip, by external applications and injection of acetic acid; yet, after some weeks seeing no prospect of cure, had eventually to remove with a knife the affected portion of the lip. None of the foregoing cases, however, seem to have been quite suitable ones for a fair trial of Dr Broadbent's plan.—*Gazette des Hôpitaux*, January 29, 1867.

ON REPRODUCTION OF THE CRYSTALLINE LENS.

FROM a pretty full resumé of an important paper, read to the Academy of Sciences on 28th January 1867 by M. Milliot, we may extract a few conclusions. The experiments were very numerous, extending over two years, and were performed on sheep, dogs, cats, rabbits, guinea-pigs, rats, and frogs. Extraction was generally performed by the flap operation, aided by free division of the anterior capsule.

He has proved the fact incontestably, that in animals a regeneration or reproduction of the crystalline lens does frequently occur, even after complete removal. That this commences about the end of the second week after the operation, but is not completed till from 5 to 12 months, or even longer in aged animals. That the new lens is never so large as the old one. That sometimes the new one shows all the microscopic characters of the original lens; that more frequently it is an amorphous hyaline mass, which contains a small number of nuclei analogous to those of the cells known as the humour of Morgagni, sometimes fibrous tissue in laminæ, containing proliferating nuclei. Occasionally the cavity of the capsule becomes filled with vitreous humour, or, in cases where the iris has inflamed, with plastic lymph. That in human lenses affected by cataract, M. Milliot believes that except possibly in young subjects (which he has had no opportunity of verifying by dissection), there is no reproduction of the lens. This is due partly to the age of the patients, partly to the changes which have been shown to occur in cataract, in the powers of endosmose and exosmose of the capsule, and through it on the nutrition of the crystalline lens. He has still doubts on this subject, and thinks it possible that in those cases where, in young subjects, vision is good without a cataract glass, that there really may be a reproduction of the lens in whole or in part.—*Gazette Médicale de Paris*, Feb. 9, 1867.

RESECTION OF THE CALCANEUM.

AT the meeting of the Société Imperial de Clinique, on the 23d of January 1867, M. Giraldès read a letter from Professor Rigaud of Strasburg on the above subject. He briefly recorded seven cases of complete excision of the os calcis, six of which were performed by himself, the seventh by his assistant, M. Bœckel, during the professor's absence in the autumn of 1866.

Of the seven, only one died (of pyæmia, when the wards of the Hospital were in an unhealthy condition); the rest all made good recoveries, and walked freely and without lameness; some by the aid of high heels, others without any alteration in their shoes.

His method of operating he thus describes:—"A horizontal incision passing about five lines below the ends of the malleoli, extending forwards as far as the calcaneo-cuboid articulation, and as far on the inside as on the outside, and cutting the tendo Achillis as low down as possible, allows us to detach a plantar flap as far as the border of the anterior extremity of the os calcis; this must be cut as exactly as possible; we have now detached the posterior insertion of the plantar muscles. The astragalo-calcanean articulation is now opened behind, and we penetrate between the two posterior facets by which the two bones articulate, then clearing the os calcis on either side as if scraping it, we reach the space where the astragalus and os calcis do not touch each other. The anterior articulation between the bones is then to be opened, the great fibrous band which unites them must be cut, and then the os calcis removed by disarticulating it from the cuboid. By this method of procedure, we have been able to accomplish the extirpation without exposing either the flexor tendons or those of the peronei, or even the posterior tibial artery and nerve.—*Gazette des Hôpitaux*, Feb. 19, 1867.

Part Fifth.

MEDICAL NEWS.

MORTALITY OF DISEASES IN SWEDEN.

In this country we can only guess in a very rough way at the relative fatality of different diseases, seeing that we have no means of ascertaining the number of persons attacked, excepting in public hospitals and charitable institutions. In Sweden, however, the most perfect system of registration prevails, and the accompanying Table, drawn up from the facts stated in the "Health and Sickness Report" for 1862 (recently published) will be read with interest. Excepting for pneumonia and bronchitis, the number of cases and of deaths are for the five years 1838 to 1862 inclusive, and are the numbers for the whole kingdom; the population being 3,859,728 on the 31st December 1860.

Mortality of Diseases in Sweden during the five years 1858–62 *inclusive.*

Names of Diseases.	Number of Persons attacked.	DEATHS.	Proportion of Deaths to the Number attacked.	
			Percentage.	One in every
Typhus and Typhoid Fevers,	29,520	2467	8·3	12·0
Epidemic Brain Fever (Cerebro-spinal Meningitis),	3,896	1577	40·4	2·4
Diphtheria,	2,505	677	27·0	3·7
Hooping-cough,	16,075	1113	6·9	14·4
Dysentery,	19,609	4758	24·2	4·1
Ague (Intermittent Fever),	53,371	346	0·6	154·2
Mumps (Parotitis),	3,265	44	1·3	74·2
Smallpox,	16,374	2139	13·0	7·6
Scarlatina,	15,720	3566	22·6	4·4
Measles,	41,332	2677	6·4	14·3
1862. Pneumonia and Pleurisy,	8,670	1090	12·6	7·9
Bronchitis and Influenza,	11,575	329	2·8	35·2

Some very interesting facts are brought out by the Swedish statistics, proving the modifying effect of climate, or latitude, on the time of year when the different diseases prevail and are most fatal. For instance, all our ideas regarding intermittent fever or ague are based on its form and course in warm climates. But in Sweden it is not the autumnal months which are obnoxious to attacks of ague, but the spring months of March, April, and May. Over the whole kingdom the number of cases of ague, and deaths therefrom, are ten times as numerous during each of these months than during any of the autumnal months, August, September, or October. In other words, for every eighty cases of ague in each of the spring months, there are not even eight in each of the autumnal months. This at once accounts for the fact, never before explained, that those who have suffered from ague in foreign climates chiefly suffer relapses in this country during the spring months when the biting east winds blow.

DOCTOR'S JUBILEE OF PROFESSOR ROMBERG.

On the 29th of March was celebrated, at Berlin, the Doctor's Jubilee of Professor Romberg, whose works on Nervous Disease are of European celebrity. To few of the medical profession is it given, after half a century's labour in an arduous profession, to receive, unshaken in health, unchilled in heart, and still triumphant in the race of fame, the warm congratulations of his colleagues in so genial a spirit. Romberg is not more distinguished for his intellectual gifts than for genial warmth of heart, and true nobility of character; and his countrymen have eagerly availed themselves of this occasion to do him honour.

The jubilee was worthily begun by a choral, sung by the cathedral choir, followed by the felicitations of the numerous friends and relatives of the veteran doctor. Congratulatory addresses were then presented to him from the Military Medical Institute, of which he was long a lecturer, by Physician-General Dr Grimm; from the Berlin Medical Society by their president, Von Graefe, who handed to Dr Romberg the diploma of honorary membership, the first and only appointment of the kind; and from the Berlin University, represented by the senate and deans of the four faculties. After an eloquent address, the medical dean, Dubois-Reymond, presented Romberg with a *renewed doctor's diploma!*

The next congratulations were offered by his Excellency the Minister of Public Instruction, V. Mühler, who, on behalf of his Majesty the King, invested Dr Romberg with the insignia of the Order of the Crown of the second class. Deputations and addresses followed from the different German Universities, Bonn, Breslau, Greifswald, Konigsberg, Kiel, Jena, Rostock, Heidelberg, Berne, Erlangen, Munich, and Würzburg; from the Doctor's College of the Medical Faculty of Vienna, and from the Imperial Society of Physicians of Vienna. Romberg's former assistant-physicians, after a cordial address from Geheimrath Koblanck, presented him with a splendid porcelain vase, on which a view of his residence was painted.

To these addresses and congratulations Romberg returned thanks in words characteristic of his extreme modesty and his unassuming estimate of his own attainments. The appropriate adaptation of his remarks on each occasion proved him still, as he used to be in the professor's chair, an eloquent speaker, skilled in the dignified expression of noble thoughts.

The second part of the festival consisted in a banquet at the Hôtel de Rome, given by the medical faculty, in which above 150 persons, including the élite of Berlin society, and all the deputies of the different Universities, participated. Romberg appeared on the arm of his contemporary and friend of many years, Jüngken, and was received with acclamation by the assembled guests. The first toast, to his Majesty, was proposed by the rector of the university, Herr v. Langenbeck. Then followed the health of Dr Romberg, by the dean of the medical faculty, Professor Dubois-Reymond, who remarked that it was Romberg who had made the name of Charles Bell popular in Germany, and had erected an edifice of practical medicine on his physiological theories; he was one of the first, not only to comprehend and to realize in its full significance the importance of physiology in connexion with pathology, but also to open a new and rich fountain of physiology in pathological observation. Romberg returned thanks in a speech of much feeling, in which he referred in eulogistic terms to the great discoverers in nerve science, Charles Bell, Weber, Müller, Helmholz, Dubois-Reymond. These inquiries, he said, had laid the foundation; the present age beholds the commencement of the edifice, which, rising ever higher and higher, will diffuse the light of scientific truth to future generations.

Afterwards, Virchow, in a humorous speech, alluding to the old notion that the diseases of the nerves and of women were identical, proposed the health of the estimable wife of Professor Romberg; and the professor's son, the Attorney-General Romberg, dedicated a bumper to his father's contemporaries V. Ranke, Boeckh, and Jüngken. Various toasts followed, by Bergson, Göschen, Jüngken, Bardeleben, etc., and the company separated at a late hour. —*Abridged from the Berliner klinische Wochenschrift,* 8th April 1867.

CONVERSAZIONE AT THE ROYAL COLLEGE OF SURGEONS.

On 18th April, a Conversazione was held in the Hall of the Royal College of Surgeons, the company being received by the president, Dr Dunsmure. A lecture was delivered by Dr Handyside, F.R.S.E., on "Anatomy in relation to the Fine Arts," to a highly appreciative audience, consisting largely of artists and connoisseurs in art. On the motion of the Lord Provost, seconded by Mr Bouverie Primrose, a vote of thanks was conveyed to the lecturer.

APPOINTMENT TO THE CHAIR OF ANATOMY IN THE UNIVERSITY OF EDINBURGH.

Mr Turner has been elected to the Chair of Anatomy in the University of Edinburgh. The other candidates were, Dr Struthers, Professor of Anatomy in the University of Aberdeen, and Dr Pettigrew, an Edinburgh graduate, now first Assistant in the Museum of the Royal College of Surgeons of England, whose beautiful dissections and anatomical writings are well known. Of the seven curators, by whom the appointment is made, six voted for Mr Turner and one for Dr Struthers.

PUBLICATIONS RECEIVED.

Julius Althaus, M.D., etc.,—Epilepsy, Hysteria, and Ataxy. London, 1867.
J. C. Atkinson, M.D.,—Change of Air considered with regard to Atmospheric Pressure in the Treatment of Consumption and Chronic Disease. London, 1867.
Beamish's Practical Observations on the Pathology, Prevention, and Treatment of Asiatic Cholera. With Cases. Dublin, 1867.
Béraud's Atlas of Surgical and Topographical Anatomy. Parts 3 to 9.
Professor C. L. Bloxam,—Chemistry: Inorganic and Organic. London, 1867.
W. Brinton, M.D.,—Intestinal Obstruction. Edited by T. Buzzard, M.D. Lond., 1867.
T. Bryant, F.R.C.S., etc.,—On Ovariotomy (Clinical Surgery, Part VII.) Lond. 1867.
Professor Andrew Buchanan, M.D.,—Classification of the Functions of the Human Body, etc. London, 1867.
Catalogue of Obstetrical and other Instruments, exhibited at the London Obstetrical Society's Conversazione in March 1866.
Holmes Coote, F.R.C.S., etc.,—On Joint-Diseases. London, 1867.
Henry G. Davis, M.D.,—Conservative Surgery. New York, 1867.
Demarquay,—Recherches sur l'Absorption des Médicaments faites sur l'Homme sain. Paris, 1867.
V. Duke, M.D.—Physiological Remarks upon the Causes of Consumption. Dublin, 1867.
Dr Arnold von Franque,—Das Vorkommen der Blattern in dem Herzogthum Nassau, 1818-62. Wiesbaden, 1866.
A. C. Garratt, M.D.,—Guide for Using Medical Batteries. Philadelphia, 1867.
Charles D. Homans, M.D.,—Extracts from the Records of the Boston Society for Medical Improvement. Boston, 1867.
H. B. Jones, M.D.,—Lectures on some of the Applications of Chemistry and Mechanics to Pathology and Therapeutics. London, 1867.
Dr Theodor Kocher,—Behandlung der Croupoesen Pneumonie mit Veratrum-praeparaten. Würzburg, 1866.
George Lawson, F.R.S. Eng.,—Injuries of the Eye, Orbit, and Eyelids. Lond., 1867.
T. M. Madden, M.R.I.A., etc.,—Insanity, and the Criminal Responsibility of the Insane. Dublin, 1866.
E. D. Mapother, M.D., etc.,—Lectures on Public Health. 2d Edition. Dublin, 1867.
Henry Maudsley, M.D.,—The Physiology and Pathology of the Mind. Lond., 1867.
W. Pirrie, M.D., and W. Keith, M.D.,—Acupressure. London, 1867.
Quain's Elements of Anatomy. Edited by W. Sharpey, M.D., A. Thomson, M.D., and J. Clelland, M.D. 7th Edit. London, 1867.
F.C. Skey, F.R.S., etc.,—Hysteria: Lectures. London, 1867.
Offley Bohun Shore, M.D.,—Domestic Medicine. Edinburgh, 1867.
W. A. Smith, M.D.,—On Hay-Fever, Hay-Asthma, or Summer Catarrh. Lond. 1866.
W. A. Smith, M.D.,—On Diabetes and on Enuresis. London, 1867.
Statistical Abstract of the Health of the Royal Navy, 1865-66. London, 1867.
Transactions of the Obstetrical Society of London for 1866. London, 1867.
Trousseau's Clinical Medicine. Translated by Bazire. Part II. London, 1867.
Dr Ullersperger,—Die Heilbarkeit der Lungenphthisen. Würzburg, 1867.
Erasmus Wilson, F.R.S.,—Diseases of the Skin. Sixth Edition. London, 1867.
T. Worthington,—Pavilion Hospital at Chorlton Workhouse.

PERIODICALS RECEIVED.

American Jour. of Med. Sciences,—Jan.
Berliner klin. Wochensch.,—Nos. 3 to 16.
Births, Deaths, and Marriages, Monthly Return of, for March 1867; Quarterly Return for Dec. 1866; Supplement for 1866.
Brit. and For. Med. Chir. Review,—April 1867.
British Medical Journal,—Feb. 2 to April 27.
Bulletin Générale de Thérapeutique,—Dec. 15, 30, 1866; Jan. 30, Feb. 15, 1867.
Canstatt's Jahresbericht,—Vols. III. and V., for 1865.
Dublin Quar. Jour. of Med. Science,—Feb.
Gazette des Hôpitaux,—Nos. 10 to 47.
Gaz. Hebd. de Médecine, etc.,—Nos. 4 to 16.
Gazette Médicale de Paris,—Nos. 4 to 16.
Glasgow Med. Jour.,—Feb., Mar., and Apr.
Journal of Mental Science,—April.
Klin. Monatsbl. für Augenheilk.,—Jan., Feb.
Medical Press,—Feb. 6 to April 24.
Med. Times and Gazette,—Feb. 2 to Apr. 27.
Medizinische Jahrbücher,—Nos. 1 and 2.
Nederlandsch Tijdschrift voor Geneeskunde,—Nos. 28 to 42 for 1866; Nos. 1, 2, for 1867.
New York Medical Journal,—July 1866; Jan. and Feb. 1867.
Rev. de Thér. Méd.-Chir.,—Feb. 1 to Apr. 15.
Scottish Meteorological Journal,—No. 13.
Viertelj. für die Prakt. Heilkunde,—Vol. I. Prague.
Virchow's Archiv,—Dec., Jan., and Feb.
Wochenblatt der Zeitschrift der Aerzte,—Nos. 50 to 52, 1866; Nos. 1 to 9, Vienna.

Part First.

ORIGINAL COMMUNICATIONS.

Article I.—*The Life, Character, and Writings of William Hey of Leeds: a Discourse delivered at the Annual Meeting of the Harveian Society, Edinburgh, April* 12, 1867. By Benjamin Bell, F.R.C.S.E., President of the Society.

While thinking of a topic which might be suitable for this Harveian Discourse, it occurred to me that a biographical sketch of that eminent surgeon, the first Mr Hey of Leeds, might be acceptable to the Society, particularly to the junior members, who are less likely to be acquainted with his merits. His life was written nearly fifty years ago, by his pupil and friend the late John Pearson, surgeon to the Lock Hospital in London; and no one can read the work without perceiving that the subject of it must have been not only distinguished in his profession, but remarkable as a useful citizen, and a man of the highest moral and religious character. Still, the book is defective in its arrangement, contains a good deal of irrelevant matter, and fails, I think, in bringing out with sufficient prominence, what I believe to be true, that Mr Hey was, as a professional man, really in advance of his time, and contributed largely towards the improvement of practical surgery. Although practising in a provincial town, and contemporaneous with many surgeons, both in London and elsewhere, of distinguished reputation, men, who not only improved the practice, but established the principles of scientific surgery, Mr Hey, if I mistake not, was quite on a level with the best of them, as a wise, sagacious, and successful practitioner. His long life made him contemporary with a number of bright names in our profession. When Cheselden died, he was fifteen years of age. John Hunter was born (1728) eight years before him, and died (1791) when Mr Hey was fifty-five. Samuel Sharp died (1778) when Hey was forty-two, and Percival Pott (1713–88) ten years later. B. Bell was his junior by thirteen years (1749–1806), and predeceased him by twelve. Desault was eight years younger than Hey (1744–95), and predeceased him by twenty-three. Coming still farther down, he was contemporary with John Abernethy (1765), Astley Cooper (1768), John Bell (1762), Charles Bell

(1774), Benjamin Collins Brodie (1783), William Lawrence (1783), and others, who were already known to fame, when he still practised his profession. It may not be unprofitable, therefore, to endeavour, in the time allotted to me, to bring before our minds one of the worthies of a former generation, whom we are apt to overlook and almost forget, in these days of excessive teaching and cramming, when, as it appears to me, the dead level of mediocrity is seldomer relieved than it used to be by men of special attainments in their profession and strong individuality of character.

William Hey was born on the 3d of September 1736, in the parish of Calverly, near Leeds, in the West Riding of Yorkshire. His father, Richard Hey, was a respectable tradesman, chiefly noticeable among his neighbours by a peculiarly nice sense of honesty, by his vivacity and strong abhorrence of every practice repugnant to these sterling qualities. His keen eye for the smallest deviation from honesty almost bordered in his last years upon disease; but he did not judge his townsmen alone, he included himself in the condemnation, saying to his son on one occasion, "*I* am not an honest man," although, as already noticed, honesty was the feature of character for which he was chiefly remarkable. William Hey must have owed, in some measure, to his father those sterner qualities by which he was afterwards distinguished among his compeers.

On his mother's side he had a professional ancestry, which probably suggested his future occupation; his grandfather being Mr Jacob Simpson, a surgeon in Leeds, and his great-grandfather, Dr William Simpson, a physician, who practised in the neighbouring town of Wakefield. His mother appears to have brought up her large family, consisting of four sons and three daughters,—another son having died in infancy,—with great judgment, and with a provident concern for their interest both temporal and eternal, while she was remarkable for the even and impartial affection with which she treated all the members of her family, whereas her husband sometimes permitted his strong views, already referred to, to pass into what might be regarded as undue severity and even abiding prejudice. In short, both parents were excellent people, and supported one another in the conscientious upbringing of their children; but there is reason to believe that Mrs Hey was the wiser and more judicious of the two, and therefore, as is very often the case with mothers, bore the principal part in moulding their characters. Two of her sons, in extreme old age, furnished Mr Pearson with a brief memorial of both parents, which he has prefixed to his life of their brother William; they touchingly conclude their reference to their mother with these words of Solomon, "Her children rise up and call her blessed."

William, the second son of this family, with whom we are mainly concerned, was a child of great vivacity and energy, entering with zest into all the active sports of youth, and giving early indications of the vigour and activity both of mind and body which were conspicuous at all periods of his long career. At three years of age, he made a narrow escape from being burned to death by his dress taking fire; and in the following year, while cutting a piece of string with a pen-knife, the point wounded his right eye, so seriously as permanently to destroy the vision. The deformity thus occasioned is evident in his portrait, the injured eye being, to a certain extent, everted; but the other eye, instead of participating in the mischief, as is too often the case, possessed the power of vision in great perfection, and this continued to a very late period of life, enabling him to read even small print and perform the nicer details of surgical practice without the aid of glasses. This is the more remarkable that we have no evidence of his being originally near-sighted.

Before he was eight years of age, he and his oldest surviving brother John were sent to an academy near Wakefield, where, during seven years, besides being instructed in the classics, he acquired a taste for natural philosophy, and a competent knowledge of the French language, which he afterwards spoke with correctness and fluency.

At fourteen years of age, the natural bent of his mind,—being, as already noticed, a lad of much activity and energy,—led him to choose the navy as his profession; but, in compliance with the wish of his parents, he agreed to study medicine, consoling himself with the hope that, some day or other, he should attain the object of his ambition, by becoming surgeon to a man-of-war. He was accordingly placed as an apprentice with Mr Dawson, a surgeon and apothecary in Leeds. In this position, he conducted himself in the most creditable manner, performing many duties that would now-a-days be regarded as drudgery, embracing every opportunity of gaining professional knowledge and securing the approbation and permanent esteem of his master and his family. On one occasion, his zeal in acquainting himself with the effects of medicines, threw the whole household into great alarm, having led him to swallow so much of some preparation of opium that his life was in danger for several hours.

In the autumn of 1757, having served his apprenticeship, and with a mind prepared, by his previous training and opportunities, to study his profession with zeal and intelligence, William Hey went to London to complete his education. We may form some estimate of the spirit which animated him at that critical period in his history by the following sentence in a letter addressed to his parents upon his arrival in the great metropolis:—"I would spare no pains to qualify myself for that state of life to which the providence of God has called me, and then trust Him with the success of my endeavours."

The first winter in London was devoted to anatomy, which his good sense taught him to be the only true foundation of professional knowledge. Besides attending lectures, he almost lived in the dissecting-room at this time, and instead of being satisfied with the aid of books to guide him, he continued to dissect without them, and even composed a description of the nerves and bloodvessels from his own dissections. We cannot doubt that this earnest study of human anatomy enabled him afterwards to take and maintain the eminent place which he filled for so many years; because his anatomical knowledge, besides being precise and serviceable in itself, was relatively great at a time when professional accomplishments were less generally diffused than in the present day. When accurate attainments, steadiness of purpose, and native sagacity thus meet together, they seldom fail eventually to place their possessor upon a pinnacle of deserved professional success.

Having become a pupil at St George's Hospital under Mr William Bromfield, a man of considerable reputation as a practical surgeon, he began to act as a dresser in June 1758. During the same summer, he attended the lectures on medicine of Dr Donald Monro, one of the physicians, who was much esteemed as a teacher and regarded as an able practitioner. This gentleman, it is worth knowing, was an elder brother of Alexander Monro *secundus*, and wrote the interesting memoir of their amiable and distinguished father, prefixed to the collected edition of his works. Dr Donald Monro was only twenty-seven when William Hey was his pupil, and that very year his younger brother Alexander commenced his long career of professor of anatomy, in Edinburgh, at the age of twenty-five. Early in 1759 he attended the lectures of Dr Mackenzie on midwifery, and, as we gather from a letter to his son Richard, who studied medicine, but died young, must have been very assiduous in endeavouring to make himself master of the subject,—" When you attend Dr Lowder, and are come to that part of his lectures in which the pupils execute deliveries on the machine, I would advise you to sit by the machine all the time of lecture. I found more advantage from seeing the various ways in which others got wrong, than from being corrected merely for my own blunders."

His education in London being completed, it was put in his power to resort to Paris, if he chose, for the further prosecution of his professional studies; his father, who, in spite of his peculiarities, was evidently a man of liberality and enlarged views, being desirous that all his sons should be as fully qualified as possible for their respective professions. But William, after weighing the professional benefits to be expected from a residence in the French capital against the moral dangers to which his principles might be exposed, declined the tempting offer, and returned home to enter upon the long and honourable career which awaited him.

It would appear from the testimony of intimate friends, that, before this date, young Hey had taken great pains with himself, in the way, more especially, of strengthening his power of attention and regulating the current of his thoughts. For example, he would systematically bind himself down to a particular subject while walking to a certain distance, even in the streets of London, and would then dismiss the subject for another, which received in turn a due measure of attention. The power to proceed in this manner implied a natural aptitude for continuous thought by no means common, and we can understand therefore, that when disciplined by use, it became habitual, and was of essential service afterward in enabling him to turn to good account the broken intervals of professional occupation. Are we wrong in believing that this valuable capacity for controlling and guiding the current of thought is the main distinguishing feature in all minds which have left their mark upon the generation that produced them? It was so characteristic of Mr Hey,—making him very slow in expressing opinions of his own, or assenting to those of other people, until they had been systematically pondered,—that some of his associates used to feel a little uncomfortable in his presence, when his cold and cautious reserve of manner conveyed the impression that they had given utterance to something which was either foolish or displeasing.

But we anticipate, in thus describing the man in his maturer years, when as yet only a youth of twenty-three, entering upon his professional life.

His old master, Mr Dawson, offered to take him into partnership, and when this was declined after much deliberation, kindly expressed a desire that Mr Hey should reside near him, that he might recommend to his care such surgical cases as were willing to employ him. Mr Billam was at that time the principal surgeon in Leeds, a man of classical tastes, general information, and extensive practice; but even he had performed very few important operations. A Mr Lawman had once performed lithotomy, and once amputated a limb. We mention these things to show that, although the population of Leeds and the neighbouring townships was much smaller than it is now, there could not fail to be a large field for surgery, to a young man prepared from the very first to treat the most serious accidents, and to undertake every operation that circumstances might require. His capabilities being well known, and his character appreciated, his brethren kindly gave him their countenance and assistance when needed; and he appears to have rapidly taken a conspicuous place among them. He performed lithotomy successfully three times in his private practice within the first year; and although there is no regular account of cases till the year 1763, when he had been four years in business, from that time till 1767, four cases of lithotomy are recorded, and four of the operation for strangulated hernia, besides various other operations. But still his progress in obtaining remunerative business was slow, and nearly ten years elapsed before the emoluments of his practice were equal to the expenses of his family.

It is time to mention that, on the 30th of July 1761, he married a young Yorkshire lady, Miss Alice Banks, who was the faithful partner of his life during fifty-eight years, and survived him. His courtship was characteristic of the man; his conscientiousness and self-control leading him to bring systematically before her mind the solicitudes, responsibilities, and duties of the important relationship into which he invited her to enter. The young lady was not repelled by this unusual but business-like mode of procedure. She understood and appreciated the character of her future husband, and never had any occasion to regret the confidence which she reposed in him.

When Mr Hey settled in Leeds, there was no infirmary, and it was not until eight years after, in May 1767, that the first steps were taken towards the establishment of such an institution. No time was lost, a temporary building was secured on the 25th of July, the rules of the establishment were read and confirmed on the 29th, and the medical officers were appointed at the same time. Mr Hey took a very active share in all the proceedings, and was thereafter the only medical man whose name appears on the building committee. It followed, almost as a matter of course, that he was elected one of the surgeons. His colleagues were Mr Billam, whom we have already mentioned as a surgeon of eminence, Mr Jones, and Mr Lucas. The former of these two was about four years Mr Hey's junior, and held the position during sixteen years, retiring first to Bingley, and then to Bradford, where he died on the 4th of January 1803, in the 63d year of his age. Mr Lucas was two years Mr Hey's senior, and acted as his colleague for twenty-six years. Retiring from business in 1794, he afterwards resumed it as a consulting surgeon at Ripon in 1807. He died in 1814, in the 81st year of his age. Mr Billam, who was much older than any of his colleagues, resigned the office of senior surgeon in November 1773, and was succeeded in it by Mr Hey. The new building was opened for the admission of patients in March 1771. It underwent during Mr Hey's time three successive enlargements, until at length it contained 116 beds.

Although an enthusiastic student of his profession, and much occupied with its duties, Mr Hey found time for other pursuits. Somewhere about the years 1768 and 9, he formed an intimacy with the famous Dr Joseph Priestley, who then resided at Leeds, and found in the young surgeon an intelligent coadjutor in his chemical and physiological investigations. He evidently formed a very high estimate of Mr Hey, for he proposed him as a Fellow of the Royal Society of London, and in the letter informing him of the distinction which had been conferred upon him, remarks:—"I wish I could say that one of the members in ten had equal pretensions to it." In Dr Priestley's memoirs, we also read as follows:—"The only person in Leeds who gave any attention to my experiments was Mr Hey, a surgeon. He was a zealous Methodist, and wrote answers to some of my theological tracts; but we always conversed with the greatest freedom on philosophical subjects, without mentioning anything relating to theology. When I left Leeds, he begged of me the earthen trough in which I made all my experiments while I was there." The intercourse thus referred to was very creditable to both parties, when we bear in mind that Dr Priestley was a Socinian, and opposed to the established authorities in Church and State, while Mr Hey, not only held tenets directly at variance with those of his companion, but wrote against them, especially his religious errors, in the most uncompromising manner. The works referred to are two pamphlets, one entitled, "A Short Defence of the Doctrine of the Atonement," the other, "A Short Defence of the Doctrine of the Divinity of Christ." They were regarded by competent authorities at the time as able and satisfactory productions, remarkable for the absence of controversial acrimony, and yet characterized by an earnest and firm tone, indicating the author's conviction that the doctrines which he defended are vitally important.

In 1773, Mr Hey was laid aside from active duty for some weeks by an injury of his knee, from which he never perfectly recovered, although he was again able to follow his avocations in town without perceptible inconvenience, and to visit his patients in the country on horseback, by day or night, as he had been accustomed to do previously. He was now fully employed, stood high as an operating surgeon, and was confided in by many at considerable distances from Leeds, who either came to consult him, or sent for him in cases of peculiar difficulty and danger. Everything seemed prosperous, and, to all human appearance, his professional eminence was about to be crowned with pecuniary emolument. This he really seemed to need, as he already had a family of ten children, with the near prospect of an eleventh. But, early in 1778, five years after his former accident, a stroke from a horse upon the thigh of his weak limb, while he was in the act of mounting, disabled him so completely that neither he nor his professional friends expected that he would ever regain the power of walking. After consulting some eminent surgeons in London, he visited Bath and Harwich, and then returned home, after an absence of four months, in good general health, but with little improvent of his lameness. From this period to the end of his life, forty years after, he was unable to walk, except across a room of ordinary size, without the aid of a crutch, and to stand for more than a few minutes was beyond his power. He could ride occasionally on horseback in town, or a mile beyond it; but most of his professional visits were now paid by means of a carriage, which soon became indispenable to him, from the rapid increase of his business, in spite of circumstances apparently so adverse.

The long period of suffering, and hope deferred, through which Mr Hey passed during the spring and summer of 1778, seems to have called forth much patient resignation to the divine will; and we cannot help admiring the undaunted courage and resolution with which he afterwards performed, in spite of so much lameness, all the duties of an arduous professional life. We get some insight into the lofty principles which sustained him, by the following reply to a friend made during the period when the result of his accident was still doubtful—"If it be the will of God that I should be confined to my sofa, and he command me to pick straws during the remainder of my life, I hope I should feel no repugnance to his good pleasure."

In addition to this personal affliction, Mr Hey had to sustain a succession of family bereavements, which must have tried him very severely. The first of these was the rapid removal of his eldest son Richard, a young man of great promise, who, after assisting his father in practice for a few months, with much acceptance to his patients, fell a victim to pulmonary consumption. So insidious had been the progress of the disease, that the day of his marriage was fixed, and all the arrangements for it made, before any anxiety was awakened regarding his health. He died on the 20th of March 1789 in his twenty-fifth year.

The second son, William, who was then seventeen, and had been an apprentice to his father for two years, completed his studies, as his brother had done, in London, and afterwards commenced practice in 1794. After assisting Mr Hey for about two years, he met with an injury of his ancle, which laid him aside for four years and a-half, during which he could hardly walk without crutches. But he eventually got well, and became an eminent surgeon, amply sustaining the family reputation.

Mr Hey lost his third daughter, Alice, of a pulmonary complaint in 1794; and subsequently, two excellent sons, John and Robert, were carried off by the same relentless disease in 1801 and 1802 respectively, both of them in the twenty-fourth year of their age.

In August 1805, another afflictive dispensation occurred in the death of Mrs William Hey, a most admirable person, who fell a victim to consumption at the age of thirty-five, leaving a young family of five children.

In August 1816, his daughter, Mrs Jarrat, also died consumptive at Wellington, leaving behind her a husband and seven children, some of them very young. We mention these particulars with the view of showing that Mr Hey had an ample share of those trials, both personal and family, to which all are liable, and that instead of interfering with his usefulness as a professional man, and as a citizen, they probably helped to mellow the natural hardness of his nature, making him more capable of sympathizing with the sufferings and sorrows of other people.

Mr Hey delivered a short course of anatomical lectures, interspersed with physiological and practical observations in 1801, when nearly sixty-five years of age; and again in 1803, 1805, and 1809. These courses were very popular, especially the last, and enabled him to contribute handsome sums of money to the funds of the

Leeds Infirmary. They were suggested by having the bodies of executed criminals placed at his disposal, and are signal proofs of the activity and energy of his mind, which neither infirmity nor advanced years could subdue.

In October 1812, he resigned his position as surgeon to the Leeds Infirmary, after holding it for forty-five years, during thirty-nine of which he was senior surgeon.

With the exception of his habitual lameness, he had enjoyed uninterrupted good health until 1805, when, on his homeward journey after a visit to London, he suffered from a serious disorder of the bowels. In 1808, he had a similar attack, curiously enough under the same circumstances, and at the same place, which reduced his strength so much, that for many months he was unable for any professional work. Again, in December 1817, another alarming disorder in his bowels prostrated him for three months. Although afterwards capable of resuming the practice of his profession, he never thoroughly recovered. Can we wonder at this? He was now in his eighty-second year. Even yet his eyesight was remarkably good, enabling him, as already noticed, to read and write without spectacles; his handwriting was firm and distinct; his hearing was acute. The only indication of old age was a slight failure of memory—all his other powers of mind appearing to have undergone little, if any diminution. Within six months of his death his biographer, Mr Pearson, found him able to discuss a recondite subject with all his wonted vivacity, clearness of perception, and logical accuracy; and he attributes this freshness and vigour at so advanced a term of life, probably with justice, to the following circumstances in Mr Hey's mode of living,—his habit of early rising, his strict temperance, the equanimity of his temper, and that internal peace, composure, and serenity which accompanied his unshaken religious principle, and his uniform consistency of character.

He died, after a short, and not very painful, illness on the 9th of March 1819. Comparatively little took place on his death-bed that deserves to be recorded; but he was patient, meek, resigned; and even the aberrations of his intellect evinced the habitual tendency of his thoughts towards spiritual and heavenly topics.

It will be gathered from the foregoing short, and necessarily imperfect, narrative, that Mr Hey was a man of strong individuality of character. We purpose, in the remaining part of this address, to dwell for a little upon his attainments as a surgeon, our principal object being, as already hinted, to prove that this man, who was so eminent as a Christian in a period of English history when there was less religious profession than now, was, at the same time, quite abreast, if not in advance, of his contemporaries as a distinguished practical surgeon.

Before entering upon this review of his published writings, it may be both interesting and profitable to make a few general remarks upon his professional habits, as these are represented to us by Mr Pearson, who knew him intimately, both as a pupil and as a friend.

Mr Hey had a clear and vigorous mind, which took a practical view of every subject with which it grappled, having little taste for discussions merely theoretical, and no great avidity for an extensive perusal of books. His reading was therefore select and discriminating, rather than discursive. He thought more than he read; and this is very evident from his professional writings, which are essentially memoirs or monographs upon important subjects, replete with personal experience. Such performances are often very valuable, and continue to be read with interest, long after the facts and conclusions which they first communicated have become part and parcel of more systematic works, and have thereby lost much of their original freshness and flavour. Besides this, Mr Hey, like Pott and Abernethy, and others, had the gift of telling his cases in such a way as to make the reader interested in the individuals of whom he wrote. The mere mention of a few incidental circumstances, not absolutely essential, takes away the character of baldness from the narrative, and makes you feel that the author is speaking of real men and women, to whom this world, with its accidents and vicissitudes, was once as important and as engrossing as it now is to ourselves. Cases detailed in this way are read more cordially, and make a more lasting impression, than the cold mythical, impersonal items, fit only for statistical tables, which we too often meet with in the present day.

Mr Hey, as an operator, is said to have been firm, steady, and collected. Everything was well considered beforehand; and therefore unexpected occurrences had little power to disconcert him. Very few provincial surgeons were called upon to perform so many capital operations, and probably none in his own day were more successful, yet he never became confident or rash, but continued to bestow the same thought, consideration, and benevolent care upon his cases to the very end of his long professional life.

From all that we can gather respecting Mr Hey's manner and general bearing, we conclude that his tendency was rather in the direction of hardness and austerity, particularly in the earlier periods of his life. But this was more apparent than real, as underneath it there was always a steady current of genuine benevolence, which, however, came more to the surface as he advanced in years and mingled extensively with all classes of his fellow-men. To his family, and to intimate friends, he was in the habit of freely unbending, and was a very different man from what he seemed to be in large companies, or in the society of those who were not altogether congenial.

He was very fond of music. "In his younger days he played on the flute in concert, but latterly, he chiefly amused himself by playing the compositions of Handel, Corelli, etc., in studying thorough bass, and performing pieces of sacred music on the harpsichord." The following extract from one of his letters to his daughter Alice, dated August 20, 1791, will serve to illustrate his musical taste, and at the same time to show his tenderness of feeling as a parent, and the warmth of his devotional sentiments:—"Your sister, D., brother, W., and I have been at York. I only went one day to hear the Messiah. I expected that the soft echoes, which are in the Minster, from its loftiness and fine arches, would have helped the sound much; but I was disappointed. The choruses were much too thin for the size of the building; and it was so difficult to get a good place, or to know, at first, what was a good one, that I lost the pleasure of the first part. I got much nearer the orchestra, at the ending of that part, and heard the two succeeding ones with more satisfaction. I had just been thinking of your brother, Richard, (March 1789,) before I went to the performance; the hallelujah chorus brought him strongly to my mind; and what with the solemnity of the piece, and what with the pleasing, yet affecting hope, that I had a child then singing that song with the happy spirits above, I could not help giving vent to my affections with my tears. This is our rejoicing indeed. 'The Lord God Omnipotent reigneth.' Whatever shocks may agitate the nations in this changeable and sinful world, yet the kingdoms of it are given to our Redeemer, and they will one day become by possession the kingdom of Christ, and he shall reign for ever and ever, King of kings, and Lord of lords. Hallelujah."

Let us now endeavour to bring out, in as few words as possible, the eminent claims which Mr Hey has upon our respect and gratitude for the improvements suggested by him in the practice of surgery. This will be best done by following the order of the various subjects as they appear in his "Practical Observations." The third, and, I believe, the latest edition of that valuable work was published in 1814; having made its first appearance in 1803, and the second edition in 1810.[1]

The first chapter is on fractures of the skull. It will hardly be credited that Mr Hey found it necessary gravely to combat the doctrine of Mr Pott, that a circular portion of scalp should be removed by the knife as a preliminary to every application of the trephine. Having argued that point, and successfully established the principles which guide us in the present day, he then proceeds to take for granted that it is of "still greater importance to preserve every portion of the cranium which the safety of the patient does not compel us to remove." This leads him to describe, as a convenient substitute for the trephine in many cases, those small saws, straight and circular, since called by his name, which a previous trial of thirty years enabled him to recommend with confidence.

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[1] Mr Hey published in 1779 an elaborate essay, entitled, "Observations on the Blood," which does great credit to his talents for patient and careful research.

Whether these or cutting pliers are employed is of minor importance, he did great service in discouraging, both by precept and by example, that *nimia diligentia chirurgorum* in injuries of the head which then prevailed. Let us bear in mind also the series of cases of abscess and caries of the tibia, in which he obviated amputation, and effected, with the aid of his small saws, chisels, and gouges, permanent cures quite worthy of the advanced surgery of the present day.

Mr Hey's observations on cataract may be studied with advantage by every one who takes a special interest in ophthalmic surgery. He exhibits an accurate acquaintance with the relative anatomy of the different structures of the eye. His directions as to the operations with the needle, to which he confines himself, are the result of careful consideration, and much practical experience; and not a few original observations are made which now occupy the place of recognised and well established facts in all systematic works. Mr Hey recommended a needle for the operation of couching, the idea of which he got so early as 1768 from an itinerant oculist who visited Leeds; and, judging from his own subsequent success, independently of the form of the instrument, it seems to have been well adapted for the purpose. The principle of the operation is now-a-days regarded as unsound; but this opinion has probably originated very much in want of success; and the want of success may have depended, in some measure, upon disregarding the instructions so minutely laid down by Mr Hey, and by employing needles of an improper shape.

The important subject of *hernia* occupies a conspicuous place in Mr Hey's book, and he writes upon it as one who has a great deal to communicate from the rich store of his own experience. His knowledge of the surgical anatomy is very precise, and, considering the limited opportunities which he must have had for such investigations, extremely creditable to him as a scientific surgeon. He has the merit of being the first to describe that variety of the *hernia congenita*, in which a true peritoneal sac, enclosing the protruded intestine, descends into the tunica vaginalis, and comes in contact with the testicle. The case which enabled him to ascertain the true nature of this complaint, which he did by careful dissection, occurred so early as 1764; and his original description is quite equal, if not superior, to any that have followed. He called it *hernia infantilis*, the name which it still retains.

Upon the general subject of hernia, he writes in a very enlightened manner, enunciating views and laying down principles which are so familiar to us in the present day that we make no inquiry as to their origin. For example, he shows the importance of carefully examining those parts of the abdomen which are the usual seats of hernia, whenever we are called to a patient with symptoms of *ileus.* He points out the mistake, which seems to have been common, of adopting one mode of treatment in all cases, before proceeding to the operation, regardless of essential differences in the condition of the patients. Keeping this remark in view, he discusses the varieties of procedure then in vogue with admirable judgment, pointing out the circumstances under which they are severally indicated with appropriate illustrations from his own experience. Those expedients are now almost entirely superseded by the use of chloroform; but for many years the judicious observations of Mr Hey were, we doubt not, signally useful.

He writes strongly upon the importance of not losing valuable time in renewed attempts to reduce a strangulated hernia, mentioning that, in his large experience, he had often lamented having performed the operation too late, but never that he had performed it too soon.

He discusses very intelligently a question which has received much attention of late years, the propriety of relieving the stricture, and reducing the protruded parts without opening the sac. He considers the reasons in favour of the practice in very old and large ruptures to be unanswerable, but thinks that, in ordinary cases, the advantages expected from the procedure are not to be set in competition with its dangers.

His remarks upon the management of the omentum are specially worthy of careful perusal, indicating, as they do, the thoughtful care, sagacity, and large resources which characterized him as a practitioner.

The *fungus haematodes* is always associated with the name of Mr Hey, although, strictly speaking, it was first described by Dr John Burns of Glasgow, under the name of *spongioid inflammation*. But the very first sentence of that gentleman's dissertation upon the subject shows, I think, that his view of its nature was both defective and erroneous,—"The disease which I am going to consider has either not been described at all by any author, or has, when it was noticed, been considered as of a cancerous nature." Mr Hey published a series of striking cases of the disease in question, the earliest of which had occurred so far back as June 1780, and mentions quite explicitly that, as it was a disease which had not hitherto been described by any author with whose writings he was acquainted, he had taken the liberty of calling it *fungus haematodes*, a name as expressive of its character as any he could devise. The fact is, that although his cases were observed and named at a much earlier period, Dr Burns, whose book on inflammation appeared in 1800, was the first to publish an account of the disease.

On dislocations, Mr Hey has left very valuable observations, particularly on those of the shoulder and hip-joints. They are founded upon the anatomy of the parts, and thoughtful consideration of the muscular and mechanical obstacles to reduction. When we remember that Sir Astley Cooper, and other modern authorities, had not then written upon the subject, we cannot but admire the intelligent and precise manner in which he lays down the principles of treatment in dislocation, for example, of the femur into the *obturator foramen.* He also understood, by anatomical investigation, the peculiar difficulty which exists when the head of the metacarpal bone of the thumb is luxated completely, and depressed towards the palm of the hand. He recommended the practice, now generally followed, of moderate extension, and firm pressure against the luxated extremity of the first *phalanx* in the proper direction for pushing it into its place.

Under the title of internal derangement of the knee-joint, Mr Hey described a form of injury which he supposed to consist in some displacement of the articular surfaces, probably of the *semilunar* cartilages. Occurring instantaneously either from a strain or greater violence, it causes considerable lameness, while the joint admits of being extended or bent by the surgeon without giving pain to the patient. He found that in all his cases the joint was restored at once to its natural condition, by first extending and then suddenly bending the leg backwards, until it made as acute an angle with the thigh as possible. Such cases are distinct from those in which loose cartilaginous substances are the cause of lameness; and Mr Hey deserves credit for his *diagnosis.*

On the subject of wounds opening into joints, he is very judicious, pointing out the absolute necessity of complete rest, and of attention to every circumstance calculated to promote healing by the first intention. Some interesting cases are related in which the result was much more favourable than might have been looked for; and there is one case in particular (1801) which may have contributed in some measure to that confidence with which Mr Syme revived the operation of excision of the elbow-joint, and gave an impulse to conservative surgery after a long period of disuse and neglect. The case was that of a boy of nine, in whom the elbow-joint was cut quite across on the anterior aspect, while the inferior part of the *humerus* was broken transversely about an inch and a-half above its articular extremity, the bone below the fracture being also broken in different directions. Mr Hey dissected out the whole extremity of the *humerus* from the part where it had suffered the transverse fracture, brought the integuments into contact, and placed the limb, gently bent at the elbow, upon a pillow, surrounding it with a poultice. The boy made a good recovery, and some years afterwards, when about fifteen, had a useful arm, being able to write tolerably well, and also to lift heavy weights, and do many other things with his arm in a depending position.

The chapter upon compound dislocations of the ankle-joint, embracing not only those cases in which the lower end of the tibia protrudes, but those also in which there is displacement of the *astragalus,* is well deserving of perusal. It exhibits some praiseworthy examples of successful conservative surgery.

One of these dislocations of the *tibia* occurred in September 1766, more than a hundred years ago, and really it seems to have been treated with as much skill and success as could be expected in the present day. Mr Trye of Gloucester having excised a dislocated *astragalus* with a favourable result, Mr Hey was encouraged to do the same. The operation was successful, but the patient died in a few weeks from a pulmonary disease, under which he laboured at the time of the accident. In another case, a large portion of the *astragalus* remained in contact with the *tibia*, so that excision of the entire bone would have been very difficult, if not impracticable. Mr Hey purposed sawing off the projecting portion of bone, but the patient withheld her consent. It turned out, however, that all the projecting part came away gradually in fragments without surgical assistance, and a firm cicatrix formed nearly upon a level with the skin. There was little deformity of the foot, and the patient could walk eight or ten miles without inconvenience.

The subject of retention of urine received much elucidation from Mr Hey, who was the first, so far as I can see, to clear up certain phenomena which are now fully understood by well-informed surgeons, but still often mislead the uninitiated. For example, he taught and illustrated by striking examples that a retention may exist when the flow of urine from the bladder is by no means totally suppressed. Sometimes the complaint may be mistaken for a strangury, when, after violent efforts put forth at intervals, the urine is expelled in small quantities. Sometimes the retention is supposed to have come to an end, because the patient can make water in a stream, whereas the use of a catheter would show that the bladder continues distended, a state of things otherwise indicated by a hard and painful tumour in the *hypogastrium*. Sometimes, and this is very apt to happen, the retention is mistaken for incontinence of urine, when the bladder having suffered its utmost distention, the urine dribbles off by the *urethra* as fast as it is brought into the bladder by the *ureters*. The importance of ascertaining the exact nature of the case cannot be over-estimated, because neglect in relieving the distended bladder must lead to disastrous consequences.

He gives very precise instruction, founded upon the anatomy, for passing an instrument into the bladder; and to him we are indebted for the well-known device, in cases of enlarged *prostate*, by which the introduction of a gum elastic catheter into the bladder can be greatly facilitated by a partial withdrawal of the stylet.

He dwells a good deal upon the risk of forming false passages, and shows that the accident often happens from pushing forward the catheter before its handle has been depressed.

He is decidedly of opinion, that a patient with retention of urine regains, as a general rule, the power of expelling it much sooner when the catheter is withdrawn after each operation, than when it is left in the *urethra*. He gives an important rule of practice in such cases,—that the use of the catheter is to be regulated, not by the quantity of water expelled, but by the patient's power of emptying his bladder. He makes two remarks in connexion with this subject, which experience certainly confirms: the first is, that the quantity of urine secreted during the night is much greater than that during the day; the second is, that a very copious secretion of urine may be generally expected to follow the first use of the catheter, when the retention has existed for some time, so that the interval betwixt one introduction of the instrument and another ought to be regulated in accordance with both of these observations.

In one of his recorded cases, where considerable hæmorrhage took place from the bladder, it is interesting to observe that Mr Hey prescribed twenty drops of the tincture of the muriate of iron every six hours, as we might do in the present day, and evidently with a good result.

The treatment of *procidentia* or *prolapsus ani* was greatly improved by Mr Hey. Instead of being contented with mere palliatives or deceptive modes of cure, he studied the complaint thoroughly, and introduced an operation which is essentially the same as that which we perform in the present day. It consists in cutting away with curved scissors the loose fold or folds of skin around the margin of the anus; thereby causing a firmer adhesion of the *rectum* to the surrounding cellular tissue, and enabling the *sphincter* to contract with increased power and efficiency.

Cancer of the *penis* has been well illustrated by Mr Hey, in a series of twelve cases, in various stages of development. He drew special attention to the fact, that no fewer than nine of these had been affected with congenital *phymosis*, and this led him to ask the question,—"Ought not the phymosis to be removed in early life in all cases where it is congenital?" Knowing, as we do, that a local source of irritation, such as *phymosis*, can only induce malignant disease in persons constitutionally prone to it, this proposal is perhaps too comprehensive; but it should be borne in mind, that the presence of a *phymosis*, besides its inconvenience, even in childhood, is a source of irritation, and aggravates every affection to which the prepuce is liable.

In amputation of the *penis*, he cut right through the organ, as is done now, without retracting the integuments, and then secured the bleeding vessels with ligatures. On one occasion, he brought the skin over the divided *corpora cavernosa*, hoping that the wound might heal by the first intention. To prevent puckering, he made a longitudinal division of the skin at the inferior aspect of the *penis*, so as to cover the *corpora cavernosa* without covering the *urethra*, introducing, at the same time, a small silver canula into the urethra. He failed to procure primary union, but the wound was cicatrized in five weeks, and the remaining portion of the *penis* did not retire, as is usually the case, within the *scrotum*. He recommended a bougie to be introduced in all cases for a short time every day, to prevent contraction of the orifice.

The two next chapters contain severally the record of two interesting cases, which are remarkably well told, and repay a careful perusal. The *first* is that of a gentleman who attempted to hang himself, and was cut down by his son in the very nick of time. A neighbouring surgeon bled him largely, and convulsions immediately supervened, which lasted for an hour, when Mr Hey was sent for. His treatment was very judicious and successful. Warmth to the extremities, which were very cold; and for a very feeble pulse, the administration of warm wine and volatile tincture of valerian, by means of a large spoon inserted betwixt his teeth during a short interval of quiet, the fluids being poured into the spoon while his teeth were kept asunder by it. The convulsions ceased instantaneously upon the mixture being swallowed; but they showed a tendency to recur, unless the remedy was repeated at intervals. The patient eventually recovered.

The other case was a tumour situated in the neck of an infant, immediately above the clavicle on the left side. It was of rapid growth, having attained the bulk of a pigeon's egg in four days. Mr Hey, upon a consideration of all the circumstances, regarded it as varicose in its nature, and probably connected with the external jugular vein. In a fortnight it had increased to four times its former size, descending below the clavicle, and rising as high as the angle of the lower jaw. He now saw reason to believe that the fluid in the tumour was extravasated, and made an exploring puncture with a small round couching needle. Dark-coloured bloody fluid issued in a small stream, and continued to ooze out for two hours. The wound healed. Next day he punctured again with a spear-pointed needle; a small quantity of blood, brighter in colour, issued out *and coagulated*, whereas the first had remained fluid. Three days elapsed, and a third puncture gave exit to a small quantity of blood, the remainder of the tumour feeling solid but soft, like coagulated blood. A few hours after this last operation, the tumour became rapidly larger, and went on increasing for two days. It then became stationary; at the end of a week another puncture was followed by florid arterial blood coagulating firmly. After this, the tumour gave no further trouble, and was gradually absorbed.

Was this a hæmatocele of the neck, evacuated by the first puncture, and cured by the formation of a coagulum in its sac,—which coagulum had originated in the wounding of a vessel by the spear-pointed needle at the time of the second puncture?

Mr Hey takes occasion to recommend the use of an exploring needle in cases of doubtful tumours, a practice not unfrequently adopted in the present day.

An interesting case of *empyema* is narrated, which Mr Hey treated successfully by making an incision of two inches through the integuments and *serratus magnus*, close to the upper edge of the sixth rib, and an opening into the chest large enough to admit the tip of his finger. Five ale-pints of purulent matter were evacuated; and a leaden canula, introduced on the second day, was worn for fifteen months. New milk, with rice and bread, constituted the patient's principal articles of diet, and the recovery was complete. It is worth noticing the symptoms upon which Mr Hey founded his *diagnosis* in the absence of those physical signs which guide us in the present day. The early symptoms were febrile disturbance, with acute pain in the *left* side, relieved temporarily by venesection and blisters. There was increasing *dyspnœa*, and the patient could not lie on his *right* side without danger of immediate suffocation. On the twenty-eighth day, when the operation was performed, the face, especially the eyelid, was a little swollen on the *left* side; the *left* side of the chest was larger than the right, and its integuments were œdematous; the intercostal muscles felt distended, yielding a little to strong pressure and rebounding again.

Mr Hey attaches great importance to retaining the *canula* in the wound until all probability of a relapse is removed. He appends a case in which a youth received the whole charge of a fowling-piece into his side, the muzzle of the gun being very near him at the time when it was fired. The *canula* was retained for twelve months, although taken out and cleansed every day, and pellets of lead and small fragments of bone were discharged through it, now and then, for a long time. The cure was complete.

The next chapter is occupied with the case of a delicate girl of fourteen, in whom the mammæ enlarged to such extraordinary dimensions that a permanent curvature of the spine was brought on by their weight, as she was unable to walk upright, and usually sat with the breasts resting upon her knees. The glands had been rather larger than ordinary from infancy, but had grown very rapidly after a sudden obstruction of the menstrual discharge at the time of its first appearance. Mr Hey, remembering the intimate sympathy subsisting betwixt the mamma and uterus, removed the left breast, which was the larger of the two. It weighed 11 lbs. 4 oz. avoirdupois. Menstruation soon returned, and became regular; and at the same time the right breast soon diminished in bulk to a marked degree, and still more so during a fever which she had about six months after the wound was healed. Fifteen years afterwards the right breast was not half so large as it had been before the operation, and the patient had become a healthy woman.

Two interesting cases are narrated under the title, "Collections of Pus in the *Vagina*." One of them, which an ordinary practitioner might have regarded as a case of *fluor albus*, was remarkable for irregular and sudden discharges of yellow matter, which the patient could not explain, sometimes ceasing entirely for a time, sometimes gradually diminishing. The parts were irritated by the fluid. Mr Hey discovered a considerable cyst where the labium of the left side joins the *vagina*. He laid open the cyst, which was an inch and a half in diameter, and removed the greater portion of its wall formed by the mucous membrane of the *labium*. The wound healed kindly.

The other case was similar in its symptoms, as described by the patient, who had been affected for sixteen years. A roundish tumour was discovered at, as Mr Hey considered it, the bulbous part of the *urethra;* for, when compressed, pure pus issued from the *urethra*. He divided the swelling longitudinally, and filled the cavity, which was smooth, with lint. A perfect cure was the result. Had it been an abscess of one of Cowper's glands?

Mr Hey narrates three cases of alvine concretions which deserve perusal. In one of them, which, accurately speaking, was an instance of fæcal accumulation in the *sigmoid* flexure, he eventually succeeded in relieving the patient by carefully introducing a long flexible catheter through the mass, and then injecting a mixture of gruel and olive oil in equal proportions. This procedure required to be repeated night and morning for a fortnight, and the patient ultimately recovered. The treatment, although familiar to us, was a novelty when introduced by Mr Hey.

His remarks on the *atheroma*—including, under that name, encysted tumours of the eyelids and *talpæ* of the scalp—are very judicious, and indicate a complete knowledge of the subject. He prefers removing the former by an incision in the line of the fibres of the *orbicularis palpebrarum*, by evacuating the contents, and then pulling out the cyst with a pair of dissecting forceps. This mode is perhaps more expeditious and complete, but, at the same time, more severe than the present mode of everting the lid, opening the cyst, and stirring up its contents with a probe.

The subject of amputation received ample justice from Mr Hey. He was fully alive to the importance of healing the wound, as often as possible, by the first intention. "I am sorry," he says, "that this capital improvement is not yet universally adopted. If I were not aware of the force of long-established opinion and practice, I should be ready to conclude that a surgeon was defective either in knowledge or humanity who did not prefer this method whenever it was in his power to make use of it." In the mode of performing the circular amputation he adheres very much to that of Alanson and B, Bell, but advises that the *posterior* muscles in the thigh should be divided at a lower level than the *anterior*, to make allowance for their greater range of contraction.

He was at great pains to improve and lay down precise directions for the amputation below the knee; and there can be no doubt that the operation which he recommended was a great improvement upon those commonly performed at that time. But, in the present day, owing to Mr Syme's admirable operation at the ankle, and the improved operations at the lower end of the femur, very few amputations of the leg are required, and those are generally performed just below the tuberosity of the tibia, with two equal semilunar flaps, or, much lower down, with a long anterior flap.

But it is with partial amputation of the foot that his name is chiefly associated. "Hey's operation," as it is called, consisted in removing that portion of the foot which is in front of the *tarso-metatarsal* joint. It was a very creditable example of conservative surgery at a time when little was attempted in that way, and doubtless saved many limbs from needless mutilation. But this operation, as well as that of Chopart, is now seldom performed in this country, being superseded by Syme's amputation at the ankle-joint.

Our author deserves the credit of being the first to recognise the nature of those tumours of the nose which depend upon hypertrophy of the integuments. The disease had been described previously in the Memoirs of the Academy of Surgery in France, and two successful operations had been detailed; but the writers considered the tumours to be carcinomatous. Mr Hey describes a very aggravated case upon which he operated with perfect success; and I am not aware that the sixty-six years which have elapsed since the case occurred have either added to our knowledge of the disease, or suggested any improvement in the operative procedure.

It cannot be doubted that the man whose life and works we have been considering was eminently useful while he lived, and deserves well of posterity when he is gone. Practising a laborious and anxious profession in spite of habitual lameness and infirmity, he found time for recording his experience and publishing a work of enduring value. Moreover, he took a warm and active interest in numerous schemes of benevolence and missionary enterprise, maintained an extensive correspondence with William Wilberforce, Isaac Milner, Dr Jowett, and other distinguished contemporaries, and on two occasions filled the office of Mayor of Leeds in difficult times with great conscientiousness and efficiency. Taking all these things into consideration, and remembering, in addition, that he was remarkable during the whole of his professional life for his high-toned and consistent piety, we are strongly of opinion that few characters exhibit so many features deserving of unqualified admiration as that of the good and able man whom we have endeavoured to portray.

ARTICLE II.—*On Chloroform in London.* By CHARLES KIDD, M.D., Member Royal College of Surgeons, England; Associate Mem. Surgical Society, Ireland, etc.

THE administration of chloroform continues, and is likely to continue, the modern blessing of widest application in the world. Natural facts have outlived the bodings of doubt and evil which early beset them. Nor does it wisely follow that, because our grandmothers did without anæsthetics, that now, in the sorest trial of female life, to please ancient London prejudice, we should still adhere to the theories or inductions dangerous of the earlier writers.

Ten years' additional experience have corroborated previous ten as to the remarkable safety of chloroform, if administered with steadiness, delicacy, and attention.

Twenty years' experience convinces me that, like Prospero, we must bury our books of experimental magic. Experiments on animals are only "part of the truth"—often fallacious, like those of Orfila, which indicated death and nervous desolation from ingestion of very simple medicines in animals; but where he forgot to state that he had cut the animals open and tied strings round the œsophagus, which strings had also constricted important nerves. The danger in chloroform seems to be from depressing mental impression in the patient, small tedious doses, anxiety at the idea of losing consciousness slowly, fear of the result of operation, etc. We do not meet death in the else sleepless troubles of midwifery, because the chloroform, if taken, removes depression and fear of coming pain, the respiratory system is active, and removal of pain gives strength, sleep, and confidence.

Tens of thousands of such cases now crowd on us without one accident; tens of thousands of eye operations by Graefe and others with a similar immunity from danger; hundreds on hundreds of ovariotomies, amputations, hernia cases less dangerous with, than without chloroform.

Every year helps to prove that chloroform is free from danger, if we could but get rid of the earlier errors as to fatty heart, danger of napkin, necessity of mixtures, patented inhalers, etc.

Ether was supposed to be safer than chloroform. Experience of both has, unhappily, only fortified the belief that there is not much difference in this particular. It is best, too, to administer them alternately, or in separate inhalers, rather than mixed. The nineteen accidents of Trousseau by ether now merge in thirty-six given by Sabarth. All late deaths have been from "mixtures."

The law of tolerance is a "true thing." In one hundred ovariotomy operations personally superintended or watched as to the chloroform administration, where full anæsthesia was quickly induced, the chances of danger seemed far less than in an equal number of tooth-drawing cases. It is untrue, too, as stated by advocates of ether spray or Clover's apparatus, that the pulse sinks in the former cases.

There can be little doubt that accidents are more frequent in the *early* stages of anæsthesia, when reflex action is excited or only half-abolished. Emotion or fright then, too, is most active. Three hundred accidents from anæsthetics bear out this view: reflex action stopping the heart through the par vagum in the lung. The heart's action is less active during the excitement stage as a rule, due to the inhibitive function of the vagus. It may even intermit, but by continuing to full anæsthesia, the pulse becomes equable, steady, and full. No doubt, if an animal be cut open, we reverse all the phenomena.

Nearly all our London writings on anæsthetics have been founded on such vivisections, in oblivion of the principle of "emotion." As emotion will act on the uterus or stomach in digestion, so will it influence the heart, causing apnœa and convulsion. A far more serious fact is this, than that chloroform dropped on the exposed heart of a frog will induce disturbance or death, the latter probably arising as much from the violence done to the animal as the effect of the chloroform.

Mention was made by me twelve years ago of the views of Brown-Séquard on these points; but "fatty heart," and the opposition of Marshal Hall, were in the ascendant, and retarded progress in anæsthetics. A multiplicity of opinions still exists on chloroform, its benefits, mode of action, etc. We want very much the exact previous history of patients who have died of chloroform. In four cases that were examined by me, but pronounced healthy, a previous history of delirium tremens or hysteria was made out in each; in another, organic disease of brain was found.

The condition of the eye under chloroform is a very useful guide to the experienced administrator, much more so than the regulation of unnecessary balloons and valves and bellows. The latter, says a French author, are "not only dangerous, but a cause of danger." The pupil contracts in the stage of excitement under chloroform, an indication of irritation of the cerebro-spinal centres and stimulus to the circular fibres of the iris; but in the later, or third stage, the pupil is dilated, depending on a suspension of the function of the third nerve. Vomiting takes place in the former stage, stertor in the latter. This fact teaches how to avoid vomiting, and when.

Chloroform accidents have a remote analogy to railway accidents. The public do not always avoid railways because of this danger, or the sea because there are shipwrecks; the two things at sea are a chart and good pilot. But our one-sided essays now on mixed vapours, balloons, ether spray, etc., would abolish chloroform, and gird us with *robur et aes triplex* brass valves and mixtures, as Horace, in the familiar ode, could only fancy the father of the winds investing that hardy mortal who first ventured out of sight of land. All which lessens the confidence of the public in the safety of pure chloroform, but reacting in favour of instrument makers.

We want still a theory of the action of chloroform that will agree with the many deductions of clinical experience. But everywhere else now in the world, our English views have been questioned as to the necessity of specific inhalers, danger of fatty heart, etc.

The opinions, after twenty years, of MM. Giraldes, Sabarth, Lallemand, Perin, Duroy, etc., as well as various American writers, disagree widely with the views of the Committee of our London Medico-Chirurgical Society. The asphyxia of these physico-chemical experimenters is held to be rather asphyxia of animals in closed vessels, and their inductions in opposition to all experience in surgical practice. Chloroform has, this year, been tried with very beneficial effect in the spasms of hydrophobia and tetanus, the neuralgic cramp of cholera, the agony of gall stones, strychnine poisoning, and in preventing the pain experienced in the compression or Dublin method in aneurism.

It is a moral duty to explain these points: as we have known ovariotomy and acupressure opposed, so with chloroform: the "grandmother argument" pervades, too, all our obstetric circles, and so the utmost quackery, in shape of patent "mixtures," in midwifery, is taking the place of the proper skilled use of simple chloroform,—Miller's remarkable "law of tolerance" ignored for some balloon or bellows, of no real value.

ARTICLE III.—*Cases illustrative of the Insanity of Pregnancy, Puerperal Mania, and Insanity of Lactation.* By JOHN B. TUKE, Esq., M.D., Medical Superintendent of the Fife and Kinross District Lunatic Asylum.

THE object in compiling the following series of cases is to illustrate, as far as possible, the facts which seem to me to have been educed from the statistical inquiry into the symptoms and peculiarities of puerperal insanity, as observed in the Royal Edinburgh Asylum, published in a former number of this Journal.[1] Since that time I have met with several cases which have strongly confirmed my opinions based on that inquiry, and strengthened especially the more important results as to diagnosis, treatment, and prognosis. Further consideration and observation of this series of diseases, generally classed under the one head of puerperal insanity, viz., insanity of pregnancy, puerperal insanity, and insanity of lactation, have led me to further conclusions which were not embodied in the original paper, conclusions which tend to point out more fully the necessity for considering them as separate and distinct forms of disease. It is not presumed that these observations are entirely original; there is little doubt that the same remarks have been made by physicians under whose care such cases are placed, but no notice is taken in any systematic work on mental affections of many of the salient points of the disease, and some have been omitted in the pamphlets especially devoted to the subject.

On reflection, a difficulty suggested itself in the fact that these diseases very often run one into the other,—that the insanity of pregnancy does not generally cease with labour, and that mental symptoms occasionally occur within two or three months after delivery, before the system of the patient could be so exhausted by nursing as to induce insanity from anæmia,—thus rendering accurate classification difficult or impossible. Again, the variety in the form of aberration in each class militated against the idea of pathognomonic symptoms. But a careful revision of the tables obviated these difficulties to a very considerable extent, and proved beneficial in demonstrating more fully that, with very rare exceptions, the several diseases are evidenced by well-marked, peculiar, and persistent indications.

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[1] May 1865, p. 1013.

On glancing over the tables (pp. 1025–1028, vol. x.), it will be seen that there is a marked preponderance of certain mental symptoms in the aggregate of each class. Thus, in insanity of pregnancy 20 are reported as cases of melancholia, 6 as moral perversion and dipsomania; in puerperal insanity proper 53 as cases of acute mania, 15 melancholia; in insanity of lactation, melancholia 39, acute mania 10. The balance of exceptional cases, dementia and epileptic insanity, seemed so slight as to lead to the suggestion that they were not exceptional, but that circumstances connected with them had not been ascertained, or that material symptoms had been overlooked in the report. In many instances this proved to be true, in others the lapse of time was so great as to make inquiry impossible.

To a certain extent there always exists a source of fallacy in observations based on the case-books of a public hospital. This depends on the imperfect, at times false, information obtained with the patient. This remark extends to all classes of the insane, rich or poor, as regards the obtaining of a history of previous attacks, hereditary predisposition, and even the earlier symptoms of the disease. I reiterate this common complaint of all asylum physicians, partly in order to excuse us from a not unfrequent accusation, that of not inquiring into the previous history of patients, and of ignorance of very important facts tending to an accurate diagnosis of cases placed under our charge. The fault seldom lies with us. Amongst the higher classes relatives suppress the truth, or even pervert it. In the case of paupers, inspectors of poor frequently, from negligence, ignorance, or carelessness, do not furnish the information demanded in the statutory statement, which is in itself insufficient. It is careful in its inquiries as to the age, sex, and religious denomination of the patient, but does not insist on important points, such as previous habits, state of health, disposition, and character. This statement not only does no good, but often misleads, but could be rendered of great benefit if, more particularly in the case of paupers, the parochial surgeon were required to furnish such information instead of the inspector of poor. In certain cases this is impossible; but in a large number, the medical man of the parish is acquainted with the whole history of the patient, but has no opportunity of notifying to the asylum physician facts material to the proper treatment of the case. In a disease dependent upon actual structural derangement, but which is often only characterized by mental alienation, too close inquiry is impossible; and were we furnished in every case with a full and particular report, there can be little doubt that a larger proportion of recoveries would ensue. It must consist with the experience of every alienist that where an accurate history of the bodily ailments has been furnished, the patient has soonest recovered mental health, or if such was not the happy result the minds of friends have been satisfied by a distinct prognosis.

These observations may seem beside the mark; they were, however, suggested by the consideration of those cases whose symptoms seemed exceptional to the mass. But it may be well to examine separately each of the diseases referred to.

Insanity of Pregnancy.

Out of a series of 28 cases of this form of disease, only two are reported as characterized by mania. On more particular inquiry, it was discovered that one of these women had suffered from an attack of true puerperal insanity after a previous confinement, of which no history had been afforded, and which was only ascertained by the merest accident. Moreover, she had been subject to recurrent attacks at intervals, and therefore it was to be expected that the affection, with the symptoms of mania, should reappear during pregnancy, predisposed as she was to insanity in that form. The other case occurred too long ago for inquiry; but as it stands solitary in a long series, there is presumptive evidence that it was of a similar nature with the one last alluded to. Mania is not the symptom one would expect to appear during pregnancy; melancholy, with or without moral perversion, seem not unnatural consequences. There are few fecund women who do not evince some symptoms, however slight, of a reflex affection of the faculties in the form of morbid appetites or longings, or change of temper and disposition; should these go on to actual insanity, moral perversion or melancholy are the most *likely* results, and, as shown by experience, are so. I think it will be found that in those rare instances where mania occurs, the patient has previously been the subject of insanity in that form, and therefore that we are justified in laying down that melancholy, occasionally associated with moral insanity, is *the* mental symptom characterizing the insanity of pregnancy.

Cases are on record in which the insanity of pregnany is said to have disappeared with labour. This does not seem to be a common result; in fact, even if the mental symptoms abate or entirely disappear before confinement, the rule is that they again present themselves. If they persist up to that period, an improvement is often noticeable for a week or more; but, as far as my observation goes, and it is confirmed by that of others, they generally crop out again for a shorter or longer space of time. This tendency to recurrence will, however, be more fully alluded to when considering puerperal insanity proper.

The following cases seem to me to illustrate the insanity of pregnancy pretty fully:—

A. B., married, æt. 23, wife of labourer. Her mother became insane late in life.

This woman was naturally of cheerful disposition, steady, and industrious. During the third month of her first pregnancy she became very much depressed in spirits, and took a strong aversion to her husband. She suffered much from the usual symptoms of pregnancy. The melancholy rapidly became more intense. She attempted suicide by cutting her throat, but did not succeed in hurting herself severely. She never evinced any definite delusion. She was for three months under treatment, and improved so much as to be to all appearance well, and was discharged recovered. She was confined in due course; the labour was natural, although somewhat prolonged. Three weeks after, the depression and melancholy returned, and she was again sent to the asylum, where she remained for three months, gradually improving, and was again discharged recovered. She has born two children since then, but no recurrence of the malady has been observed.

A. C., æt. 21, married, wife of tradesman. Her sister and maternal aunt have been insane, the latter under treatment on three several occasions in an asylum.

During the fifth month of her first confinement she became very low-spirited and depressed. She attempted suicide by drowning, but did not succeed in her intention, from the shallowness of the water, although she persevered for several hours. The attempt was made in the sea, where the sands were not deeply covered, and extended so far out as to make it difficult or impossible for her to reach deep water, so that as the tide receded she was always left high, if not dry, after each effort to effect her purpose. From this melancholy state she partially recovered, and her baby was born in due course. Eleven weeks after its birth she deliberately strangled it, and then attempted to poison herself with laudanum. At the instance of the Procurator-fiscal she was visited in prison by Drs Maclagan and Skae, who reported on her case. On the various occasions these gentlemen saw her she seemed happy and contented, exhibiting symptoms of a morbid exaltation, talking of the prison being a palace to her, and was occasionally mildly excitable. On certificates being granted that she was convalescent from an attack of "suicidal and homicidal insanity," she was removed to Morningside. On admission she seemed perfectly happy, made herself quite at home, and settled at once to work. Her mind was evidently very weak, she was facile and reserved. A few weeks after her admission, I got her to converse about her child, and her motives for destroying it. She was not in the least confused, nor did she seem to appreciate her position. She said that her impression at the time was that it would be happier if it was dead, and that she attempted suicide so that her husband might not be taunted with having a murderess for a wife. She expressed no remorse or regret. She continued in this state for nearly two months, when she again became depressed and melancholy, crying bitterly at times, as she said, about her child, but in no way alluding to her own guilt. At the next menstrual period she again took a low turn, but not so severely as on the former occasion.

Both attacks lasted about a week. She soon afterwards began to improve, her expression brightened, she worked cheerfully, saw her husband frequently, and joined in all the amusements. She became a great favourite both with attendants and patients. Menstruation was always regular. Six months after admission she was discharged, on the authority of the Procurator-fiscal, recovered. Within two years after her discharge she was again confined. She passed through her pregnancy without a bad symptom, and made a good and perfect recovery. Her mind was not affected.

B. E., æt. 35, wife of tradesman; naturally cheerful, although thoughtful and reserved; steady and industrious; educated in one of the public hospitals; mother of several children. There exists strong hereditary predisposition, her mother having died in one of the Musselburgh asylums, which event, and the death of one of her children, are the alleged causes of her insanity.

She admits that, during the third month of her last pregnancy, she took morphia with the intention of committing suicide, and also bought oxalic acid for the same purpose; the oxalic acid she threw away, and now has the delusion that she thereby caused the death of ten children; she also thinks that she is constantly watched by the police, and must suffer a public death for her crimes. Bodily health weak; pale and bilious.

These delusions she seems to have kept to herself till a month after her confinement. On her admitting them to her friends she was sent to the asylum. It is possible that they never existed till subsequent to delivery, and that not till then delusion complicated the symptoms of melancholy. On admission she was dull, anxious, and fearful, had a vague dread that she was to be taken away somewhere, was idle, listless, and desponding. When asked how she was, she only answered, "miserable." She was ordered nourishing diet, iron, and other tonics. No great improvement manifested itself for upwards of four months, when she began to work a little, and occasionally admitted that she had no real cause for anxiety. A marked change for the better, however, came over her after a very severe attack of influenza, the delusions were much more under her control, and the lucid intervals became more frequent and prolonged. Seven months after admission, she was discharged on the sanction of the General Board of Lunacy, although not quite well, but it was thought that change might be beneficial. It happily was so, and she is now in the full possession of her faculties.

It would be needless to cite more cases of a similar nature, although I could easily do so. No two cases are *exactly* alike, but the great leading symptoms agree, differing no more than is observable in every disease according to idiosyncrasy or circumstances.

The case of A. C. was certified as one of "suicidal and homicidal insanity," but this was done solely for the purpose of giving a name. The act of homicide was distinctly recognised by the eminent physicians, who certified as the result of a perverted train of thought consequent upon a peculiar bodily condition. Homicidal impulse accompanies many other well-marked forms of insanity, —*e.g.*, epileptic insanity,—but no one would consider a homicidal epileptic a case of "homicidal insanity," any more than he would designate the albuminuria of scarlatina "Bright's disease," simply from the fact that this symptom is common to both maladies. The cases stand apart. A homicide is committed by a puerperal female, —hers is a case of puerperal insanity. A similar act is committed by an epileptic,—his is a case of epileptic insanity. Albuminuria supervenes on scarlatina; this does not make the case one of Bright's disease, although the same symptom results. No insane impulse can exist independent of a causating structural change. To class all homicidal lunatics together is simply begging the question; we might as well content ourselves with calling all exanthemata by the common name of spotted diseases without further distinction.

The last case I shall instance of this form of disease is one in which melancholy was associated with moral perversion in the form of dipsomania. It seems doubtful whether the latter ever exists by itself; if so, it is rare. The strong probability is, that it is the result of the depression—the patient takes to stimulants to alleviate her distressing sensations, and in so doing becomes subject to the usual results of drunkenness. Should the case not improve, the moral perversion overrides the original melancholy, and, as in the following instance, becomes the leading characteristic of the insanity.

J. M. B., æt. 26, married; of limited education; naturally of cheerful temperament. Her mind was affected after her second pregnancy, but she made a good recovery. She had also a slight return after her third and fourth confinements. On her becoming pregnant for the fifth time, a great change came over her whole nature; she became at first irritable, unsettled, and excitable, and then untruthful and intemperate, pawning or selling her household effects, her own, husband's, and children's clothes to procure drink; was lost to all sense of decency. As gestation advanced, she exhibited suicidal tendencies, and at last became so dangerous to her husband as to make her removal to an asylum imperative. This state of matters was directly opposed to her natural disposition. From being cheerful, she had become melancholy and depressed, on which the more serious symptoms had supervened in the order above described.

On her admission she soon showed the character of her disease in lying, stealing, annoying her fellow-patients, pretending various ailments, and generally making as much fuss and getting as much attention as possible. On several occasions she tried the effect of hysterics, but soon gave them up when she found that they did not attract much sympathy or notice. She invented the most ingeniously contrived stories reflecting on the character of all around her, fully understanding the "lie with a circumstance." This state of matters continued for nearly three months with but slight amelioration, when a sudden improvement took place; she began to work a little in the gallery, ceased to steal and lie, and her conduct and demeanour became quite natural, kind, pleasant, and agreeable. Her recovery took place at the commencement of the fifth month of pregnancy.

She was confined in due course, but shortly afterwards the melancholy returned, and she made a most determined attempt at suicide by taking a considerable quantity of black hellebore. From the effects of this drug she was saved by the use of the stomach-pump. On her readmission she was deeply melancholy, and suffered much from the milk. By the usual appliances she was soon relieved from this; and tonics, iron, and good diet were administered, with such good effects as to enable her to leave the asylum in two months, quite well.

Three months after her discharge she again became pregnant, and again the moral perversion returned in, if possible, greater force than on the previous occasion. To it was now superadded a series of delusions as to her husband and neighbours of a suspicious character. From this it was prognosed that the disease would be much less amenable to treatment. This state of matters went on with little intermission till her child was born, a fine healthy boy; the labour was tedious, but she made a good recovery. The following is the entry in the case-book three months afterwards:—"No improvement in her mental condition. A more complete moral perversion could not exist in any one. She lies, steals, tells the nastiest stories without a blush, has not a grain of gratitude in her composition, invents the most dangerous stories against those who have been kindest to her, and seems, in fact, to be an incarnation of evil. She became quite unbearable in the sick-room, so her baby was weaned, and she was removed to another part of the house." Again, four months after,—"She broke a number of panes of glass to-day. Judgment, powers of reflection, and self-control much impaired. Neat and tidy." Subsequently, she again improved, and was removed by her husband; but it has since been ascertained that, being again pregnant, her old malady has reappeared. She is now a patient in the asylum.

Puerperal Insanity.

There is no section of the tables illustrative of puerperal insanity which I am so anxious to amend as Section 5, Table II., page 1026, May 1865. Out of 73 cases of puerperal insanity, 53 are there stated to have been characterized by acute mania, 15 by melancholia, 4 acute dementia, and 1 epileptic insanity.

There is not any system of medical nomenclature which leaves such a wide margin for the taste and fancy as the existing one of mental diseases; and, consequently, we often find one man calling a case one of melancholia which another designates dementia, or a difference of opinion arising as to acute dementia and mania. The distinction between the last two forms of aberration is so very slight and ill-defined as to render them mere matters of degree. Unluckily, in compiling the tables, the nomenclature of each reporter was adopted, and thus this section became complicated, as it was found, on further inquiry, that the symptoms in the four cases of so-called acute dementia were equally those of mania,—in fact, in some respects preponderated strongly in favour of puerperal mania. I would therefore amend this section by classing these 4 acute dements under the head of acute mania, thus making 57 cases of acute mania out of a total of 73. The one instance of epileptic insanity is certainly exceptional, and must be regarded as such. 15 cases remain under the head of melancholia. Regarding these, the curious fact was made out that none showed symptoms of insanity until sixteen days after labour. The liability to recurrence of the melancholy originating during pregnancy, after labour, suggested the strong probability that not a few, at least, of these melancholy puerperal patients might have been the subjects of morbid depression previous to confinement. Inquiry was made in the very few cases in which it was possible to do so, and the conviction was strengthened, although not positively confirmed. My opinion on this point is based on cases which have been under my own observation, and it is that the insanity of pregnancy frequently recurs after labour, and that it never does so in any other form than that of melancholy. Within the last few weeks two cases have come under my care which bear out this idea. Both were much depressed (one suicidal) during pregnancy; in both the same symptom returned about a month after confinement. I believe puerperal insanity to be a thing by itself, characterized by a constant train of symptoms of a maniacal character, and that the melancholy which occasionally supervenes some time after labour is but the recurrence of the insanity of pregnancy. This seems the more likely when we consider that the excitement consequent on confinement has almost abated by the time when this symptom presents itself, that the patient has, in the large majority of cases, returned after sixteen days to the condition she was in previous to labour, and that her constitution has not had time to be debilitated by nursing.

It has been said by no less an authority than Dr Gooch that no physician could, by simply looking at and examining a patient, diagnose that hers was a case of puerperal insanity, unless her history was at the same time afforded him. This I take the liberty of doubting, and will instance a case in point. A woman was brought some months ago to the Royal Edinburgh Asylum by the police, under a certificate of emergency, in a highly maniacal condition. No information could be afforded further than that she had been found in this state by the police, and had been at once removed to the asylum. There was no other evidence whatever; but the physicians who saw her on admission, before she was taken to the ward, immediately came to the conclusion that she was suffering from puerperal mania. Within a few days the inspector of poor of her parish informed them that she had been confined five days previous to her admission.

The puerperal maniac has symptoms which, as a rule, cannot be mistaken for any other form of insanity, with perhaps one exception,—mania *a potu;* but even here there are points of diagnosis which are very prominent. The bodily symptoms are at direct variance with the mental. She is pale, cold, often clammy, with a quick, small, irritable pulse, features pinched, generally weak in the extreme, at times almost collapsed-looking. But withal she is blatantly noisy, incoherent in word and gesture; she seems to have hallucinations of vision, staring wildly at imaginary objects, seizes on any word spoken by those near her which suggests for a moment a new volume of words, catches at anything or anyone about her, picks at the bed-clothes, curses and swears, will not lie in bed, starts up constantly as if vaguely anxious to wander away, and over all there is a characteristic obscenity and lasciviousness. Suicide is often attempted, but in a manner which shows that it is not the result of any direct cerebration; she may wildly throw herself on the floor, attempt to jump from the window, or draw her cap-strings round her throat, but there is no method about it, it is an impulse, the incentive of which is purely abstract.

This description of course applies to the severest class of cases, but it is taken from the recollection of not a few. Even where the symptoms are not so acute the same *tone* exists; and the shorter the time after its supervention, the more acute and marked the mania, and more rapid the recovery. As was remarked in the original paper, masturbation is sometimes noticed, but this is the result more of a wish to allay than excite irritation. The obscenity of word and manner often continues for a time during convalescence.

Since the first paper was published, several cases of puerperal insanity have come under my notice. In these particular attention was directed to the state of the urine, and in none was the presence of albumen detected, with one exception, and in that the lochial discharge was excessive. It has been said in describing the symptoms of puerperal insanity that the patient "is pale, cold, often clammy, with a quick, small, irritable pulse, features pinched, generally weak in the extreme, at times almost collapsed-looking." In the work of Drs Tuke and Bucknill on "Psychological Medicine," p. 259, it is stated as follows:—"There is, however, a class of cases in which the pulse and other symptoms indicate an inflammatory condition of the system, and such cases are of a much more serious character. Dr Burrows noticed them chiefly in connexion with the first secretion of milk, on the fourth or fifth day. Some of these are examples of phrenitis, and not properly of mania. Frequency of pulse is a symptom of primary importance." An *inflammatory* state of the pulse is indeed a most serious symptom; and why? Because it indicates, not phrenitis, which is *excessively* rare, but inflammation of some internal organ, of which the usual symptoms are masked by the mental affection. The latency of the symptoms of acute and chronic diseases amongst the insane is one of the great difficulties asylum physicians have to contend against. Phthisis may exist for years undetected, were it not for the stethoscope, without cough, sweats, emaciation, or hectic, till the disease is very far advanced. Tuberculosis of other organs may run its course without any indication of its presence. Caries and necrosis kill without pain, most extensive abscesses collect without causing inconvenience. I have known dements die of typhoid fever without any symptom but diarrhœa. This peculiarity extends to inflammation of all the internal organs. Reflex action is impaired amongst the insane;—this is exemplified in a small way by the idiot as he sits with a drop hanging from his nose without any irritation of that organ; in a greater, by the dement or chronic maniac dying of consumption, bronchitis, or pleurisy, without cough or pain, however extensive the internal disturbance may be. This remark applies equally to the subject of puerperal insanity, in whom inflammation of vital organs may exist *without a symptom further than a slightly inflammatory pulse;* and I have known even this symptom absent. But where it is present the prognosis must be most unfavourable. I am sorry to admit that, in my experience, which extends to four cases complicated with internal inflammation,—two of bronchitis, one each of peritonitis and pelvic cellulitis,—the result was invariably fatal. There may have been circumstances in these cases which tended to such an issue, such as aggravation of the disease produced by the moving of the patient from her home, and consequent cold and exposure; nevertheless it justifies the conclusion that any such combination must be looked upon as serious in the extreme.[1]

On the other hand, puerperal mania of itself does not kill, and, where you have to combat it alone, not only death need not be dreaded, but, in the very large proportion of cases, a return to sanity may be prognosticated. It is, perhaps, *the* most curable form of insanity. This statement is made advisedly, but does not extend to those cases which are placed under asylum treatment as a *dernier ressort.*

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[1] Puerperal paraphrenitis is by no means denied. Two probable cases are alluded to in the previous paper. But it is of rare occurrence, and may be mistaken for puerperal insanity associated with internal inflammation. Marked *head symptoms* always, however, indicate the diagnosis.

A strong protest must be here entered against the routine treatment which we so often see, more especially in the country, carried out quite indiscriminately. To shave and apply cold to the head, administer tartar emetic, purge and blister, are not uncommon remedies (!) applied where mania exists. In puerperal insanity this (bad) treatment insures a lapse into dementia,—the patient can resist the disease, but not the remedy; each dose of antimony, each cold application, each blister puts the case further and further beyond the control of the physician. In the Fife and Kinross Asylum are some sad instances of women slightly but hopelessly demented, of whom there can be no doubt that if they had been treated in any of our public asylums they would now be useful members of society.

Drugs seem of no avail; opiates, more especially, do more harm than good. A large dose given at the very first indication of insanity is said to have the effect of cutting short the attack; this I cannot speak to, but repeat the statement previously made,—that when it has fairly established itself, although large doses of opium *may* moderate the intensity, they tend to prolong the period of the mania. Stimulants seem to feed the excitement without increasing the bodily strength. The best calmative is food which can be easily assimilated; it should be administered frequently, in small quantities. The patient has a better chance of sleep after a dose of beef-tea than after a dose of morphia. The bowels are often in a very constipated state,—this condition must be attended to cautiously. Above all, careful nursing is the great element of success,—the patient should never be left for a moment till a sound sleep indicates approaching convalescence.

The following cases are extracted from the records of the Edinburgh Royal Asylum, and were reported by myself:—

B. C., æt. 32, married; in good circumstances; naturally frank and cheerful; mother of five children.

The following history of her case was sent with her. She received a great shock when she was three months gone in pregnancy with her fifth child, from an attempt at burglary being made on her house when her husband was absent, but no symptoms of anything like insanity supervened. She was doing well up to the seventh day after her confinement, which was a somewhat protracted one, and succeeded by considerable hæmorrhage. On that day, however, when sitting up in bed, a few friends being present, the discharges suddenly ceased, and shortly afterwards she became maniacal.

On admission she was so weak as to be considered almost moribund, but fearfully excited, causing astonishment as to how so much noise could be produced by one so debilitated. The face was blanched, eyes wild and staring, pulse small and quick, surface cold and clammy; she was so exhausted as to necessitate her being carried to bed. Her mania was of the wildest description, she incoherently raved that she had brought forth dogs instead of children, recognised old friends in the strangers now around her, cried that her food had been poisoned, pointed to imaginary objects, and at intervals screamed loudly. She could with difficulty be restrained in bed, requiring the constant attendance of two nurses to control her. During the first night of her residence she was utterly sleepless, the mental symptoms not in the least abating. She was ordered drachm doses of the solution of the muriate of morphia which were persevered in till she had taken one ounce, but sleep was not produced. Brandy, custard, and beef-tea were administered. Date of admission, January 26th.

29th January.—No improvement; mental excitement has continued since admission; to-day ʒ i. doses of Squire's tincture of Indian hemp, pushed to half an ounce, produced sleep for a short time, and some degree of composure. (*Doubtful; more likely to be the result of exhaustion and the beneficial effect of nutriment and nursing.*)

30th.—Nervously excited, screams fearfully at the slightest sound. (*Probably the result of the cannabis.*)

31st.—Somewhat stronger, but still in a most precarious condition, as the excitement continues though not to such an extent as on admission. To take 15 drops of tincture of steel three times a-day.

10th February.—Since last entry this patient has gradually improved, but is still very weak. A small abscess has formed over the right nipple, also on both thumbs; on being opened they discharged healthy pus.

13th.—To-day she menstruated,—the discharge was not great, but the pain was considerable. The skin over the sacrum shows symptoms of breaking. Tincture of arnica to be applied. Attendant still to be constantly with her. She now takes her food ravenously, and her general health is much stronger. Her mind is also much more composed, although she requires the cannabis at night. She spoke quite rationally to-day for the first time. Oleum morrhuæ prescribed, but it produced diarrhœa.

15th.—To-day, rose from bed for a couple of hours. Very weak still. Skin over sacrum quite healed. She now takes a pint of porter and four ounces of wine daily. Nervousness continues, and the delusions occasionally return, more particularly when she wakes suddenly; they are assuming a religious character.

21st.—Sleeps well without opiates.

23d.—Not so well, rather restless at night. Draught to be continued.

25th.—So restless as to require the nurse to sleep with her in the same bed. Saw her husband to-day with no bad result.

3d March.—Nurse still sleeping with her; requires the draught at night. To-day ordered valerianate of zinc and quinine, a a gr. ii.

4th.—Attempted suicide to-day by sticking knitting-needles into her neck, but did not succeed in injuring herself much. Still very noisy at times; ordered a slight shower-bath morning and evening.

9th.—Sleeps about three hours each night; on awakening is very noisy.

1st April.—Since last entry has improved very much in mind and body. The narcotics have been discontinued.

28th.—Still improving although slowly. Tonics have been continued.

20th May.—Is slowly but gradually recovering. Still very childish, and occasionally plays very puerile pranks.

For the next two months her progression towards sanity seemed to be effected by fits and starts. She would improve a little, continue in that state for a fortnight, and then make another step in the right direction. About the end of June she was sent out on trial, but returned of her own accord in a few days, saying that she preferred the asylum. She on two occasions subsequently wrote letters containing incoherent nonsense bearing on religion, and at times would dress herself fantastically, but, notwithstanding, her recovery progressed favourably step by step, and on the 20th of August she was discharged recovered. She has since been confined twice, with no bad results.

This case is reported *in extenso* to illustrate that the treatment by narcotics was a mistake; the continuance and recurrence of the mania was long and frequent, and I very much fear it was the result of the large doses of opiates. This opinion is confirmed by the fact that all subsequent cases under my care have recovered comparatively rapidly without their employment. Nor did the stimulants produce any lasting good: the ultimate recovery depended much more on careful and nutritious diet, nursing, quiet, and absence of exciting influences.

Case 2.—E. R., æt. 25., married; trade, hat-binder; eight days insane; first attack; her aunt is an inmate of Morningside Asylum.

This woman became violently maniacal two days after the birth of her first child, and was sent to the asylum eight days after the appearance of insanity. She was exceedingly weak on admission, small quick pulse, pale and blanched. She was noisy, and, as far as she could be, violent, cursing and swearing, obscene in speech and action. On being put to bed appeared to masturbate violently, and could with difficulty be restrained. Sent to sick-room. Ordered beef-tea and custard, to be frequently administered in small quantities; attendant to be constantly with her. She was utterly sleepless: the excitement continuing without intermission, and of a character resembling that of delirium tremens. She had distinct delusions as to personal identity. On the third night, she slept for four hours, and the symptoms abated markedly afterwards, the obscenity persisting longest. A fortnight after admission she was up, her strength having materially improved, as also the mental condition; she was, however, mischievous and erotic for two months, with a slight degree of exaltation. Tonics and careful dieting were the only remedies employed. The insanity gradually disappeared, and she was discharged recovered in less than four months.

CASE 3.—A. B., married; formerly a teacher. Immediately after her first confinement she was attacked with acute maniacal symptoms, the labour was natural, but the child died a fortnight afterwards. Her mother states that she was exceedingly violent, and attempted to destroy her child, and subsequently her own life. This condition lasted for five weeks when she was brought to the asylum. On admission she was excited, though taciturn, and attempted to swallow a brooch during the first night of her residence. She walked up and down, and even when induced to sit could not stay quiet for a moment, jumping about and fidgetting. She had delusions as to personal identity. Quite sleepless. An attendant was ordered to be constantly with her. An abscess of the right mamma was discharging freely. Tonics and nourishing diet were prescribed. After a few days she began to sleep better, and the restlessness decreased; she took to singing and playing the piano. Her manner was characterized by a considerable degree of amativeness; when moving about she grimaced and conducted herself in a playfully ludicrous fashion. A marked change came over her for the better shortly after the sinus of the breast had been freely laid open, and the wound commenced to heal. The expression of her face, which had previously been rather repulsive, became pleasant and agreeable, she gradually gave up her restless habits altogether, and was at last induced to sew. When the breast had quite healed, the old symptoms disappeared, a little waywardness and intolerance of control excepted, and she soon became the life of the gallery and a great favourite. She subsequently had one or two slight melancholy fits, but within six months of her admission she was discharged perfectly recovered.

It is in my power to bring forward a long series of such cases, but the above seem to illustrate the general characteristics and successful treatment of the disease. The period of excitement lasted from five days to three weeks, always leaving behind a degree of dementia, the last delusions to disappear being those as to personal identity; in some a slight melancholy was observed, but not deep seated. The treatment seldom altered, except as to the nature of the tonics. As the mind re-established itself and the more violent symptoms were overcome, porter or wine was administered, and at this stage proved highly beneficial.

The following cases illustrate a non-successful issue.

J. H., æt. 30., "married; formerly house-servant; three days insane; first attack; cause, childbirth. Respectable education, cheerful disposition, habits steady and industrious. A sister is said to have suffered in the same way after childbirth." Admitted 20th May.

Bodily health reported as "weaker than usual, health as good as possible, having been only confined of her first child after an easy labour."

The history of this case, beyond that given in the statement, is, that a fortnight ago she was confined and continued very well until three days ago, when symptoms of insanity set in. On admission she was fearfully violent and destructive, noisy and loquacious. The pulse was *somewhat low.* She was very weak. We were informed that previous to admission she had been treated with antimony for the purpose of allaying the excitement. She was put to bed in the sick-room, and ordered wine, beef-tea, and custard. To be under special observation. During the afternoon the pulse became of better character, and the patient seemed stronger. She complained of pain over the abdomen, but not more than might have been expected in a woman so recently confined; the bowels were opened by an enema.

21*st May.*—Has not slept during the night, still noisy, swearing and cursing, with difficulty kept in bed. To continue on the same diet and stimulants. During the day the symptoms did not abate. Towards the evening she was somewhat quieter, and seemed inclined to sleep. Pulse improved.

22*d.*—Early this morning symptoms of sinking set in, which were not relieved by the administration of stimulants. At 7 A.M., the extremities were quite cold, but the excitement was not abated. At 9, she was evidently fast sinking,—there was a lucid interval of about an hour. She died at 10 A.M.

On post-mortem examination, extensive pelvic cellulitis was found; several ounces of pus were discovered. A thin plate of osteophyte was observed on the dura mater, under frontal and parietal bones. Grey matter congested; white matter œdematous.

Mrs B. B., æt. 25, admitted 15th April; "married; kept house; protestant; first attack; insane for four days; supposed cause, childbed. No hereditary predisposition."

This lady was admitted in a state of great exhaustion and considerably excited. Her appearance was typically one of puerperal insanity,—the wild staring eyes, catching at trifles, resembling somewhat delirium tremens, were very characteristic. She had delusions as to personal identity. Her movements were often indecent. She had been confined nine days when the mania appeared. It was her third child; all her family having been born within four years. She was very anæmic. The confinement had been natural, with only slight hæmorrhage.

16*th April.*—Is very excited,—has not slept. She is fed with Liebig's extract, jelly, milk, etc.

17*th.*—As yet no sleep; still excited; picking at bedclothes. Her eyes follow imaginary objects.

18th.—Slept a little this morning. After she awoke, her pulse was found to be 140, tongue dry. About 4 P.M. she took a fit of syncope, so profound that she was thought to be dead.

19th.—Symptoms of bronchitis have set in, her pulse is very quick, small, weak, 160; at times so rapid that it cannot be counted. Dr M. Duncan was called in to see her.

18th.—Ordered mustard to the chest, and an expectorant mixture; to get wine.

20th.—She has great difficulty in expectorating; weaker; has had another attack of syncope.

21st.—Much weaker.

22d.—No better,—worse in fact; to get brandy in small quantities, and often.

23d.—At times is nearly suffocated by the bronchitic discharge; very weak; sinking; still very insane.

24th.—Moribund from suffocation.

25th.—Died this afternoon at 5 P.M. Post-mortem examination was not allowed.

There exists a small class of cases midway between puerperal mania and insanity of lactation, viz., those whose insanity supervenes between one and three months after confinement. A large number of these, as has been already said, are dependent on a recurrence of the insanity developed during pregnancy, but there do occur instances where no such previous aberration has been noticed. These are complicated cases, usually dependent on a weakened state of the system from too rapid child-bearing. For instance, a woman of naturally weak constitution bears three children in four years, and early in the nursing of her last child shows symptoms of insanity. Mania is the usual symptom, but bears a stronger affinity to that consequent on lactation than to puerperal mania. Still these are doubtful cases, and it is difficult to assign them to either class, but, on the whole, it seems most probable that they belong to the former. Profuse hæmorrhage also, in some rare instances, tends to the production of insanity at a lengthened period after labour; an anæmic condition being produced which is very liable to affect the mind.

Insanity of Lactation.

There is little to be added to the remarks made on this form of insanity in the statistical paper. It is distinctly the result of an anæmic state of the system, and in a modified form is not unfrequently noticed among patients applying for advice in general hospitals. Nursing women present themselves complaining of symptoms of hysteria, restlessness at night, and a vague feeling of apprehension, over-action of the heart, bronchocele and exophthalmia frequently are present, anæmic bruits are heard on auscultation, and a morbid condition of the blood detected on microscopic examination.[1]

These physical signs are frequently observable amongst those patients in whom insanity has been developed. The mental symptoms are either mania or melancholy, the latter being the more frequent, and accompanied by delusions either of a suspicious character or as to personal identity, hatred of children, husband, or friends, and a strong suicidal tendency,—it is seldom, however, very profound. The mania is of an evanescent nature, violent whilst it lasts, but not associated with the obscenity observable in puerperal mania. In this form the insanity of lactation is more rapidly amenable to treatment than when melancholy exists; but in both forms, when taken in time, the disease is readily curable.

The following case illustrates the melancholia of the insanity of lactation:—

J. C., æt. 32, married; wife of tradesman; mother of four children; has always been a very devoted mother and wife; first attack; her mother died insane. She has been nursing her baby for eleven months, and for the last few weeks it has been noticed that her manner and appearance have undergone great change. She began to be suspicious of her husband, and at last took a positive dislike to him. On admission a slight scar was observed on her neck, which she admitted she had caused by attempting to cut her throat. Her general appearance indicated anæmia. A distinct bruit was detected, and cervical pulsation; the eyeballs were protruding. She was exceedingly depressed and nervous; said that she "feared herself," that her husband had always been a "guid man" to her, but that she could not overcome her hatred to him. To be under constant observation; ordered nourishing diet and tinct. ferri mur. For a few nights after admission she was sleepless and restless, at times talking and muttering to herself. She was soon induced to sew and otherwise employ herself. Within a month considerable improvement took place in her bodily condition, but the mental symptoms did not materially abate. Shortly afterwards, however, symptoms of convalescence set in; she gradually but slowly became more active and energetic, evinced interest as to her husband and family, and slept soundly at night. A variety of employment was found for her, which she readily undertook, feeling that it was highly beneficial to her case; the melancholy state had quite disappeared five months after admission, when she was discharged recovered.

The following case was characterized by mania:—

N. A., æt. 35, admitted 10th December; "married; wife of labourer; protestant. First attack, which has lasted ten days; cause, nursing a child for eight months." One of the medical certificates stated as follows:—"Incessant talking; now and again furious and difficult of restraint; says she is with God; says she is an angel, and the only one with God in heaven."

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[1] On Vascular Bronchocele and Exophthalmos. Dr J. W. Begbie. Edin. Med. Jour., 1863, p. 217.

This patient, on admission, was acutely maniacal, very violent, cursing and swearing, noisy night and day. She had a delusion as to the visiting physician being a young doctor whom she hated, and, accordingly, swore at and cursed him whenever he came near; she also supposed that the head attendant was his mother, but took very kindly to her. Ordered to be kept in bed, with constant attendance, and to be supplied liberally with beef-tea and other nutritious diet.

16*th December*.—Up to to-night Mrs A. has been very noisy and violent. On the reporter visiting her at about 8 P.M. he found her lying with her face covered up with the sheet. On pulling it down the expression of her face was found to have undergone a great change since the morning,—it was quite natural,—and, on being addressed, she spoke rationally and civilly. Early in the forenoon she had taken a large meal consisting of bread and beef-tea, had slept shortly afterwards, and awoke in the state described.

17*th*.—A slight remission to-day, chiefly evidenced by irritability of manner to other patients.

1*st January*.—Quite convalescent. On conversing with her about her late attack, she said that she recollected perfectly everything which occurred during her illness. She remembered that she entertained the delusion that the reporter was a doctor whom she disliked, and that the attendant was his mother; also that she had sworn at and abused him, apologizing most penitently for so doing.

12*th*.—This day discharged recovered.

Curable as these three diseases are, there exists a class of patients in which permanent bad effects result from the attack,—that in which the patient is congenitally weak-minded. Wretched, half-idiotic girls, whose weakness has been taken advantage of, become maniacal after confinement or during nursing, and, although the mania disappears, a greater degree of imbecility is left after the attack than existed previously. In these patients, also, the disease is almost sure to recur with each confinement.

In conclusion, I would remark that the consideration of this long series of cases has convinced me of the necessity for a more accurate classification of so-called mental diseases. The one at present in use is unsatisfactory, arbitrary, and perplexing. It is a mere begging of the question. We might as well be content with the diagnosis of a disease as "dropsy," irrespective of its cause, as mania unconnected with a specific bodily condition; and if we succeed in so associating a mere symptom with its cause, why not give the disease a specified name? Advances are being made in the right direction. Dr Skae of Morningside some years ago proposed a scheme of classification based, not on the symptoms individually, but on a system of grouping peculiar symptoms and associating them with peculiar bodily conditions. This had been done already in the case of general paresis, epileptic insanity, puerperal insanity, senile insanity, etc., thus demonstrating that such a classification is possible. Dr Clouston has described another well-marked form,—"The Insanity of Tuberculosis,"—and other papers have been published by Dr Skae's old pupils with a view of elaborating his system. That great difficulties stand in the way, and that the original scheme is imperfect and requires modification is true; but, as our knowledge increases, so will it become evident that this is the true theory on which to found a rational classification.

ARTICLE IV.—*Case of Suppression of Urine, fatal on the thirteenth day.* By J. W. MILLER, M.D., Physician, Dundee Royal Infirmary.

(*Read before the Dundee Medical Society.*)

THE patient, whose case I am about to relate, sent for me about a week after her eighth confinement, to attend her for mammary abscess. She was thirty-six years of age, of rather pale flabby complexion; and on her neck there were cicatrices of old, probably strumous, sores. Her confinement had been natural and rapid, so much so that she had not succeeded in procuring a medical attendant in time for her delivery. About ten days after the commencement of my attendance, she called my attention to the fact that her urine was very scanty and very high-coloured. I had some of it sent to my house for examination, and found it to present the following characters:—it was turbid and of a dark smoky colour, acid, of sp. gr. 1020, and had a dense flocculent sediment; it was abundantly albuminous, the coagulum, on settling, occupying about a fourth of the bulk of the fluid in the test tube; the deposit, when examined microscopically, was seen to contain blood-globules, and epithelial and granular casts in great abundance.

There was no dropsy, beyond very slight œdema about the ankles.

On being questioned as to whether she had presented no dropsical symptoms before her confinement, she said she had observed none; but since the termination of the case I have learned from a nurse, who saw her occasionally, that the face was œdematous within a few days previous to her delivery. Immediately before the close of her fifth pregnancy, about five years since, she had been severely ill with dropsical symptoms, there being nothing abnormal, however, in her labour; and there had been no recurrence of the symptoms since that time till the present attack.

The treatment now prescribed consisted of cathartics, with a turpentine stupe, followed by hot poultices over the renal region.

Three days after this, she was no better, was troubled with occasional vomiting, and the urine was more scanty, the quantity being not more than twelve ounces in the twenty-four hours. It was of sp. gr. 1023, was similar in microscopical characters to that formerly examined; and contained now so much albumen as to become almost solid on boiling.

Next day, 23d April, I found that she had passed no urine since the preceding morning. She complained of considerable lumbar pain, and of debility. The pulse was about 90, of good volume, the tongue pretty clean. The vomiting had ceased. She was now cupped over the loins, after which the hot poultices were continued, and full doses of compound jalap powder were given, which acted well. She was also ordered to drink largely of simple diluents. In the course of the afternoon she thought that a few drops of urine, not more, had passed when the bowels were being moved.

24th.—No urine; very slight œdema of the face and legs; pulse, 84. Bowels moved, but not very copiously.

I now resolved to ask Dr Gibson to visit her along with me; and he accordingly did so in the evening, continuing his attendance till the end of the case. We were told that she had passed about a tablespoonful of bloody urine, and had had one attack of vomiting. We agreed to repeat the cupping, and to continue the cathartics and hot poulticing.

25th (fourth day of suppression[1]), morning.—No urine; there had been no more vomiting; bowels had acted thrice, freely; pulse, 72. There was no headache, and no mental confusion or drowsiness. Evening.—Little change; had vomited twice. The jalap was ordered to be stopped, and she was to take a drachm of bitartrate of potash every four hours.

26th (fifth day of suppression), morning.—No urine; troublesome vomiting during the night; considerable purgation; pulse natural, but rather more compressible. To continue the bitartrate, and apply fomentations of infusion of digitalis to abdomen. Evening. —Four fluid stools, and several attacks of vomiting since morning; some headache and inclination to drowsiness; pulse, 72, of good volume and regular. She was able to take but little nourishment. Ordered half-ounce doses of inf. digitalis with the bitartrate every four hours; and to continue the poultices to the back, and the digitalis fomentations.

27th and *28th.*—There was very little change in her condition. She passed no urine. The dose of digitalis was diminished to ʒij., and of the bitartrate to ʒss., as the former produced, she said, curious indescribable sensations after each dose, and the latter was keeping up purgation rather violently. A hot bath was also tried without any good effect.

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[1] I consider the case as one of suppression from the morning of the 22d, not thinking of any account the very small quantities of urine passed after this on a few occasions.

29th (eighth day of suppression), morning.—No urine; catharsis and vomiting continue; pulse, 78; tongue moist and pretty clean; no œdema; no head symptoms. Seeing that the treatment hitherto pursued had been totally unsuccessful in inducing the kidneys to resume their functions, we now determined to try a complete change; and finding that many eminent authorities recommended cantharides as having been occasionally successful under similar circumstances, we considered it advisable to try it: and prescribed ten minims of the tincture, with as much tinct. ferr. mur. every four hours; discontinuing the bitartrate and the digitalis, as also the fomentations of the latter. Evening.—There had been some diarrhœa and vomiting, but not much; nor the latter so soon after taking the medicine as to lead us to suppose it had been ejected. She was now inclined to fall into sleep rather easily, and had had occasional headache since morning, but her intelligence was perfect; and the extraordinary absence of symptoms of urgency was still, perhaps, the most remarkable feature of her case.

30th (ninth day of suppression).—No urine; pulse, 66; more drowsiness and headache, although these were still slight, and the latter was described as being more a sensation of weight than actual pain. There had been four loose stools during the night, and as many attacks of vomiting. She complained much of a bad taste in her mouth, and suffered also from very hot feet, and a sensation of shivering now and again in her back. A large cantharides blister was ordered to be applied over the region of the kidneys.

1*st May* (tenth day of suppression).—General condition little changed. A lichenous eruption, attended with troublesome itching, was making its appearance on the face and extremities. The blister had risen well.

The case having been now for some days a very anxious and extraordinary one, I went to Edinburgh to consult Professor Christison with reference to it. He had never seen such a case, more especially in respect of its long duration without the production of urgent uræmic symptoms, and its supervention after instead of before delivery. He approved of the treatment which had been adopted, and said he had little to suggest after the failure of the bitartrate of potash and the digitalis, but that, if on my return the patient's state were such as to admit of it, we might try the effect of a very small general bloodletting.

At 10 P.M. I saw the patient again with Dr Gibson; and we did not feel that we could venture on bloodletting. She had felt as if she could pass some urine, and tried, but in vain. I should have mentioned that before now the catheter had been introduced to make quite sure that there was no urine in the bladder.

2*d* (eleventh day of suppression).—No urine; no œdema; very little drowsiness; no mental confusion; pupils quite normal; vomiting and purging troublesome; eruption on skin more abundant; pulse, 78. The mixture of tr. canthar. and tr. ferr. mur. was ordered to be omitted, and we prescribed a grain of the pulv. cantharid. in pill every four hours, along with a drachm of sp. æth. nit. and five minims of ol. juniper: as she was feeling considerably more prostration, we also gave her from two to three ounces of gin in the twenty-four hours. I obtained from the blister about two drachms of serum to test it for urea.

3d (twelfth day of suppression).—She stated that when at stool during the night she had passed a little urine, but not more than a tablespoonful. Her condition was little changed. She thought the sp. æth. nit. and ol. juniper increased the vomiting; so they were discontinued. In the evening the catheter was introduced, and about a drachm of urine was obtained. There was much suffusion of the eyes, and the eruption was increased. Her intelligence was still perfect, and there was no increase of the drowsiness, and no œdema. We agreed to repeat the blister over the loins.

4th (thirteenth day of suppression).—No urine. The patient looked considerably more depressed, and her countenance wore an expression of great anxiety; but there were still no head symptoms. Her sister said that she had had "a bad turn" early in the morning, which, from the description given of it, had evidently been a slight convulsion. The vomiting continued to distress her much. The bowels had acted once during the night. As the catharsis had been now for some days much abated, and there had nevertheless been no effect produced by the diuretic remedies towards inducing the kidneys to resume their functions; and considering that the bowels were the chief channel by which we could hope that the excrementitious matters, which should have been passing off by the kidneys, might be eliminated, we resolved to discontinue the cantharides (she had now taken ten grains), and re-induce the purging by a drop of croton oil. This was administered about 11 o'clock in the forenoon. At 2.30 I was sent for. I found her suffering intense pain in the region of the kidneys, rolling about in her bed, moaning, and clasping her hands in her agony. Warm poultices applied to the back gave some relief. There had been no more convulsions. The croton oil had produced copious discharge from the bowels; and the vomiting continued. At 7.30 P.M. there was a great change for the worse. The pulse was scarcely perceptible. She was still suffering much pain, and had an intensely anxious expression. Her intelligence was quite unaffected. The catheter introduced into the bladder obtained about two drachms of urine. We prescribed a grain of opium; and to relieve the distressing sense of sinking, some champagne was given her. At 11.30, when we visited her for the last time, she was moribund. Her intelligence was clear till within a few minutes of her death, and there was not a trace of convulsion.

On 7th May (sixty hours after death), with the assistance of Dr Gibson, and Dr Stewart of Newport, I examined the body. It was much advanced in putrescence, and we only examined the abdomen. The peritoneal cavity contained about a pint of clear serum. The kidneys were much enlarged, the left weighing eight ounces, the right about a drachm less. Both were much congested, and showed on section, under the microscope, a great abundance of fat cells. I forwarded one of the kidneys to Professor Christison, who kindly sent me the following report of its examination by Dr Grainger Stewart, Pathologist to the Edinburgh Hospital :—"A fresh cut surface emitted numerous oil globules when the kidney was gently squeezed. A thin section under the microscope presented the appearances of stearosis, intermingled with those of inflammatory epithelial degeneration of the tubules." He says farther, "The conclusion is, that in all probability the lady had latent stearosis of the kidney, and that the other appearance was the result of more recent, sudden, inflammatory Bright's disease, superinduced therefore, and the immediate cause of the suppression."

With reference to the serum from the blister, which, as I have said, I obtained on the eleventh day of suppression, I examined it for urea as follows :—The albumen in it was coagulated by heat, which rendered it almost a solid mass ; this was well shaken and boiled with alcohol, then filtered ; the alcoholic solution was evaporated to dryness, and the residue dissolved in distilled water, and slightly concentrated ; a drop of this was placed on a glass slide, and a drop of nitric acid added to it ; this was evaporated under a glass shade and over sulphuric acid to dryness, and the result was a crystalline film, showing distinctly, under the microscope, the characteristic appearances of nitrate of urea. Some of this serum I sent to Professor Christison, who wrote me that there was distinct evidence of its containing urea, though on a very small scale. I also examined the vomited matter, but the result was unsatisfactory ; and Professor Christison wrote me that there was no trace of urea in some which I had sent him.

This case presents several very remarkable features. It is strange that the exacerbation of the renal affection should have been subsequent, instead of previous, to delivery ; and that the chronic disease should not have produced, before delivery, more marked symptoms. Probably the explanation of the supervention of the acute disease during the puerperal state lies in the fact, that the blood during this period is loaded with excrementitious matters, requiring active elimination by the various excretory organs ; that the kidneys in this case, being diseased, were able but very imperfectly to perform their functions ; and that the excitement of these organs, caused by the sudden demand for increased work, was sufficient to light up the acute disease, which resulted in the suppression. The long duration of the case is also remarkable, and the advanced period attained before the supervention of uræmic symptoms; and, further, the extreme slightness of these symptoms after they did appear. It is strange that there was little or no œdema throughout the case; and owing to this circumstance the absence of convulsions is the more remarkable; for it is held by Frerichs that when there is no œdema, the occurrence of eclampsia is more probable, as the serum exuded into the tissues carries urea along with it, and so assists in the purification of the blood. It should be remembered that it is not the presence of urea in the blood, as urea, which causes convulsions, according to Frerichs, Braun, and others; but that the urea must first be changed, under the influence of some peculiar ferment, into carbonate of ammonia. Oppler, Hammond, and other authorities, however, consider this theory as incorrect; Oppler considering that the uræmic symptoms are probably the result of the formation and accumulation in the brain and spinal cord of certain products of decomposition of tissue.

The most full description of "suppression of urine" which I have been able to find is in Willis's work on urinary diseases. Several other authors have little, some nothing, on the subject. Willis gives the details of a number of cases, reported by various observers: one of total suppression for between nine and ten days, which resulted in recovery; another of six weeks' duration, and another of two weeks' duration, of neither of which is the result stated: another, the case of a lad, 17 years of age, who had never in his life passed any urine, the explanation of this case, however, being that the ureters probably terminated in the great intestine: another case, that of a child aged 15 months, the apparent cause being dentition, the child having had a former attack of four days' duration, on the occasion of some teeth cutting the gum; in this second attack there was a remarkable absence of symptoms until the fifteenth day, when the face and extremities became bloated, and there was œdema under the eyes; on the twenty-first and twenty-second days there occurred convulsions and vomiting, and on the twenty-third day the child died. He gives another case, that of a man, in which the suppression was associated with calculous disease, and proved fatal on the tenth day. Throughout this case the intelligence was unaffected, the principal symptoms being a disagreeable sensation rather than a positive pain in the right ilio-lumbar region, total inappetence, constant sickness, and occasional vomiting; about the middle of the case, severe pain in knees and calves of legs, œdema of the scrotum and tops of the thighs, and ultimately great weakness and prostration. Willis alludes to some other cases, but those given are the most remarkable. He says the affection may occur at all ages, but is much more common in infancy than in advanced life. Watson, in his "Practice of Physic," mentions a case reported by Sir Henry Halford, being one of five which that gentleman had seen, and which, he says, was an exact copy of the others, this case proving fatal by coma in three days.

In the Sydenham Society's Year-book for 1861, at page 218, a case is given, that of a boy, aged 5½, in which there was no urine secreted for about five days, and exceedingly little for five days more; yet there were no uræmic symptoms, nor any notable dropsy during these ten days. Uræmic symptoms first occurred on the restoration of the urinary secretion, and this was slightly albuminous. This case was one of scarlatina, and ended fatally.

With reference to cases of alleged suppression for days, and even weeks, without any other symptom of bad health, Watson says,—"What degree of credit such narratives deserve, I do not know; but assuming that there was neither fraud nor mistake, it may be suspected that either the natural secretion was compensated by some vicarious or supplemental discharge, or that a small quantity of urine was actually separated by the kidneys." Willis says that he does not believe in any vicarious secretion of the elements of the urine; meaning, evidently, in such a perfect manner as to compensate for total inaction on the part of the kidneys. But, as is well known, during a case of suppression of urine, the effete matters which it should contain pass off, at least in part, by various channels; and it is quite inconceivable that life could continue with so little disturbance for so long as it did in the case which I have related, and also in the others which I have alluded to, unless this were the fact. With reference to this point, it is worthy of remark, that the first uræmic symptoms in my case occurred on the diminution of the catharsis and vomiting.

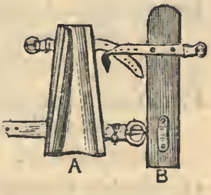
As to treatment, Willis recommends, in the first instance, general bloodletting, aided by cupping locally; blisters over the kidneys, cathartics, digitalis, diluents, and the warm bath. Then, if these means have failed, the plan advocated by Dr Elliotson, namely, turpentine, and the more acrid diuretics, especially cantharides. Bitartrate of postash, oil of juniper, and other diuretics, are also recommended by various authors. But, under any treatment, the disease seems to be most dangerous and intractable.

ARTICLE V.—*Colles's Fracture.* By LAWSON TAIT, Esq., Member of the Surgical Society of Ireland, etc.

WITHIN the last three years twenty cases have come under my observation where this fracture had occurred, had not been properly treated, and has left its characteristic hideous deformity. A few weeks ago a gentleman, who had heard of some new splints that were used for this fracture, came to consult me about his arm—to see, as he said, if I could not re-break and set it better. He had been hunting one day about two years ago, had a fall, and was picked up with this fracture. It was diagnosed at the time as a sprain and treated by fomentations; but now the deformity shows only too plainly that a fracture had occurred. The bases of the metacarpal bones of the thumb and index finger, along with the styloid process of the radius, are thrown upwards and backwards on the shaft of the radius and are fixed there by a mass of callus; the corresponding part of the ulna constitutes a most unseemly prominence on the outer side of wrist, like a large ganglion; the power of pronation is almost gone, the hand is rapidly becoming atrophied and is so weak that it (the left hand) is almost powerless over the reins. I told him what he had been told before, that any interference was out of the question and that there was but a slender prospect of the state of matters mending. Such cases are only too common. I have seen them where the fracture had been correctly diagnosed and treated by one or other of the many plans adopted in this injury. The worst instances, however, are those where the accident is treated as a sprain. Last year I dissected the arm of a woman in whom the deformity was excessive. In her case the fracture had been recognised and very properly put up by a competent practitioner, but another happening to pass the house shortly after who was not on good terms with the first, he was called in to examine the case, failed to detect the fracture, declared that it was a sprain and that the splints were useless. The result was that after three months the hand was utterly useless, and remained so till the patient died. On dissection, I found that barely half an inch had been torn from the styloid process of the radius, that this fragment was lying on the back of the radius firmly bound in the mal-position by new bone, and that there was scarcely a trace of a lateral ligament between the head of the ulna and the cuneiform and pisiform bones. There escaped from the joints on incision a viscid turbid fluid, quite unlike anything I had previously seen in such a position. Such a mistake as the above occurred from the want of knowledge of the facts that in this fracture crepitus is rarely to be discovered and that the greater or less displacement marking fractures generally is, in this instance more frequently absent than present immediately after the injury, rarely being manifest unless to the practised eye until many days have passed. When it is marked immediately after the injury the displacement is very great and the injury to the soft tissues very considerable. It is in old people that this immoderate deformity is most frequently observed.

To avoid such mistakes as those mentioned and their painful consequences, I should propose the rule that in cases of injury of the wrist, especially when a fall on the *palm* has been the cause, when there is much pain, swelling and inability to pronate and supinate, such should be treated as fractures for a few days till certainty be arrived at. This practice could not possibly do harm; if a fracture should be present it will be safe; and if there is only a sprain, the rest implied by the use of the splints will be the best treatment.

What is required in the treatment of this fracture is not merely repose of the fragments, which alone is obtained by the varieties of apparatus formerly in use, but their accurate replacement and confinement in that position,—a condition that is best obtained by the use of the splint devised by Professor Gordon of Belfast. This apparatus has been so well described and represented by the inventor, Porter, and others, that I need do no more than refer to the woodcut below to give an idea of its appearance. In a former paper (*Medical Times*, Feb. 1866) I advanced a theory of its mechanism, the accuracy of which the experience of eleven cases of its employment has confirmed in my own mind. The invariable success which has followed its use in my own cases and in those which I have been enabled to examine through the kindness of my friends, Professor Macnamara, Messrs Porter and Collis of Dublin, and Dr Heron Watson of Edinburgh, is my excuse for thus again endeavouring to draw the attention of the profession to the apparatus. There is, however, one slight modification which I have ventured to suggest in the arrangement of the straps which I have found of some importance. It is that, instead of using two straps, to secure the apparatus, one at either end, two small straps be substituted for the one at the distal extremity. As represented in the woodcut, one of those straps is fixed to each of the splints A and B, so that the tongue of the one fits to the buckle of the other. The advantage of this is that the edges of the splints are retained equidistant. Without this arrangement they are very apt to approach each other on the radial side, and so exert their influence in a manner neither comfortable to the patient nor beneficial to the state of matters requiring their use. This is easily remedied or prevented if the patient can be seen daily; but if it should happen, as it did to a patient of mine during the late severe storm, who was prevented from coming to me by the snow for nearly three weeks, it might chance to become serious. Besides, by these small straps pressure is made more accurately on the spot where it is most required, and is persistently maintained there. The padding, too, remains more accurately in position.



Since the above was written, I have examined the arm of a gentleman in whom the deformity is certainly worse than any I have yet seen. The fracture was recognised, and treated by straight splints. Now the hand is almost useless, pronation and supination almost entirely lost, the hand and arm very much atrophied, and always presenting a cold, blue, miserable appearance. The temperature of the injured hand is generally perceptibly lower than that of the other.

Part Second.

REVIEWS.

Notes on Cholera. By George Johnson, M.D. Lond. Pp. 112. London: Longmans, Green, & Co.

On Epidemic Diarrhœa and Cholera. By George Johnson, M.D. Lond. Pp. 32. London: Robert Hardwicke.

On the Nature of Cholera as a Guide to Treatment. By William Sedgwick, M.R.C.S. Pp. 200. London: Walton & Maberly.

Cholera: its Real Nature and Treatment. By Charles Shrimpton, M.D. Pp. 109. London: John Churchill & Sons.

Acholic Diseases, comprising Jaundice, Diarrhœa, Dysentery, and Cholera. By A. C. Macleod, L.K.Q.C.P.I. Pp. 230. London: John Churchill & Sons.

On Malignant Cholera: its Origin, Pathology, Treatment, and Mode of Prevention. By Edwards Crisp, M.D. Pp. 135. London: Robert Hardwicke.

Practical Observations on the Pathology, Prevention, and Treatment of Asiatic Cholera. By W. Beamish, M.D. Pp. 32. Dublin: Fannin & Co.

On Saline Venous Injection in the Collapse of Asiatic Cholera. By Gilbert Finlay Girdwood, M.D. Pp. 48. London: Darton & Co.

Recent Cases of Cholera at Ibrox. By S. J. Moore, M.D. Pp. 20. Glasgow: James Maclehose.

The Curable Stage of Cholera. By Andrew Fergus, M.D. Pp. 15. Glasgow: James Maclehose.

Like the ancient Corinthians in regard to the novel religious views then first being made known, so now in regard to that mysterious and novel disease Cholera, every medical man "hath a doctrine," and an "interpretation," upon which, as befits the subject, they proceed to base a method of treatment; while, as critics, our quarrel with these would-be apostles is the same as that of St Paul with those of old, viz., that they publish their views, and thus compel us—in the interests of truth—to pay them sometimes more attention than they deserve. And be it remarked—*par parenthèse*—that in medicine it is abstract truth alone that we seek for, and not any modern *idolum specûs*, yclept orthodoxy. No; in medicine every man is entitled to hold and to act upon those opinions which seem to him most truthful; but to escape being branded as a quack or a fool he must be able to show that his views are consistent with the welfare of mankind, and with the eternal laws of nature, as well as that they hang logically together.

Amongst the many authors who have of late written upon the subject of cholera, no one is more entitled to the foremost place than Dr George Johnson, whether we regard the forcible manner in which his views have been brought before the profession, the plausibility of the arguments by which they have been supported, or the important effect they would have—if accepted—upon the treatment of this disease. Basing his views upon the undeniable facts that the collapse of cholera is not by any means always proportionate to the amount of fluid lost by vomiting and purging, while it by no means infrequently bears precisely an inverse ratio to that loss, and upon the hypothetical idea that this vomiting and purging are nature's efforts to throw off that poison which, if retained, gives rise to collapse, and too often to death, he not only deprecates any interference with these salutary evacuations, but proposes to lend active aid to their eliminative action. "Can we," says he, "do anything to assist this natural curative process? Unquestionably we can; by emetics and purgatives" (Notes, p. 88). This is the key-note of his work, and though other matters are taken up and employed, partly in rounding off his pathological theory of cholera to one consistent whole, and partly in attempting to show the reasonable nature of his practice, yet these have no necessary or inseparable connexion with his views, which must stand or fall, in our estimation, by the amount of probability we attach to the theory that cholera is produced by a poison which is eliminated from the body by means of the serous discharges from the stomach and bowels, the retention of these producing collapse primarily, and death finally. Cholera is usually regarded as belonging to the zymotic class of diseases, to that class which includes, among others, typhus and enteric fevers, plague, scarlatina, smallpox, etc.; and we suppose, from the comparison Dr Johnson has instituted between cholera and smallpox, that he acknowledges the correctness of this classification. We have at present neither time nor space to enter into any inquiry as to the meaning of the term "zymosis," or the correctness of its application to these complaints; we regard it for the present simply as a generic term, applicable to certain diseases which are received by contagion, as a rule run a definite course, and propagate themselves by contagious miasmata given off from the bodies of those affected, which attach themselves to the cutaneous, pulmonary, or intestinal exhalations or secretions, the special vehicle varying with each disease. Now, we know that many of these diseases occasionally assume a form, only too apt to prove suddenly fatal, which has been termed *siderans* in relation to plague and typhus, and *malignant* in reference to scarlatina, smallpox, and measles, and which has been supposed —and with good reason, we think—to depend upon the sudden overwhelming of the vital powers by an enormous—or at least a relatively excessively powerful—dose of the special poison. And we also know, and it is to this that we wish to direct special attention, that where recovery has taken place from any of these usually fatal types of zymotic disease, it has not commonly been accompanied by any increased evolution of that special eruption by which the poison is ordinarily regarded as being eliminated, or at least by any increase adequate to explain the rapid and important change in the attendant phenomena, nor has it been produced by any means which of themselves could produce this active specific elimination. In like manner, in regard to enteric fever, which, like cholera, is supposed to be propagated chiefly by the poisonous germs contained in the stools, which, therefore, ought to be regarded as eliminating its special poison, we know that one great danger of the disease is connected with the amount of intestinal irritation present, and may therefore be to a large extent measured by the number of the stools, and may actually be diminished by such soothing measures as have, for one of their results, a reduction in the number of daily stools, and a consequent diminution of the amount of poison thus thrown off. Moreover, in reference to smallpox, which Dr Johnson has selected as illustrative of his views regarding cholera, we know that not only is there no relation between a copious eruption and a mild disease, but the reverse: the disease being milder in proportion to the scantiness of the eruption; but we also know, specially from the results attained by Daniel Sutton, the most successful inoculator that ever lived, that the best way to secure a mild disease is not to determine to the skin, the natural emunctory of the disease, but to the intestinal mucous membrane,—a part from which there is good reason to suppose that the natural elimination of the poison in this complaint never takes place, or only in the rarest instances and to the most limited extent. Thus we gather from the history of these zymotic diseases, that there is no necessary connexion between mildness of type and copiousness of specific eruption or discharge, but rather the reverse, the mildest forms being frequently entirely without specific eruption or discharge, as in the *morbilli sine morbillis*, *variolæ sine variolis*, etc.,—forms of disease which distinctly prove that malignancy of type is not the mere result of absence of specific eruption or discharge, but the reverse, and even where these are suppressed by malignancy of type, recovery is not necessarily or usually heralded or accompanied by any increase in this specific elimination. There is therefore, *à priori*, no reason to suppose that cholera differs from its congeners, and should have its malignancy diminished by increasing its specific discharges. Nor have we, *à posteriori*, any better reason for supposing this to be true, for the treatment of cholera by emetics and purgatives is no novelty, as Dr Johnson well knows, but one that was largely carried out by our Indian physicians of former days, as well as by Dr Searle, at

Warsaw, and by several physicians in London during the first European epidemic, and that with so little success that it dropped so completely from the category of remedial measures, as to be, when revived by Dr Johnson, regarded as a discovery of his own. Nor has the result in modern times been one whit more successful than formerly, as we learn from the report by the Treatment Committee of the Medical Council of the Royal College of Physicians, who collected and tabulated the facts supplied them by others, where it is stated "The evidence of these tables condemns the eliminant treatment altogether as a principle of practice" (p. 13). The actual percentage of deaths following the use of eliminants being 71·7, the greatest percentage recorded; castor-oil being specially fruitful of mortality.

Moreover, Dr Johnson has in his little work a peculiar aptitude for collecting facts capable of being turned to good account in support of his theory, and of omitting those which tell against it. He is quick at perceiving and bold in denouncing the false reasoning of others, but just as apt to reason falsely himself when led away by his own ardour, and self-deceived by the speciousness of his own arguments. Thus he quotes from our pages with approbation the statements of Dr W. Robertson (Dec. 1848), and of Dr Gairdner (July 1849), that the milk continues to be secreted in cholera patients apparently in undiminished quantity, and coupling this with his idea that the bile as well as the urine is suppressed, he argues that the continued secretion of milk, the constituents of which, he says, may be procured from the blood without the addition of oxygen, and the suppression of bile and urine, the constituents of which are all the results of oxidation, are just so many proofs of the truth of his theory that collapse consists essentially in defective oxygenation of the blood, the result of spasm of the pulmonary arteries induced by the cholera poison. But he forgets to mention that both Dr Robertson and Dr Gairdner agree in stating that, according to Professor Maclagan, "the milk of these patients differs very materially from healthy milk, that it is of low specific gravity, contains little or no butter, and when examined microscopically presents an unusually small number of milk granules." That it is, in fact, little else than rice-water effusion into the milk tubes, somewhat flavoured as it were by their washings, and only by courtesy called milk. While he also omits to mention that Dr, now Professor Gairdner of Glasgow, expressely says that "the assertion that the bile is suppressed is obviously incorrect,"[1] stating as proof the facts that in all the cases examined the gall-bladder contained more or less bile, except in two cases, the one being a diseased liver in which the gall-bladder contained only a "thin light-coloured fluid," and in the other, though the gall-bladder was empty, there was abundance of bile in the duodenum and stomach, as was also the case in several of the other cases. Thus without travelling from the authorities quoted by Dr Johnson himself, we obtain ample evidence of the insufficiency of the basis upon which his theory was supposed to rest, as well as of the specious but not very trustworthy manner in which his facts are selected and his arguments put forward.

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[1] Dr Warburton Begbie found bile in the urine secreted in the earlier periods of reaction.

Moreover, the links of Dr Johnson's pathological theory do not hang very logically together; thus, he supposes that the cholera poison circulating in the blood displays its irritant action on the muscular tissue by "the painful cramps which it occasions," and he argues that the blood thus poisoned "excites contraction of the muscular walls of the minute pulmonary arteries, the effect of which is to diminish, and, in fatal cases, entirely to arrest the flow of blood through the lungs;" and this he believes to be the efficient cause of collapse, producing imperfect aëration of the blood, the consequent fall of temperature, the dark and thick appearance of the blood,—the result, as he says, of defective aëration, like the smoky flame of a lamp, and the suppression of bile and urine. But he forgets that a poison which can produce painful cramps of the voluntary muscles and spasmodic contraction of the minute pulmonary arteries must equally affect the minute systemic arteries; while a contraction efficient enough so strikingly to diminish the physiological action of the lungs must also of itself be sufficient to diminish all the glandular secretions, not merely those of the liver and kidney, but of the mammæ also, which on his own showing it does not: the obvious conclusion being, that this contraction does not exist in the minute systemic arteries, and this we conceive is an insuperable argument against its existing in the minute pulmonary arteries, so that the cause of collapse must be sought elsewhere. Connected with this matter, too, is this other curious fact in Dr Johnson's pathology, viz., that an amount of oxygen insufficient to provide for the secretion of bile is, nevertheless, sufficient to enable the cerebral functions, which involve a considerable amount of oxidation, to continue uninterrupted till death. Besides, we know that deprivation of oxygen produces asphyxia, and not collapse. "Choleraic collapse is a form of asphyxia," says Dr Johnson; but the attendant phenomena of these two different states are in many respects the very antipodes of each other; thus, in the one the long continuance of heat even after the heart has ceased to beat is a well-known fact, while with the first onset of the other a mortal coldness overspreads the body with extreme rapidity; in the one, we have the liver and kidneys turgid and the bile and urine freely secreted, in the other we have the very reverse; in the one we have the blood thicker than usual, in the other more fluid. To argue under such circumstances, that the one condition is but a "form" of the other, is to confound all our notions of the meaning of words or states, and to paralyze all our efforts to obtain any correct ideas of any morbid process whatever.

Further, Dr Johnson adopts Dr Mackintosh's view, that "the good effects of the (saline venous) injection were rapid in proportion to the heat of the solution," and explains its success not by the direct addition of fluid to the inspissated blood, but to the influence of the high temperature in relaxing the spasmodic contraction of the minute pulmonary arteries. But this theory is completely opposed to the views of Dr Girdwood, who was a most successful employer of this remedy, and in the pamphlet before us strongly recommends (p. 43) that the temperature of the fluid to be injected should never be higher than 100°, instead of 106°—120°, as employed by Mackintosh. Nor is it consistent with the fact that saline fluids produce almost equally remarkable results when introduced into the system cold; thus, the saline remedy most vaunted during the epidemic just passed—the bromide of potassium—relieved the cramps at once, restored the colour and the temperature at least partially; the relief of the cramps clearly proving, according to Dr Johnson's theory, that the partial relief to the symptoms arose from this drug neutralizing the spasm, yet it did not act by reason of its heat, but only from its saline virtues. Or, if we believe that in this instance the bromide acted by reason of such narcotic virtues as it undoubtedly possesses, still this cannot be said of the chlorate of potass, which, hypodermically injected in quantities of an ounce of the saturated solution, produced the same striking results, not by its narcotic properties, for it has none, nor by its warmth, for it was never more than tepid when injected, but solely by its saline properties,—properties which, for some hitherto unexplained reason, have a very wonderful temporary effect in cholera.

We do not question the existence of a morbid poison in cholera, as distinct and as evident in its results as in any other of the zymotic diseases, though not perhaps so virulently contagious as in many of them, nor is its contagion so easily traced. About twenty years ago—1848-9—when cholera spores first began to be talked about, two young medical men were in the habit of examining cholera stools in the side-room of one of the wards of a well-known hospital. When they were done, the nurse removed the stools. One day, in passing through the ward with the pitcher in her hand, she was forced to set it down, from being suddenly called to a patient. A girl in the ward, ill of cardiac disease, curious to see what the doctors had been about, went and sniffed at the contents of the pitcher, and in twelve hours she died of cholera of a most virulent type; and her case was subsequently commented upon in a clinical lecture by a distinguished physician as a most exquisite illustration of the non-contagious origin of cholera, as, from her long residence in the ward, it was impossible that she could have been exposed to contagion.

During last epidemic, in a district where deaths were subsequently numbered by hundreds, the first two cases imported from an infected district were buried—the one as a case of apoplexy, and the other as inflammation of the stomach—their true nature not having been recognised at the time nor discovered till afterwards, and yet the medical attendant was not an ignoramus. Again, a man came from a distance to see his son; he found him coffined and his dwelling locked up. By night he broke into the house, unscrewed the coffin lid, and had a good look at his son He went home and died of cholera in six days, originating a tolerably widespread epidemic in the district where he resided. Further, he and his family stoutly denied that he had any premonitory diarrhœa, yet he was seen from the roof of a house by some slaters, on the day previous to his death, repeatedly to throw down the tools with which he was dressing a piece of ground and retire to a ditch, which, on being subsequently examined, afforded ample proof of the pre-existence of diarrhœa. These are some examples of the difficulty of tracing contagion—the most certain proof of the existence of a morbid poison—in this country, where, from climatic circumstances, the disease probably spreads solely by contagion. At other times the evidence of this is more unquestionably evident. Thus, in 1853, some Irish labourers, having spent the night in a country town where cholera prevailed, and as far as could be ascertained in a cholera house, returned to their bothy at a farm town, where three of them were seized with cholera, and one died. Their evacuations were emptied into a stagnant ditch in front of the cottages forming the town. The farmer, aware of an approaching visit from the Government Inspector—the late Dr Gavin, who was accidentally shot by his own revolver in the Crimea,—set his whole strength of men to clear out this ditch: *every one* of these men were seized with premonitory diarrhœa. Fortunately the medical man was paying domiciliary visits to their houses night and morning, and every one of these cases were nipped in the bud. In India, on the other hand, the soil seems saturated with cholera germs, which only require the return of the favourable season to be roused from their dormancy into active virulence; thus it often happens there that it breaks suddenly out over a large district of country, as it did in 1861, when in one night it broke out in every town on the Ganges, from Rajmahal to Mirzapore, a distance of six hundred miles. In India, consequently, the disease is not so readily traced from man to man, the endemic causes of extension apparently preponderating very much over its merely contagious causes; but we think that, in this country, where the endemic and meteorologic influences are infinitely less active, its chief, if not its sole, mode of extension is by contagion; not aërial infection, as in ordinary typhus, but direct contagion by the absorption into the body, through some channel or other, of the cholera poison, chiefly generated and multiplied by the decomposition of the peculiar stools, precisely as in enteric fever; and we cannot therefore refuse to acknowledge the existence of a morbid poison in cholera, and to that limited extent we agree with Dr Johnson. But it is far otherwise with his pathology. Even supposing that the very prettily drawn diagramatic sketch which does duty as frontispiece represented a constant phenomenon, which we deny, we think it could be very readily explained in a manner at once more rational and more consistent with facts than that employed by Dr Johnson. Thus, if we adopt Dr Parkes' theory, to which all opinions seem now converging, that "there is some impediment or arrest of the circulation in the capillary system generally," we can readily understand that so long as life continues there is a natural tendency for the pulmonary arterial system to become congested, because while life remains the circulation must go on. Under ordinary circumstances, however, each ventricle sends into the large artery connected with it nearly a similar amount of blood at each pulsation, the same amount being sent to the lungs that is taken from them; but it is obvious that whenever the capillary circulation becomes generally impeded, a portion of the supply is at once cut off from the left ventricle and detained in the pulmonic arterial system, congestion of that system being thus at once commenced; and this congestion is continued and increased by the fact that the right ventricle draws its supplies from the left, through the intervention of a large venous reservoir, which is capable of filling it many times, even though its supplies were wholly and not merely partially cut off. Pulmonic arterial congestion does not, therefore, require for its explanation the adoption of any such untenable supposition as that of a poison, acting especially upon the pulmonic, and not also upon the systemic capillaries, while it is consistent with the more probable idea of a general interruption to the capillary circulation, an interruption probably produced not so much by vascular constriction as by a more or less complete cessation of all the vital acts of secretion or nutrition, and consequently of circulation throughout the frame, wholly independent of drainage by evacuation, and often most marked where there are no evacuations, and depending apparently upon a more or less complete paralysis of both our nervous systems, so as more or less completely to put a stop to that interchange of influences between these systems, upon which, according to all our present knowledge, these vital acts depend. At the same time, according to Dr Hensman (Army Medical Reports), this partial cessation of the circulation is accompanied by such an amount of capillary congestion, especially in mucous membranes, as to give rise to serous effusions containing the saline and organic contents of the blood, according to the nature of the mechanical facilities present. Hence the rice-water evacuations from the intestines, often filling also the bile ducts and gall bladder, and, as we have seen, the breasts of nursing women; hence also the albuminous exudation in the kidneys. Thus we see that the facts upon which Dr Johnson has built his elaborate pathological theory are capable of an explanation totally different from that which is given, while his theory is not only inconsistent with all that we know of the history of zymotic diseases, but is also wholly illogical and inconsistent with our present physiological views; we do not think, therefore, that he has succeeded in giving "a very clear and definite view of the pathological interpretation of symptoms, and of the mode in which remedies may assist the natural process of cure;" we cannot regard him as a "safe guide" to treatment; and we do not think that the treatment attempted to be revived by him holds out any prospect of being more successful in the future than it has been in the past.

Mr SEDGWICK'S book has been written to illustrate "the theory which refers the disease to functional disorder of the central parts of the sympathetic nervous system, excited through the medium of the stomach," a very old theory; having been started—though not maintained—by Dr Jameson, in his history of the first epidemic of cholera in Bengal, and one in favour of which, if properly modernized, a good deal may yet be said. However, Mr Sedgwick modestly says, that in an inquiry "which has baffled many of the most distinguished members of the profession, he has no right to anticipate any great success," and we do not think he has been disappointed; in particular, his treatment,—bloodletting,—will, we think, be followed by but few in the present day.

Mr MACLEOD does not believe in the contagiousness of cholera: he thinks the poison is neutralized in the body, and that the discharges are innocuous; there could therefore be no harm in retaining them; nevertheless, he is opposed to astringents, and in favour of a treatment by emetics and purgatives to promote the restoration of the biliary secretion, which he believes to be wholly checked for the time. He recommends cold drinks and brandy, frictions with cajeput oil, and ten-grain doses of calomel repeated frequently, with or without castor oil and tartar emetic.

Dr SHRIMPTON'S work was originally written in French, and is useful in giving French views and illustrations. He disbelieves in contagion or in the existence of any virus or morbid poison. Collapse he believes to be produced by electric interference with the functions of the elementary cells of the whole economy. The treatment he proposes is chlorodyne, opium, and stimulants in the stage of simple flux, preceded by the production of what he calls "a slight general commotion" by the aid of one or two scruples of ipecacuanha; subsequently, in the stage of confirmed cholera, he recommends the application of external warmth by hot bottles, etc., the use of warm drinks, port-wine, and ether, or chloroform administered in gelatine capsules; ether should also be given as an enema; opium and chlorodyne should now be discontinued; and if preferred, ice and cold drinks may be allowed without detriment. Stimulants and warmth are however his favourite remedies, his chief object being to bring about reaction.

Dr CRISP'S work consists of a series of essays strung together. In one of them he tells us that Dr Hughes Bennett, when a candidate for the chair he now occupies, "told the town-councillors (of Edinburgh), in his trial dissertation by a kind of Scotch concours, which very properly elected him," etc.,—a curious piece of imagination on his part, as here, as every one except Dr Crisp knows, we have never had anything so good as a competitive examination for any of our professorial chairs. We trust his other statements have a better foundation in fact. Of the 135 pages which comprise the work, 89 are occupied with the cattle plague, with which at present we have nothing to do. Dr Crisp believes in a specific poison in cholera acting upon the nervous centres: he believes that cholera is communicable, though to a less extent than smallpox, scarlatina, and other zymotic affections; he regards the eliminative treatment in the early stage of cholera as likely to prove injurious, and that the great majority of cases, if seen early, are readily controlled by a full dose of opium, combined with astringent medicines; but he also believes that, in the second stage of cholera, when serious vomiting and purging set in, the calomel treatment properly carried out—one grain every fifteen minutes, or two grains every half-hour for several hours—offers the best chance of success, though, like every thing else, it is too often of no use.

Dr GIRDWOOD'S pamphlet is written to advocate the one line of practice indicated by its title-page, and the key to his views is to be found in the statement that "the restoration of the normal condition of the blood, by the injection of water containing the lost saline ingredients, is specially indicated, and *recovery not to be expected until that restoration be accomplished*." The italics are Dr Girdwood's own.

Dr MOORE'S pamphlet is a reprint from the *Glasgow Medical Journal*, and is chiefly occupied with the question whether several cases which occurred at Ibrox, near Glasgow, were cases of true cholera, or of fish poisoning. The proofs he adduces seem to point to the former, and he alludes to the difficulty of explaining their occurrence — as an isolated group — in accordance with present theories.

Dr FERGUS'S pamphlet is chiefly occupied with details regarding the preventability of cholera, and its perfect curability in the stage of diarrhœa by means of a sufficiently large dose of laudanum, not less than 40 minims, or a small teaspoonful, repeated according to necessity as indicated by the recurrence of the diarrhœa. Perfect rest in bed is also recommended, as well as the application of hot lintseed poultices to the abdomen, and, if necessary, hot bottles to the feet. In the collapse stage he considers opium a poison, yet, in all stages, he is thoroughly opposed to eliminative treatment. In collapse he prescribes ice, small quantities of beef or chicken tea, arrowroot, milk and water, etc., the external application of heat, and friction to the limbs with or without stimulating liniments. Dr Fergus brings forward a large number of cases in support of his views, and his pamphlet is certainly likely to produce a favourable effect in regard to the abortive or opiate treatment of the first stage of cholera.

Dr BEAMISH is also strongly in favour of cutting short cholera in its premonitory stage by means of a large dose of opium; he thinks that in the stage of collapse no drug has its specific effect, while even if opium could be then absorbed, it is not so specially indicated as the patient is now drained, and the losses it was meant to check have subsided of themselves. Nevertheless, he does not discard opium even then, but cautiously employs it in smaller doses, repeated according to circumstances, to assist in stimulating the brain and nervous system and bringing about reaction. Even for ordinary diarrhœa, especially in cholera times, he considers it useless to give less than from 25 to 40 minims of laudanum to an adult; and in actual cholera, with distinct pulse, he considers a minimum dose to be three grains of powdered opium with a drachm of the tincture, or about six or seven grains in all; and in a more aggravated case, short of collapse, a still larger dose, "say six grains and a drachm of the tincture, or about nine or ten grains," and these doses are repeated as required. Along with this he gives copious warm or cold drinks, uses stimulants "judiciously," applies external heat, sinapisms, or even small blisters, and over the raw surfaces made by them he sprinkles morphia. When we read of the large doses of opium employed, and supposed to be required to check even an ordinary diarrhœa in cholera times, and of the still larger doses employed to check the discharges in true cholera, and when we couple these statements with the fact that when collapse sets in these discharges frequently cease spontaneously, we certainly feel but little inclined to sympathize with Dr Johnson's self-reproaches for having induced collapse by administering to an adult three doses of five grains of Dover's powder at hourly intervals (vide Notes, p. 74). As a counterblast to his case, we may relate another, the particulars of which we happen to know, and which occurred and was published many years ago. A little fellow, two years of age, partially collapsed, had taken fifteen minims of laudanum in two doses, when the vomiting ceased, all the symptoms of collapse did not increase, but disappeared, the pulse became full, the cheeks rosy, and the blue lips and dark circles round the eyes gradually recovered the hues of health; but after an hour or two, he woke up, vomiting recommenced, and back came the collapse; a single dose set him asleep again, and from that time he progressed steadily. "Look upon this picture, and on that," and say which exhibits the most probable evidence of the active agency of this drug? Collapse produced in an adult by 15 grains of Dover's powder, or collapse cured in a child of two years of age, by 15 minims of laudanum. Omit all consideration of the remarkable fact of the collapse relapsing on the evanescence of the narcotism, and again ceasing on a repetition of the dose, and say, from what we know positively of the action of opium in children, as well as of the only too frequent results of cholera which has attained the stage of commencing collapse, is there the faintest possibility that this child could have escaped unscathed from such enormous doses of two such fatal poisons had they acted together and not been antagonistic? Yet the whole science of therapeutics is based upon probabilities; and in enunciating principles it behoves us, as we would be reckoned wise and prudent, carefully to give effect to all the various circumstances which influence these probabilities. As far as concerns cholera, this has not yet been done; in all the various pamphlets and brochures upon the treatment of cholera, the proportion of recoveries under the homœopathic and expectant systems has never once been alluded to; yet in estimating the value of any method of cure, we must know whether it is better ·than nothing, and to know that we must find out the value of nothing. As a sample of what we mean, we may mention that Dr W. Robertson mentions a girl in whom the state of collapse was so profound that no suitable vein could be found into which saline injections (the only feasible remedy then known for so desperate a case) could be found, yet she ultimately recovered. Now, had this patient been injected or got calomel or any other drug, her recovery would of course have been attributed to it, and we demand what proof have we that any collapse cases were the better of treatment,—by drugs at least; for in none of them has the recovery been preceded by any physiological action, while recovery has followed the use of remedies so diametrically opposed to each other in their actions as to lead us at once to conclude that treatment during the stage of collapse is of comparatively little avail. In regard to the premonitory stage of cholera, on the other hand, there really seems to be a considerable amount of proof in favour of the utility of opium in full doses sufficiently often repeated; but even in regard to this we think the deductions from pathology are against the utility of the opium depending solely upon its astringent action, and in favour of its neutralizing the tendency to collapse by means of its peculiar action on the nervous system,—an action which we know to be similar in kind to that of strychnine; and this would lead us to expect much benefit from strychnine, especially if hypodermically injected so as to ensure absorption in the early stage of collapse. Yet, strange to say, Dr Beamish says, in regard to strychnine, that he trusts when "experiments are tried that they may be founded on something rational," and Dr Hensman, the advocate for strychnine, regards opium as "inadmissible" and its influence as "pernicious;" their opinions clearly proving how little either of them knew of the physiological action of the drugs whose agency they were advocating. We could not provide a more apt illustration of the chaos of opinion prevailing in regard to the treatment of cholera, or give a more striking example of one very powerful reason for this chaos; and the same may be said in regard to its pathology. No pathology of cholera has yet been worked out which is capable of giving a clear explanation of all the phenomena of cholera — including the remarkable post-mortem movements and rise of temperature—in accordance with the doctrine of modern physiology, and yet the reason why this has not been done seems mainly that it has not been attempted. Those who attempt it, however, must remember that limited views are sure to lead to error, and if they expect their opinions to have any influence upon therapeutics at all, they must seek by enlarging their sphere of observation to render them such as may be capable of simplifying the future by explaining the past in accordance with facts, and with our present scientific knowledge, and avoid those limited and narrow-minded glimpses of disease which as yet have led to nothing more than startling "Eurekas" from every quarter of the compass, bewildering alike by their number and diversity.

Traité expérimental et clinique de la Régénération des Os, et de la Production artificielle du Tissu osseux. Par L. OLLIER, Chirurgien-en-chef de l'Hôtel Dieu de Lyon. Avec 9 planches gravées sur cuivre, et 45 figures intercalées dans le texte. Tome second. Partie clinique. Paris: Victor Masson et fils: 1867.

Experimental and Clinical Treatise on the Reproduction of Bone and the Artificial Production of Osseous Tissue. BY L. OLLIER, Senior Surgeon to the Hotel-Dieu of Lyons. With 9 copper-plates and 45 woodcuts. Vol. II. Clinical Part. Paris: Victor Masson & Son: 1867.

[SECOND NOTICE.]

IN the first volume of this elaborate work we found laid before us the results of a vast number of experiments, explanatory of the structure of bone, the laws which regulate its growth and development, and the possibility of its reproduction or transplantation. These being performed on animals, have rather a physiological and scientific than a surgical and practical bearing. In this second volume the author endeavours to reap the harvest of the seed so abundantly sown, to find the practical value of the information already obtained. It is accordingly illustrated by cases more than by experiments; and dealing with opinions which may be opposed, rather than with long series of experiments which must be repeated before they can be contradicted, the style is necessarily argumentative and controversial as well as didactic.

Chapter I., on necrosis and reproduction of bone, begins with a definition of the two chief varieties of necrosis, and their respective chances of a good reproduction, so short, so clear, and (without being original) so well put, as to justify an almost verbatim quotation. The almost epigrammatic brevity and brilliancy of M. Ollier's style in his best bits are very difficult to imitate, far less to reproduce in our more sober English.

The Different Varieties of Necrosis.

"Occasionally necrosed bones resemble portions of healthy bones, at other times they present the appearance of diseased bones. In the one case the sequestrum presents, to the naked eye and the microscope, the characters of normal bone, in the other it resembles inflamed bone, frequently rarefied in texture, sometimes eburneated; the death of the part in itself making no alteration in the original characters of the tissue. This difference of appearance indicates the two chief modes in which necrosis occurs. In the first, the bone dies, as it were, before it has been sick. It passes rapidly, under the influence of an acute inflammation, from life to death, and this we call *simple or primitive necrosis*. In the second, the bone has been already diseased before its death; it has undergone various changes under the influence of inflammation; it has become opened out, vascular, and softened. The death has been secondary, often by a long interval, to the commencement of the disease: this we call *carious or secondary necrosis*.

"Between these two types there exist, doubtless, intermediate varieties, as there are between the sudden gangrene of a limb, the result of obliteration of its principal artery, and the loss of the same member by the successive mortification of its various structures, under the influence of a low type of inflammation, or of phagedænic ulceration.

"These two varieties of necrosis differ very remarkably in the point of view of the future reproduction of the bone.

"After a simple necrosis, the periosteum, unless already destroyed by the violence of the inflammation, will set itself at once to reproduce a new bone. . . . .

"After a cario-necrosis, on the other hand, the original malady runs its course, if the general diathetic conditions which maintain it still remain; the necrosis is merely an *accident* of the disease, unless, indeed, it removes all the carious portion; in that case it is its *termination*.

"No repair of bone goes on, for the tissues which ought to contribute to it are themselves altered; it confines itself merely to peripheral osteophytes. The periosteum fungates, and is more or less destroyed up to the very border of the altered bone; and without the intervention of art the disease has no tendency to cure. The sequestrum not being in itself the whole disease, it is necessary to attack the caries. . . . ."

The rarity of complete necrosis of a diaphysis, and the influence of age, are next discussed and illustrated by cases. One is so rare as to deserve mention. A girl, æt. 12½, had suffered eight and a-half years before admission from every symptom of necrosis of the femur. Only small fragments of bone had escaped. The limb was slightly longer and rather thicker than the sound one. Sinuses existed. The entire diaphysis of the femur of a child of four was easily removed from the centre of the new bone.

When are we to interfere surgically in cases of necrosis? is the next question. The amount of irritation and suppuration caused by a sequestrum may determine its removal before the new bone is sufficiently firm, for the advantage of the stimulus to ossification afforded by its presence is more than counterbalanced by the risks to the health from profuse suppuration. That, except in special cases (such as necrosis of the jaw), necrosis should not be touched before it is detached, is a very general rule. We, in this country, have little experience in means for hastening exfoliation. M. Ollier's conclusion is, that while shaking them may do harm, solvent injections are useless if feeble, irritating if strong. He also reports, as might be expected, that trepanning a dead frontal bone in various places did not in the least hasten its separation, as the half which he let alone was not behind the half which he scraped and trepanned in various places.

Chapter II. treats of losses of substance which do not affect the length of the bone, under the three heads of abrasion, "evidement," and cauterization. Comparatively little is said under the heads of abrasion and cauterization, with the means by which nature repairs the losses of bone. Under "evidement," which may be translated by "gouging," or "hollowing out," or "scooping out," we have a very full description, and a free criticism of M. Sedillot's well-known method of treating diseased bones and joints. We have no room to quote, nor could we well abbreviate the argument, but the result is, that M. Ollier strongly upholds his own "sub-periosteal resections" against the "gouging" of the famous surgeon of Strasburg. The operation which Chassaignac has called "égrugement" or "scraping," M. Ollier places in the same category as the "evidement." To those interested in the brisk warfare on the subject of these various though so similar operations, this chapter will afford much information.

Before leaving the subject of the comparative merits of the methods of operating devised by these eminent surgeons, it may be interesting to allude to—and even to quote a sentence or two from—the report of the sub-committee of the French Academy regarding the "grand prix de chirurgie." The prize consisted of 10,000 francs given by the Academy, with an equal sum added by the Emperor, and the subject was, "Preservation of Limbs by the Preservation of the Periosteum." As is well known, the prize was divided equally between MM. Sedillot and Ollier.

> "If we listened to one of these authors only, it would be necessary completely to reject all the labours of the other. But looking at the subject with care, it is not difficult to bring the difference in their opinions back to a common result. . . . Each has its own advantages. . . . To form a new nose, to close fissures in the palate, M. Ollier's method has undoubtedly great value. On the other hand, it is clear that the plan of gouging bones is more suitable than periosteal resection in adults or the aged; and that each method may aid the other without excluding it. This it is which has led the sub-committee to propose that the prize should be equally divided between the two authors."

Chapter III. treats of the reproduction of bone by periosteal flaps, and consists very much in a series of proofs from clinical facts of the opinions previously supported by experiments on animals. The first case is long, but well worth perusal. It demonstrates the possibility of the reproduction of bone after the complete subperiosteal resection of a large piece of *living* (not *necrosed*) shaft. The upper part of the humerus of a girl (æt. 15) was diseased, not dead, the periosteum was detached during the operation, and a piece, nearly five inches in length, including the head and tuberosities, was removed. A very good arm, little more than two inches shorter than the other, resulted.

A historical sketch of such periosteal resections is then given, including reports of the cases of Delamotte, Coutavoz, Vigaroux, and White of Manchester, the last three being quoted *verbatim* in an appendix.

While some surgeons deny the possibility of the regeneration of bone by the periosteum, there are others who believe that this may take place independent of its assistance. The first say the periosteum is insufficient, the others say it is useless, or at least that its place can easily be taken by the surrounding soft parts. Bones are reproduced, they say, yet we took no heed of the periosteum. Ollier explains these cases, to his own satisfaction at least, by the following observation:—"These cases are chiefly in children, and, if not, in adolescents. Now it is impossible at that age to make your resections anything else than subperiósteal, for *the periosteum then adheres more firmly to the soft parts than to the bone itself.* It is impossible to make a compound fracture of a child's limb and project the bone without its being completely stripped of the periosteum. Hence, in such cases, surgeons perform subperiosteal resections without wishing to do it, and without knowing that it has been done.

Chapter IV., on subperiosteal resections, begins with a section which we may epitomize in a single sentence. Subperiosteal resections are not merely conservative but reparative operations. The author then demonstrates the *possibility* of such operations. Surgeons who object that the preservation of the periosteum is impossible in the living subject, have forgotten two anatomical facts which are easily verified: one, a fact of healthy anatomy, that the periosteum in youth is thick and easily detached; the other, of pathology, that in inflamed bones also the conditions are the same.

Rules for operation are then given of so thoroughly practical a nature as almost to deserve quotation entire. We have room only for the heads of the subject. 1. Incision of skin should allow the bone to be reached by the shortest road. 2. Deeper incisions. If possible neither muscles nor tendons should be cut. If a muscle must be cut, cut it so as not to cut its nerve. Cutting muscles at all has a double danger—paralysis and new insertions in wrong places. 3. Raising of the periosteum, by rugines or scrapers, generally sharp only at one edge, and varying from the old form by having their cutting edge in the axis of the instrument, not across it; and by a straight instrument called "détache-tendon." These instruments are figured and carefully described. 4. Sawing of the bone, generally by chain-saws and with certain precautions. The treatment by splints, bandages, and extension, is then carefully described.

Conditions favouring reproduction by periosteum in man: Age. The years between 13 and 15 have given the most favourable specimens. Up to 20, or even 25, the periosteum still possesses its power of reproduction. After 30 or 35, it is impossible to trust to any reproduction *from healthy periosteum*, i. e., *not prepared by a previous inflammatory attack.*

Anæmia, erysipelas, the exanthemata, have all, as might have been expected, delayed or prevented reproduction of bone.

Chapter V., on the subject of the general indications for subperiosteal resections, begins with some very sensible remarks on the differences between the chances of patients in hospitals, in private practice, and in military campaigns—pointing out the comparative risks in hospitals of any operation which involves (as subperiosteal resections must do) prolonged suppuration, and the greater chances of recovery in private practice, especially in the country, and even during a campaign, if the general hygienic conditions are good.

It then goes on to discuss the various maladies or injuries of bone in which subperiosteal resection may be used—under four heads:—

1. In compound comminuted fractures.
2. In inflammation of bone and suppurating articular extremities.
3. In organic lesions of bone. In these it can very rarely be applicable, never in malignant growths; only in very rare cases of benign cysts, or certain enchondromata.
4. In cases where it is necessary to remove a bone or portion of bone as a preliminary step to operating on parts below, as when the upper jaw is removed to get at a fibrous polypus, or when one has to trepan the skull.

Chapters VI. to IX. inclusive, are devoted to the details of such cases of subperiosteal resections of *bones* as the author has watched to a conclusion in his own practice, or obtained in the practice of others; while chapters X., XI., and XII., contain similar experiences in the treatment of the principal *joints*. By referring to a few of the more important cases, we may give the reader a specimen of what these practical chapters contain, and how the previous experiments are now made to bear upon practice.

In trepanning, if we make the incision right down to the bone, and then carefully scrape off the periosteum and retain it with the flaps, we may almost be certain to obtain entire reproduction of bone even in adults.

Excision of the upper jaw may be requisite for tumour: in such cases the periosteum should not be preserved; or for necrosis. Here, again, it is unnecessary to trouble about it, as nature has already effected its separation from the bone. But in cases where the upper jaw has to be removed to gain access to a tumour behind it, then the periosteum both can and ought to be preserved. One such case is recorded, where the jaw was reproduced in front, but the palate portion did not ossify. Full details are given both of the method and its results.

In excisions of the lower jaw, M. Ollier recommends that an attempt should be made to preserve periosteum, especially of the outer surface of the bone. He then recommends that the bone should be sawn in front at the point where it is to be removed, and then the muscles adherent to the part to be taken away should be separated from their attachments by the *détache-tendon*, so that not a fibre should be cut; this can easily be managed in young subjects.

That then the bone, which is now mobile, should be seized by strong forceps and forcibly torn or twisted from its attachments at the joint. This latter procedure, which sounds severe and barbarous, has been proposed and performed by Maisonneuve, and strongly opposed by Chassaignac; but M. Ollier claims for it less risk from hæmorrhage or from injury of the important structures in the vicinity of the joint. In one recorded case it seems to have been easy and the result favourable.

In resection of the bones of the trunk, if a *rib*, or part of a rib, requires to be removed for necrosis, it is generally easy to do it subperiosteally, for the periosteum is already thickened and detached by the disease. If it be thus done with care there need be no fear of injuring pleura or intercostal artery, and the result is almost sure to show abundant ossification. The *clavicle*, if necrosed partially or entirely, can generally be very easily removed, and the periosteum which remains provides in time a most efficient substitute. Complete necrosis seems to be rare, as M. Ollier has no cases of his own to report, and can find only three to quote. Last August a case occurred in Mr Syme's hospital practice, in which, after a very severe muscular effort in a strong man of 35, the whole diaphysis had become necrosed, and was very easily removed by freeing one end by an incision, and then drawing the pure white bone out of the gutter of periosteum in which it lay, and which had already (six weeks after the injury) formed a very efficient protection to the vessels and support to the shoulder.

Subperiosteal excision of the scapula has not yet been performed on the living subject. M. Ollier gives very precise directions for its performance on the dead subject, which, however, we fear would be impracticable on the living; for the shock and hæmorrhage involved in the removal of so large a bone would be much and probably fatally increased by the prolonged dissection required. He is not so well informed as usual in the history and results of this operation, as he seems to be aware of three cases only, Langenbeck's, Jones', and Syme's first.

A rare and interesting case of subperiosteal resection of the *symphysis pubis* for necrosis is recorded, in which the patient was cured, and the bone was reproduced.

A subperiosteal resection of the *coccyx* concludes the account of these operations as practised on the trunk.

Resections of the humerus in whole or part are not very often needed. M. Ollier strongly recommends that, in cases of compound fracture with comminution, the splinters should be carefully removed, leaving the periosteum; and portions of bone which have been deprived of periosteum, even if not broken off, should be also cut away with saw or pliers, there being little risk of a false joint, and much less fear of prolonged suppuration, if only enough periosteum be retained.

Resections of the bones of the forearm are illustrated by one or two interesting cases. When we remember, however, the remarkable reproductions of bones of the forearm, even where no care has been taken to retain the periosteum,[1] when the patients are young, we must not lay too much stress on any special precautions in that direction.

On the subject of subperiosteal resections of the whole thickness of the shaft *of the femur*, M. Ollier very wisely observes that in a great hospital these operations are so very dangerous, that he would prefer amputation of the thigh as affording a better chance of recovery. Subperiosteal resections *of the tibia* are rendered easy of performance by the subcutaneous position of the bone, and have one great element of success secured by the immobility which is given by the presence of the fibula; hence there exists a considerable number of cases already, and there is more likelihood of such procedures being generally adopted than in the cases of the femur or humerus. M. Ollier gives a large series of cases, in some of which, including one by Mr Holmes, the results have been very satisfactory. In describing the operation, very full details are given: the chief points to be attended to are, that the incisions through the periosteum should be made in the axis of the bone, and that after the periosteum has been carefully separated from the bone about its centre, a chain-saw should then be passed round it, guarded by a grooved director, and thus the bone should be fairly cut across, and then the two halves may be removed separately by means of strong forceps, with less injury to the periosteum than if the bone had been removed entire.

Injury or disease of the *fibula* being comparatively rare, very few observations have been made. M. Ollier, however, remembers the wise counsel that its upper extremity should not be dealt with rashly, on account of its frequent communication with the great synovial cavity of the knee-joint and the consequent risks.

Resections *of one or more bones of the tarsus* are operations which depend for their success more on the nature of the disease for which they are performed than on any little specialties in the nature of the operative procedure. Thus the most skilfully performed periosteal resection, even by M. Ollier, if for scrofulous disease, will almost certainly result in failure from recurrence of the disease, while if it is only for some local inflammation or necrosis that the operation be undertaken, it matters comparatively little whether periosteum be left or removed, whether M. Sedillot performs his "evidement," M. Ollier his "resection sous periosteal," or whether, in less fine terms, we simply make our incision over the spot, remove the exfoliation, or gouge out the carious cavity. M. Ollier naturally enough does not homologate this opinion, but strongly supports his own subperiosteal resections, and relates cases in which after complete removal very good new heels were formed, to which the tendo Achillis preserved its attachment. Besides the os calcis, the only other bone of the foot which is of much interest from this point of view, is the first metatarsal. It is often diseased; it is of the very greatest importance in maintaining the form and giving spring to the foot; hence efforts may be made to save it, which would be hardly needed for any of the other metatarsals. The risk in all cases of removal of the first metatarsal, either in whole or part, is that the great toe is drawn upwards on the foot and rendered useless (we have seen one case in which it was placed on a level with the centre of the second metatarsal); to avoid this, M. Ollier figures an ingenious apparatus, in which by means of little india-rubber "toe-stalls" attached to cords and pulleys, the toes can be kept in their own position till consolidation has taken place.

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[1] *E.g.*, case of excision of elbow-joint and whole ulna, by Dr Williamson. Williamson's Military Surgery, p. 228.

We have left ourselves but little space to analyze M. Ollier's accounts of subperiosteal resections of the great joints. The principle is—

> "The complete preservation of a periosteo-capsular flap,—that is to say, a fibrous canal, which lies between and unites the ends of the resected bones, —capsular ligament in its centre, periosteal at each end. At the periosteal portions the bones are reproduced, the capsular portion remains fibrous, and the joint is restored.—P. 298. . . . Once we reserved the 'sub-capsulo-periosteal resections' for these cases only which had recently suppurated, or were the result of accident, fearing, in chronic cases, to leave in the wound the fungating remains of the old joint. Clinical experience has shown that our fears were exaggerated, and that in all cases of resection it is an advantage to retain the periosteo-capsular canal. In destroying the fungous remains and altering their character by the use of the actual cautery, or of nitrate of silver at the time, and afterwards by stimulating dressings, the capsule may be placed in a condition favourable to cicatrization."—Pp. 300, 301.

M. Ollier owns that it may be urged against his more difficult and complicated method of treatment, that most excellent results have been obtained without it, by Syme, Textor, Langenbeck, and others. Owning this, he hopes that by his method the good results, which have hitherto only been exceptions, may become the rule; and points out that these surgeons who have been so successful are the very men who have always shown greatest knowledge of, and confidence in the powers of the periosteum. Page 301, note.

The sections on the method of operating, on the importance of after-treatment, and the choice of suitable cases, are very interesting.

Resection of the shoulder-joint does not vary much from the ordinary method which for many years has been practised here. Instead of cutting the various tendons close to the bone, M. Ollier scrapes or shoves them off with the "detache-tendon." We never remove the capsule, and he generally contents himself with gouging the glenoid cavity. His results seem in no respect superior to the ones with which we are familiar.

The notice of excision of the *elbow-joint* is very long and full of interesting observations. M. Ollier recommends two different incisions, according to the case. In disease, he gains access by one zigzag incision along the outer side of the humerus, then slanting across to the posterior border of the ulna, and following it to the required length. For anchylosis, he uses a double incision, one at each side of the olecranon, the external one being the largest. We cannot see the least advantage which this has over the longitudinal one.

Of twelve cases of subperiosteal resection of the elbow, performed at Lyons, three died,—a very large percentage; the numbers are too small for any great stress to be laid on them.

Some of his results are most excellent, both as to the shape of the joint, range of movements, size of limb, and power; but we believe this is due more to the sensible after-treatment than to any specialty in the method of operating. And when speaking of results, he still quotes Mr Syme's well-known case of the railway-guard as the most remarkable which he has been able to discover. M. Ollier has never practised excision of the *wrist* on the living body, and he is by no means up to the latest mark even in his directions for the performance of the operation on the dead body. He ends his account of resections of the upper extremity by giving a case in which he excised with very fair result the phalangeal joint of the thumb in a man aged fifty-eight.

M. Ollier has never practised *excision of the hip-joint* in the living body, chiefly from the sufficiently well-grounded fear of the special dangers of such an operation in a great hospital. He gives a full description of the method of subperiosteal resection. The same absence of clinical facts diminishes the value of the proposals which M. Ollier throws out guardedly on the possibility of aiming at a mobile articulation in *excision of the knee-joint*, by the preservation of periosteum along with the capsule of the joint. Of three cases on which M. Ollier performed excision of the joint in the usual method, all died from causes immediately dependent on the bad hygiene; and this want of success has naturally discouraged further attempt. Judging from the cases reported, subperiosteal resection *of the ankle-joint* seems to be a most successful operation, specially so in traumatic cases. Langenbeck's series of five (now so well known) are detailed, and a few more by M. Ollier himself are well worthy of perusal.

Chapter XIII. contains an account of some accidents after fracture due to an excess or deficiency of callus. A most ingenious operation is described by which M. Ollier freed the radial nerve from its imprisonment in a mass of callus, and by this means cured a paralysis which had been caused by pressure on the nerve. The subject of treatment of false joint is fully discussed. A case is given in which a false joint, which had not been improved by four months' confinement, was cured by various perforations of the fibrous band of union, aided by subcutaneous scraping and elevation of the periosteum, as was lately practised in an interesting case by Dr John Duncan of Edinburgh.

Another case in which a false joint of the femur was operated on, by its death from pyæmia, adds another to the long list which prove the exceeding danger of compound fractures of the femur in great hospitals.

Chapter XIV. returns to the author's favourite subject of transplantation of bone. While he has no clinical facts of his own to add to the experiments on animals in the first volume, he quotes the very curious attempt made by Percy to transplant a piece of beef-bone to fill up a vacancy left between the fragments of a fracture of the tibia with loss of substance. This experiment failed. Though he fears it is a myth, M. Ollier quotes an amusing account of a successful case, to show that the idea of such transplantations was no new thing even in Percy's day. It is taken from the *Observationes Medico-Chirurgicales* of Job à Meckrem, which date as far back as 1670.

> "An ecclesiastic, named Krauwinkel, told him that, being in Russia, an engineer of that nation received a blow on the head from the sabre of a Tartar, which separated entirely a large piece of the hairy scalp with a corresponding portion of skull, the detached fragment being left on the field of battle. The surgeon, to close the orifice, detached from the skull of a dog, killed for the purpose, a portion of bone of the same size and shape as the piece that was deficient, and managed it so well that the wounded man was perfectly cured. But our gentleman, in the fulness of his joy, told how he had been cured, and at once the thunders of the church were levelled at him. To obtain re-entrance into the communion of the faithful, he had to strip himself of the unclean spoils of the hound, although already firmly consolidated, and submit to a treatment more consistent with the character of a Christian."

M. Ollier gives his views of the essentials to a successful transplantation of bone, so far as is known, as follows :—

> "It is necessary that the bony flaps should be taken from the same subject, or from one of the same species. The age of the animal is not of vital importance, but the flaps reunite best in young animals. In old subjects it is better to use a flap cut from a young one. The younger the tissue is, and the more soft elements (periosteum and medulla) it contains, the more suitable it is to form a flap."

Chapter XV. is on osteoplasty. "Osteoplasty is an operation which has for its aim the restoration of the form of organs by means of bony tissue. It is an autoplastic operation which differs from ordinary ones in consisting of flaps which either already contain bone or which are destined eventually to ossify."

The latter variety of operation will probably be in the end found the more useful and universally applicable. We cannot do more than merely mention this interesting chapter. It contains directions for operations chiefly in cases of deficiency of the nose and of cleft palate. Langenbeck's great success in the treatment of the latter deformity, by raising periosteal flaps and approximating them in the middle line, receives full justice.

Chapter XVI., and last, describes operations which M. Ollier calls "temporary resections" of the bones of the nose and face, undertaken for the purpose of giving free access to deep-seated fibrous polypi. The results were good and without deformity. It concludes with a proposal that in amputation an attempt should be made to save a flap of periosteum, which may be left adherent to the soft parts, and with them be folded over the face of the cut end of the bone. One would be afraid that such a flap might give rise to annoyance by irregular or excessive osseous deposits.

Some additional notes and a fairly copious table of contents finish the volume and the work. Our very full analysis of its contents has left little space, and, perhaps, little need for criticism. This whole book shows that it is the work of an enthusiast, an able indomitable worker, an experimenter who knows not what weariness or satiety mean, an observer who can group what he has read and what he has seen into a system. The perfect clearness, and almost epigrammatic smartness of the style, make the book very easy and pleasant to read. Without denying that here and there M. Ollier is wrong, and still oftener that his enthusiasm may have made him too sanguine, there is no doubt, that in this, his life work, the great Lyons surgeon has set his mark on the conservative surgery of the nineteenth century, and thrown out hints and established principles which may possibly, in their adoption and elaboration, make the fame of half-a-dozen younger surgeons.

The Journal of Anatomy and Physiology. Conducted by G. M. HUMPHRY, M.D., F.R.S., Professor of Anatomy in the University of Cambridge; ALFRED NEWTON, M.A., F.L.S., Professor of Zoology and Comparative Anatomy in the University of Cambridge; WILLIAM TURNER, M.B., F.R.S.E., Professor of Anatomy in the University of Edinburgh; E. PERCEVAL WRIGHT, M.D., F.L.S., Lecturer on Zoology in the University of Dublin; and J. W. CLARK, M.A., late Fellow of Trinity College, Cambridge, Superintendent of the University Museums of Zoology and Comparative Anatomy. No. II., May 1867. Macmillan and Co., London and Cambridge.

THAT a Journal of Anatomy and Physiology in our country is required, no one acquainted with the rapid progress of these sciences, and with the example of continental nations will dispute. It will be remembered that, about fifteen years ago, the late Professor Goodsir commenced a periodical of this description,—the Annals of Anatomy and Physiology,—which his other laborious occupations prevented him from carrying on. The journal now before us, under a combination of distinguished editors, which will ensure the co-operation of our principal Universities, presents conditions peculiarly favourable for success. The two numbers which have appeared contain important contributions of a high order of merit, sufficient to establish the character of the journal as a scientific periodical of the first class. Among the original articles we need only allude to the papers by Professors Humphry, Huxley, Turner, and Cleland, Dr Lightbody's able investigations on the cornea, Hulke on the retina, Ransom on the protoplasmic movements in the eggs of osseous fishes, Strethill Wright on zoophytes and protozoa, Fraser on the Calabar bean, Gamgee on poisoning by carbonic oxide, etc., etc. These papers are amply illustrated. Reviews, Reports on Anatomy by Professor Turner, and on Physiology by Dr Rutherford, together with the translation of valuable foreign papers by Dr Moore of Dublin, and, in the present number, by Dr Charlton Bastian, who brings before English readers the researches of His on the perivascular canals in the nervous centres,—form useful additions to the original matter.

Should this new journal receive the support which its excellence deserves, it cannot fail to exert a powerful influence in the promotion of original anatomical and physiological investigations in our country, and its success must therefore be regarded as a matter of importance by all who are interested in the progress of these sciences, or who are aware of the valuable assistance which, at the present day, medicine derives from anatomy and physiology. The time has come when medicine cannot advance without the promotion of the higher scientific learning which, in the education of the physician of the present day, must be combined with, though we hope it will not take the place of, the erudition in classics and mental philosophy which distinguished the physicians of a past age.

Quain's Elements of Anatomy. Seventh Edition. Edited by WILLIAM SHARPEY, M.D., F.R.S., Professor of Anatomy and Physiology in University College, London; ALLEN THOMSON, M.D., F.R.S., Professor of Anatomy in the University of Glasgow; and JOHN CLELAND, M.D., Professor of Anatomy in Queen's College, Galway. London: Walton: 1867.

THE present edition of this admirable text-book has undergone complete revision, and has, with much labour and with scrupulous accuracy, been brought up to the advanced level of anatomical science in the present day. The general anatomy is again from the able pen of Dr Sharpey, and maintains its position as the best treatise on the subject in the English language. The descriptive anatomy, including instructive sections on development, has undergone the supervision of Professors Allen Thomson and Cleland. New figures, mostly on a larger scale, have been added, or substituted for the previous ones. In its present shape this work is one of the most complete and accurate anatomical text-books in existence, and will serve to mark the actual condition of the science at the date of its publication. It will be regarded by students and teachers as a standard work for acquiring or imparting a knowledge of anatomy.

Part Third.

MEETINGS OF SOCIETIES.

PROCEEDINGS OF THE EDINBURGH OBSTETRICAL SOCIETY.

SESSION XXVI.—MEETING VI.

13*th February* 1867.—SIR JAMES YOUNG SIMPSON, Bart., *President*, in the Chair.

I. CASES OF REMOVAL OF INTRA-UTERINE POLYPI. BY SIR J. Y. SIMPSON, BART.

Sir James showed the Society four intra-uterine fibroid polypi, which he had removed from three different patients within the last eight or nine days.

The first of these cases was an unmarried lady from England, who had suffered from hæmorrhage for twelve years, and sometimes had been so greatly reduced by the discharge as to be invalided and bedridden for weeks. She was very white and blanched. The os uteri was opened up with sponge tents, a polypus found in the uterine cavity, and its neck divided without difficulty with a small polyptome. The removal of the polypus from the uterine cavity after its detachment was by no means easy, as it escaped readily from the vulselli and polypus forceps that were used to catch and extract it.

In the second case, an unmarried lady from Glasgow, the hæmorrhage had only lasted fourteen months, but had been sometimes very excessive. The same means of diagnosis and treatment were used, and the same difficulty met with in attempting to remove the free polypus, which was of the size of a large filbert, through the small os. Ultimately the os was divided to allow of the possibility of extraction.

The third case occurred in a married patient in Inverness-shire, who had had great intercurrent menorrhagia for nearly eighteen months. Sir J. Y. Simpson visited her at her own home in Inverness-shire, and thought he detected the presence of polypi with the uterine sound; but as a long diagnosis with dilatation was required, she was removed to Edinburgh when her strength was sufficiently recovered. In this instance, after the os uteri was opened, first one semi-pediculated polypus was felt and removed, and then the presence of a second similar one was discovered high up in the uterine cavity, and detached by partially cutting through its pedicle, and then dragging it down with vulselli. In regard to these cases of intra-uterine polypi, Sir J. Y. S. added the following inferences:

1*st*, In such cases of intra-uterine polypi, the presence of the disease may be guessed by the increased size of the uterus, and particularly its cervix, which sometimes expands above as it does in a threatened abortion, and by a persistent and sometimes a great menorrhagia. Occasionally it can be felt by the uterine sound, used like a metallic finger, but usually not with any great certainty. The only certain mode of diagnosis consists in dilating the cervix uteri so as to feel the interior of the uterine cavity with the finger, and touch the polypoid body. He adverted to a case which he saw with Dr Wood, many years ago, of a patient who died of hæmorrhage from intra-uterine polypi. The autopsy in this case first suggested the idea of reaching and diagnosing the intra-uterine tumour by opening the cervix with some dilating body.

2*d*, For this purpose, sponge-tents were suggested and used by him; and more lately Dr Sloan proposed tangle-tents. He believes that tangle-tents are to be preferred when a slight dilatation, as for dysmenorrhœa, was required; but for a greater degree of dilatation, when we require to introduce the finger and remove a polypus, sponge-tents are preferable.

3d, In making the diagnosis, it is usually necessary to push down the fundus uteri with one hand pressed against the abdominal parietes, and by the forefinger of the other hand search the interior of the uterus.

4th, In removing intra-uterine polypi, he had sometimes found torsion an easy plan, but the forceps must be strong in blade and joint to give the necessary power. Torsion succeeded best when the pedicle was small. When the pedicle was large and thicker, and when the tumour was only of a polypoid character, division, with the tenotomy knife, of the tissue covering of the thick neck, or even of the body of the polypus, greatly facilitated its extraction. A small polyptome, as in two of these cases, was sometimes the simplest method of dividing the pedicle; and perhaps a loop of a wire ecraseur might be passed over it to separate it, but usually not without much difficulty.

5th, But our armamentarium was chiefly deficient at the present time in proper means to seize and extract the polypus after it was separated. Fibroid polypi are elastic, and readily jump out of the catch of any form of polypus forceps. Sir J. Y. Simpson had tried an improved form of lithotrite to lay hold of them, but it had also failed. Vulselli with very long teeth were difficult to open in the narrow uterine cavity, and the teeth were apt to catch hold of the uterine walls. The best instrument which he had yet found was a vulsellum provided in each blade with three small and short teeth; but he suggested to the Society whether some of the members could not invent a more fit and useful instrument for the purpose.

Dr Main asked whether an instrument, somewhat like the old cork extractor, consisting of three prongs with a slide, might not be efficient for extracting polypi.

Dr Carmichael suggested the use of flat blades with prongs, so arranged as to project through the the blades, and thus to lay hold of the polypus. Sir James Simpson, however, said, that he generally found forceps with long prongs merely tear the polypus without holding it.

Dr Keiller remarked that it was often no easy matter to determine with any degree of exactness the position and condition of intra-uterine polypi: the common diagnostic signs of recurring menorrhagia, together with uterine enlargement—and especially a marked change in the bulk and shape of the cervix—were valuable indications in many cases, but until the polypus was sufficiently developed so as to be within reach of actual detection, it was difficult to arrive at a correct opinion regarding the probable progress and issue of the case. There was usually little difficulty in diagnosing or removing uterine polypi after they have become pediculated and partially or entirely extruded from the uterine cavity. When they became expelled through the os into the vagina, the discovery of the state of matters is generally very easily accomplished by simple tactile examinations, and if pediculated (which they often are at this stage), their removal was usually easy, and almost without exception satisfactory. The difference between the diagnosing and the treating of extra-uterine and intra-uterine cases was great, the former being very simple compared to the latter. This difference has often led to misunderstanding on the part of the patient and her medical adviser, who may have examined and formed his opinion of the case in its earlier stages, before its true nature could be correctly ascertained. It not unfrequently happens that, after the hæmorrhage and other indications of the presence of a fibroid polypus within the uterus has lasted for a long period, the patient or her friends seek the advice of another practitioner,—it may be long after the ordinary attendant has had an opportunity of making an examination;—and just because the polypus is now projecting from the os uteri, or perhaps lying in the upper part of the vagina, no difficulty is found by the new doctor, who pronounces his judgment on the case without hesitation, and by a speedy and simple operation effects a cure, much to the delight of the patient, whose gratitude and that of her circle does not always extend beyond the *lucky expert* who "found out and removed her long-continued enemy, after all the skill and all the treatment previously had recourse to had failed!" Dr Keiller mentioned the particulars of a case in point which recently occurred under his immediate observation, and made some remarks on what he considered to be due to the ordinary attendant under such circumstances. It was seldom necessary to use the speculum, as tactile was generally preferable to ocular examination, which, to the inexperienced in such cases, sometimes led to the idea that the presenting tumour was of a malignant nature. He (Dr K.) had met with cases where caustics had been freely applied to the extremity of the simple fibroid polypus.

Sir James Simpson stated that he had seen a case from Germany, which the doctor there had treated with caustic, with the use of the speculum, thinking it some disease of the cervix uteri. When he (Sir James) saw the patient for the first time, he happened to have a polyptome with him, and, without telling the patient, divided the pedicle with the instrument, and desiring her to cough, the polypus came away.

II. OBSERVATIONS ON A FEMALE DWARF. BY DR R. PEEL RITCHIE.

In the public exhibition of human beings, there is much room for deception on the part of exhibitors, and more especially is this the case when the deception is increased by statements as to the birthplace of the prodigy. In the case of the so-called Aztec children, exhibited here some years ago, the unsatisfactory statements as to their origin succeeded in producing a strong feeling of disbelief in the minds of many as to the truth of the story told. In the case of a dwarf like Tom Thumb, there is not room for so much vagueness, further than doubts are expressed as to the age of such a person.

There has lately been exhibited in this city a female dwarf, M. A. Y., who presented some interesting peculiarities. She is stated to be 17½ years old, to measure 35 inches in height, and to weigh 9 stones. The growth of this individual, so far as the bony skeleton is considered, seems to have been arrested, whilst the tendency to obesity or lateral extension is extraordinary.

In infancy she is stated to have presented similar peculiarities, and it is said she is one of twelve children, several of whom are dead, as well as their mother. The catamenia are reported to have occurred about two and a half years ago, and since have, with the exception of occasional undue frequency, appeared regularly. The general health is stated to have been good.

She is said to have been born with abundant hair, and now has a good head of dark-coloured hair. The constitutional character of this person is rachitic, as shown further by the broad hand, the large joints, and the waddling gait; the latter is apparently due to the hip-joints being insufficiently strong to support the great weight of the body.

The cranium does not seem large, but, on the contrary, looks small, in comparison to size of shoulders, the sutures closed early, and it presents no peculiarities of conformation. The features are irregular, the irides grey.

In the mental condition, there seems to be an arrest of development. The girl, though stated to be of the age mentioned, is still a child in mental progress. This may in some degree be due to want of careful education, but the manner is childish. She plays with toys, and amuses herself with pictures. The inability to fix attention is noticeable, and the deficient education may possibly be a consequence of it. There is much obstinacy at times. The inelegant gait may also be, in some measure, due to the deficient co-ordinating power of the imbecile; but the mental condition seems to me to be that of weakness, or retarded development, and deficient training, rather than true imbecility, she being, as the poet has not inaptly described the state,—

> "Pleased with a rattle, tickled with a straw."

Supposing that, in a case of such uncertainty as to the age, some light might be thrown upon it by the state of the teeth, I have, through the kindness of my friend, Mr D. Hepburn, dental licentiate of the R.C.S. Eng., obtained a cast of the teeth, from which you will see, if we regard the teeth alone, we may be still left in uncertainty as to the accuracy of the showman's statement. I have also, through Mr Hepburn, an opportunity to show a cast of the mouth of a child of 7 years old, and you may observe how greatly the development of the teeth of this dwarf resembles that of the healthy child. Judging from the state of the teeth themselves, one would say that the age did not exceed 8 years; but when the width and arch of the mouth, and the want of bony growth in other parts of the skeleton, are considered, there is the less difficulty in believing that the female really is the age she is stated to be. Instances of retarded development of the permanent teeth are by no means uncommon; but it is not usual to find, at the age of 17½ years, that only four of the permanent teeth in the lower jaw, and two in the upper, have shown themselves. Thus Mr Tomes, in his work on Dental Surgery (p. 248), observes—"It is by no means uncommon to find that certain members of the permanent do not appear at the usual time, and, even after the lapse of some few years, are still absent from the usual position, and it is not perhaps until long after their presence has been called in question that they penetrate the gum." He gives instances of a right central incisor of the upper jaw appearing at the age of 13, of an upper canine piercing the gum at the age of 32, and in another case of a similar tooth cutting the gum after the age of 40. He also states that "many cases have occurred in which the teeth have been cut at a very advanced age." I find that at the December meeting, in 1861, of the Odontological Society, "Mr Cattlin introduced a case of retarded dentition in a youth aged 14 years and 9 months, in whom the whole of the temporary teeth in the lower jaw were retained, while the second dentition in the upper jaw proceeded in the normal manner;" and other instances might be quoted.

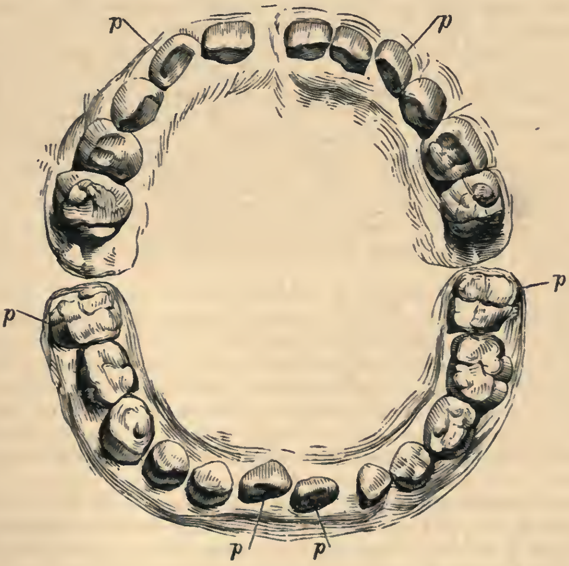

Fig. of Jaws, natural size. *p*. The Permanent Teeth.

In directing your attention to this model (see woodcut) of the dwarf's mouth, I may remark that the circle of the jaw would show it to be that of an adult. The arch is even large for a fully-developed young female. It may be observed in the model of the upper jaw that, with the exception of the one (right) lateral incisor, the whole of the temporary teeth still retain their position in the jaw, whilst on the left side the first permanent lateral incisor has made its appearance without a corresponding temporary tooth having been shed. On the right side the permanent lateral has come through in the usual manner. It will also be seen that the laterals have appeared before the centrals, instead of following them, as is the usual course.

In the model of the lower jaw, we find that the teeth seem to have followed the natural course, as regards the order of eruption. The six-year old molars are fully developed, and the central incisors have made their appearance through the gums, their chief peculiarity being that they have assumed a short stunted appearance, as if from retarded development. With the exception of these four, the other teeth are all milk-teeth, and are in their natural order.

On examining the model now before you from the healthy female child of natural growth, it may be seen that the upper jaw shows an advance in the permanent molars, which are projected through the gum, none of the milk-teeth being yet shed. In the lower jaw the development is similar to that of the dwarf, namely, that the permanent molars have appeared, and that the temporary central incisors have been shed, and their place occupied by the permanent teeth.

We are all familiar with the delay which occurs in rachitic infants, in the eruption of the milk-teeth. Retarded appearance of the permanent teeth presents for consideration the state of the teeth themselves, and the nature of the eruptive process. With regard to the first, Mr Tomes (p. 250) observes—"There is a want of decisive evidence in support of the opinion, that the actual development of the teeth is delayed much beyond the usual period, although the numerous cases of late eruption would, at first sight, favour the supposition;" and with regard to the other, the causes of the eruption of the tooth appear to be, *first*, the progressive lengthening of the root; and, *second*, as Hunter (Natural History of the Human Teeth) has suggested, not to the tooth growing larger, "but to the socket filling up behind it, and thereby continuing to push it out by slow degrees." The most common cause of retarded cutting of a permanent tooth is the mechanical one of the pressure of persistent milk-teeth. In the case under consideration, it must be apparent that this cannot be the cause, when we regard the roomy jaw, and the spaces between the teeth, and the conclusion I come to is, that the cause must be in the constitutional condition, and that whilst the teeth have not developed to sufficient length to aid in their own liberation from their cysts, the same rachitic condition has as yet prevented their eruption being aided by the deposition of bone in the sockets.

In concluding these imperfect observations, I would ask, although it is scarcely within the scope of this Society, if there be not some law preventing the exhibition of such unfortunate characters? I do not think a parent, as in the present case the exhibitor calls himself, should be permitted to reap a large harvest, and live in inactivity through the exhibition of his child's infirmity, whilst keeping it in a state of ignorance. This is not a state of matters at all creditable to us as a nation; and in this light, as well as an instance of arrested development, I have felt desirous to bring this individual's case before the Society.

Sir James Simpson said that Mr Shaw spoke of rickets in the child, either intra-uterine or after birth, affecting the primary as well as the second teeth.

Dr Main said that he had seen in his own practice a dwarf, which died of convulsions at the age of seven, but looked only like two years.

Some conversation followed, generally disapproving of such exhibitions.

III. TWO CASES OF FATAL HÆMORRHAGE FROM THE GUMS AFTER SCARIFICATION. BY JAMES YOUNG, M.D.

At the last meeting of this Society, a paper was read by Dr Linton, which elicited some discussion on the general subject of infantile hæmorrhage. With the view of following up the subject, I have obtained some notes of two cases of fatal hæmorrhage after scarification of the gums, which occurred in my father's practice some years ago. Both cases are fresh in my own mind, as I happened to see them. Sir James Simpson or Dr Alexander Simpson saw one of them along with my father.

CASE I.—A child living in the Crosscauseway, aged 20 months, presented no evidence of disease in any respect beyond the ordinary irritation from teething. The teeth already cut had each produced some disturbance, but without requiring scarification. With the first eye-tooth, some febrile symptoms were manifested. My father was sent for, and without the least hesitation he advised scarification of the gum, which was forthwith done. A sudden and rather profuse welling of florid blood immediately appeared, and not at once ceasing, as is usually the case, pressure with thumb and forefinger was applied. The hæmorrhage seemed to be allayed, but on withdrawal of the finger, it continued to ooze up and filled the mouth. Nitrate of silver was applied steadily and pressure again, and yet the blood continued to flow. My father became somewhat anxious, and sent for me. We applied lint, moistened in a solution of the perchloride of iron and glycerine, with as much pressure as possible, and yet the hæmorrhage continued. The last alternative was the hot wire, which was applied the same evening, after Sir James Simpson had seen the case. Next morning, although pressure had been kept up more or less during all the night, and the child fed on beef-tea and wine, with use of iron internally, the hæmorrhage continued. After twenty-four hours' incessant oozing, the child became pale and exsanguine, and yet, extraordinary to say, the child lived for three or four days. Nothing had the least effect in checking the flow of blood, except the pressure, and only so during its application. There was no hæmorrhagic diathesis; the child had never lost blood before, was perfectly healthy, and of healthy parents. The blood was florid. The question here arises, how deep should the scarificator be pressed into the gum, or should the gums be scarified at all, until the teeth are shining through? Scarification may be necessary when the tooth is not close at hand, and the same tooth may require to be cut repeatedly. I have known a case of this kind where the child was attacked with convulsions, which ceased from the incision of the gum; the wound healed up, and the convulsions returned not less than six times from the same tooth, and ceased every time after cutting the gums. Then, again, suppose the scarificator be placed deeply in the gum, is it possible that the small offsets or minute twigs of the dental or alveolar branches of the internal maxillary artery could bleed to such an alarming extent without some other cause? What that was, I leave my seniors to divine.

The second case occurred at Holyrood; the only difference being that in this case it was the first molar tooth of the upper jaw, while in the first case it was one of the eye-teeth of the lower jaw. The child here was about 18 months old, and had neither in itself nor mother presented any symptom of a hæmorrhagic tendency. The same result followed the scarification, the same treatment was pursued, and, I regret to say, it had the same painful issue. The child survived one week.

Sir James Simpson thought such cases very uncommon. Mr Robertson, he said, had been in the habit of using an instrument to produce steady pressure on the gum in cases of hæmorrhage. In cases of umbilical hæmorrhage, Dr Churchill had proposed the use of sulphate-of-lime powder. He spoke of a case of umbilical hæmorrhage he had seen with Dr Moir—a child—where the umbilicus was transfixed with needle and suture. The hæmorrhage ceased for a while, but returned. The perchloride of iron was applied, and the child recovered.

Dr Wilson mentioned one case of fatal hæmorrhage from the gum, accompanied by tubercular peritonitis.

Dr Charles Bell suggested the tying of the external carotid, in cases where there was no hæmorrhagic diathesis.

Mr Pridie spoke of the removal of three teeth by a dentist, after which hæmorrhage occurred to the extent of at least sixty ounces. Perchloride of iron was applied with compression for fully four hours, and quite arrested the bleeding.

Dr Keiller said that the bloodvessels were usually in a diseased condition, which was often accompanied with jaundice. He thought nothing did good in some cases of umbilical hæmorrhage but transfixing with needle and ligature.

Dr Burn said he had succeeded with the hot wire in checking hæmorrhage.

Dr Patison said that he had used the perchloride of iron successfully. He knew of one case where a patient died from hæmorrhage eight days after a tooth extraction.

Dr Charles Bell thought the hot wire the best application in hæmorrhage from the gum.

Drs Gordon and Ritchie suggested the use of the matico leaf.

Dr Keiller recommended powered matico, with alum, to be snuffed up the nostrils, in cases of epistaxis.

Dr Ritchie urged the use of citric acid in certain cases, where the blood was diseased.

SESSION XXVI.—MEETING VII.

27*th February* 1867.—Sir JAMES Y. SIMPSON, Bart., *President*, in the Chair.

Mr Miller, 11 Rankeillor Street, was admitted as an Ordinary Fellow of the Society.

Dr Thomson, of Wrenbury, Cheshire, was unanimously admitted as a Corresponding Fellow of the Society.

I. A Case of Intra-Uterine Amputation was shown by Dr Bryce. The subject of it was a girl twelve years of age. There were two cicatrices on the end of the bones of the fore-arm, with a small projection of rudimentary finger. The amputation had taken place 2½ inches below the elbow, and the end of both ulna and radius were distinctly felt.

Dr Keiller said that he had seen five such cases in Dundee and neighbourhood. In one case he saw with the late Dr Munro, there was amputation of both lower limbs below the knees, the left arm was similar to Dr Bryce's case, and the fingers of the right hand were webbed. This child was otherwise well-formed, and lived for some time. He (Dr K.) had also examined a young man who had lost his left upper extremity together with the scapula by intra-uterine amputation, the cicatrix being very large and distinct.

Sir James Simpson had seen seven cases in Edinburgh of intra-uterine amputation, all of left arm, with rudimentary fingers existing, and he remarked that it was almost universally the left arm. He calculated, some years ago, that if this deformity was as common elsewhere as in Edinburgh—as was probably the fact—some forty thousand or fifty thousand such single-handed individuals probably existed in the world. The fact of the embryo generally lying on the left side might account for the left-arm amputations. He (Sir J.) related the case of a girl who had neither arms nor legs, residing in the Highlands; also a student, and lately an A.M. of this university, without arms, who wrote his exercises with his feet.

Dr Burn had met with two cases of intra-uterine amputation of left arm.

II. CONGENITAL MALFORMATION OF RIGHT SIDE. BY DR BRYCE.

A. M. was the eighth, the youngest, and the only child of Mrs T. that survived to adult age,—all her brothers and sisters having been still-born, or having died in early infancy. At birth the deformity was thought to be confined to the foot; but after she arrived at adult age, she herself observed that the whole of the right side was more or less affected. She felt that side weaker, and noticed that there was a perceptible difference in the length of the arms. A. M. died about seven months ago, aged 25, of tubercular ulceration of the bowels. The following measurements were taken after death:—

	Right Side. Inches.	Left Side. Inches.	Difference. Inches.
Olecranon to end of ulna, . . .	9½	9¾	¼
Lower end of ulna to point of little finger, .	5¾	6	¼
			½
Anterior superior spine of ileum to internal condyle of femur, ...	16½	17	½
Internal condyle of femur to lower border of internal maleolus, ...	12¾	14½	1¾
Lower border of internal maleolus to lower border of external maleolus, across plantar aspect, .	5¾	6½	¾
			2½
Centre of patella to great toe, . . .	17	20	3

Thus the right ulna is one quarter of an inch shorter than the left, and there is the same difference between the hands, making the right superior extremity half an inch shorter than the left.

The right thigh is also half an inch shorter than the left. A difference of an inch and three-quarters between the tibia of the right and that of the left leg, and of three-quarters of an inch round the heel, makes an absolute difference in the leg of two and a half inches.

My friend, Dr Omon, now of Wishaw, kindly took a cast of the foot for me in plaster of Paris; but before destroying the mould, I asked him to try one with parafin. I had never heard of this substance being used for this purpose; but the result was so decided that we destroyed the plaster-of-Paris cast, and I now show the Society a very perfect representation of the foot in parafin. The foot was shorter, little more than half the breadth of the left, and ended in two toes, with a rudimentary one between them. These were so joined together by a very thick web, extending to their extremities, that it was very difficult to distinguish them, except the great toe, which was quite distinct, and harmonized in size with the rest of the foot. The other seems to be the one next the little toe. We have thus the great toe undoubted—the fourth not so readily recognized—a rudimentary representative of the second and third, while the little toe is entirely absent. The appearance of the toes is also perfectly represented in the cast, which also shows by this protuberance some malformation of the tarsus.

From the above table of measurements, we see that the deformity increases from above downwards, rapidly increases from the knee, and attains its climax in the foot. Why is this? The explanation seems to be, that whatever was the cause of this deformity, it began to operate at a time when the development of the upper extremities was nearly completed, but that of the lower still in a backward condition. I may state that there was no deformity in the case of her mother, and the patient herself was the mother of three children all perfectly formed. My intention was to have got a careful dissection of the foot, so as to have seen what bones were deficient, and what rudimentary; but although I was allowed to open the abdomen, I was not permitted, on account of an ignorant prejudice, to touch the foot. I am sorry the case is in consequence imperfect, but such as it is I thought it might interest the Society.

Tubercular ulceration of the bowel, of which I have said the patient died, is not of itself of any interest, yet there is one thing about this case I should like to mention. For many weeks before her death the flatulent distention of the upper part of the bowels and stomach was extreme even for this disease, and was the only cause of complaint. She often said, "If I could get quit of that wind I would be all right." Along with this there was a constant and most distressing desire for stool. Every remedy was tried and retried, but utterly failed in relieving either of these symptoms.

Mr Malins opened the abdomen for me, and in tracing the extent of the ulceration he came, in the neighbourhood of the ileo-cœcal valve, upon a portion of the bowel so contracted as not to admit the point of the little finger. This had been caused by the cicatrizing of one of the ulcers, which is I understand rather a rare morbid appearance in these cases. I am sorry that my case is here again imperfect, in consequence of its being under observation only during the last two months of her life. I was asked to see her first in the beginning of June, when she informed me she had suffered from diarrhœa very often for nearly two years, that she became pregnant early in August 1865, was confined early in May 1866, and that after this the diarrhœa became worse and constant. Although I have no note of the state of the diarrhœa during her pregnancy, yet, from its becoming rapidly fatal after her confinement, may we not assume that the pregnant condition arrested the tubercular disease in the bowel, as often happens in the lung, and that it was then that the ulcer cicatrized. The site of the contraction is seen in this preparation.

III. CASE OF PLACENTA PRÆVIA. BY DR VACHER. COMMUNICATED BY DR GORDON.

About half-past five P.M., Sunday, 14th October 1866, I was summoned to a case of placenta prævia by Mr John Gordon (a pupil of this institution). The account he gave was that the patient was undoubtedly in labour, was flooding profusely and that he was unable to make out the presentation. I accompanied him to the patient's house and found her taking pains very rapidly, rather pale, and with a pulse about 80, but weak. An examination per vaginam satisfied me as to its being a case of unavoidable hæmorrhage. The passages were soft and dilatable, the os about the size of half-a-crown but readily dilated, the placenta protruding through like a wedge, the passages at each pain being suffused with blood. I requested Mr Gordon to go to the Maternity Hospital and return as soon as possible with Dr Yarrow in order that I might have my diagnosis confirmed. I requested Dr Yarrow to give chloroform, and, as soon as the patient was "over," proceded to turn (it would now be about half-an-hour from when I was first called). I slowly introduced my right hand; but, in reaching the os, found my fingers so cramped and confined in their movements as to render any systematic dividing the placenta from the uterus quite impracticable. I therefore, perforce, scratched through the placenta, and coming upon the child (whose position was normal), turned, and delivered, drawing it through its own afterbirth, which was also thrown off within five minutes. The child, which had all the appearance of being premature (probably eight months), was asphyxiated, but fortunately soon brought round by the aid of hot and cold douches and such usual remedies. Before leaving I got the uterus fairly contracted, and administered a dose of tincture of ergot. The mother and child are, I believe, both alive and well at the time I write. There were present at the operation, Dr Yarrow, Mr John Gordon, and a pupil from the Royal Public Dispensary, whose name I am not acquainted with.

Sir James Y. Simpson stated that a medical man in the country had lately met with a case of placenta prævia, where the hæmorrhage was so great that the patient sank in an hour. A second patient in labour was going on well (the same day), when a gossip unexpectedly intimated to her that such a one had just died from flooding. The fright caused a rush of blood, and this patient also died shortly afterwards. He remarked that, in cases of placenta prævia, many children were brought through the placenta when it could not be pushed aside.

Dr Keiller spoke of the danger of passing the hand through the placenta.

Dr Thomson, of Wrenbury, communicated some interesting facts to the Society.

IV. APPLICATION OF SOLID NITRATE OF SILVER, ETC., TO THE INTERIOR OF THE UTERUS IN MENORRHAGIA.

Sir James Y. Simpson spoke of the success which sometimes attended the introduction of a solid piece of nitrate of silver into the uterine cavity, in cases of obstinate menorrhagia that were independent of the presence of polypi or of fibroid tumours in the walls of the uterus. For this purpose he had generally used a short piece of solid nitrate of silver, made half the thickness of the usual stick, and introduced and lodged in the uterine cavity by a hollowed or tubular instrument, of the size of the common uterine sound. In some cases the hæmorrhage was arrested by this treatment, as it is in internal rectal hæmorrhoids by the application of slight caustics. The powder of the persulphate of iron may be lodged in the uterus for the same purpose. He considered solid substances and powders as much more safe applications to the interior of the uterus than any forms of fluid injections. The occasional danger arising from the latter was not, he believed, so much from their passing along the Fallopian tubes into the peritoneal cavity, as from their sometimes over-distending the uterus, and fissuring and tearing through its mucous surface, thus getting fatal access to the circulation.

Dr Burn mentioned one case where the above treatment produced a curative result, but only for the time being.

Dr Cochrane spoke in favour of the injection of the tinct. mur. ferri.

Sir James Simpson suggested caution in the use of fluid injections into the uterus, as some doctors had used such, and the patient died before they left the house. He (Sir J.) stated that the powder of the persulphate of iron might be introduced into the uterus with safety.

Dr Stephenson had pursued the treatment of injection of Condy's fluid, and the bichlor. of iron in one case, without any harm resulting. But in his case the os was open and patulous.

V. *Dr Menzies* mentioned some facts relative to two cases of Spina bifida which he had recently seen.

Sir James Simpson considered primary union the secret of success in cases of spina bifida.

Part Fourth.

PERISCOPE.

MATERIA MEDICA.

ON THE TREATMENT OF THE FEVERS OF ALGERIA WITH HYPODERMIC INJECTIONS OF SULPHATE OF QUININE. BY M. JULES ARNOULD.

In an elaborate investigation, M. Jules Arnould discusses the hypodermic employment of sulphate of quinia in Algerian fevers of a malarious and periodic type. He strongly advises this method of using quinia, and refers to a large and successful experience in support of his recommendation. At the same time, this plan should not supersede the administration by the stomach, whenever this can be done without inconvenience to the patient. In the majority of cases, however, the functions of the stomach are early impaired, and it either rejects the medicine by vomiting, or absorbs it imperfectly and slowly. Often, also, it is necessary to give emetics, and thus the administration of quinia is interfered with. M. Arnould mentions, among the chief additional advantages of hypodermic injection, the relatively smaller dose required, which seems to be about two-thirds, and this is of importance where large quantities of an expensive drug are used; the greater rapidity of effect; the certainty of absorption; the avoidance of many unpleasant draughts; and the slighter quinism (tinitus aurium, deafness, and cephalalgia) which is caused. The main disadvantage is the action on the subcutaneous cellular tissue, which may, however, be reduced to an insignificant minimum by careful preparation of the solution. The nearer this approaches to a neutral fluid, free of suspended particles, and without coagulating action on albumen, the less will be the irritation. Neutral sulphate of quinia dissolved in twelve parts of water, by the addition of a few drops of sulphuric acid, is the solution which seems to possess the fewest disadvantages.—*Bull. Gén. de Thérap.*

LOCAL ANÆSTHESIA.

THE introduction by Richardson of ether-spray as a local anæsthetic is likely to suggest many directions in which its application may be extended. Dr Wallan has successfully employed it to reduce strangulated inguinal hernia, which had previously resisted the most careful and persevering taxis.—*L'Abeille Médicale.* Dr Lubelski publishes a case in which he applied pulverized ether to the spinal column of a child afflicted with obstinate chorea, and after two aspersions of about four minutes each, the non-coördinated movements disappeared, and the patient soon recovered the power of normal movement.—*Gaz. Hebdom.*

ON THE CURE OF OVARIAN CYSTS WITHOUT OPERATION. BY DR COURTY.

THE author introduces his subject by carefully stating that no one could be less easily convinced than himself of the efficiency of any method of treating ovarian dropsy except ovariotomy. Two cases were, however, cured by him without operation. He classifies the remedies employed into,—1. Preparations of gold, especially the oxide, in doses of $\frac{3}{100}$th to $\frac{7}{100}$th of a grain; 2. Analeptics and tonics, as Vichy water, iron, quinia, etc.; 3. Abdominal friction, with iodides of lead and of potassium; 4. Diuretics, also applied by friction, chiefly squill, digitalis, and nitre; and, 5. Graduated compression of the abdomen by elastic bandages. The gold was prescribed in the pleasant form of tablet prepared with chocolate, and the frictions were made over all the body with soft woollen cloths soaked in tinctures of squill and digitalis, by which, it is worthy of note, marked diuresis was caused. The first case was of an unmarried woman, forty-three years old, with a large, probably unilocular, cyst of the right side, which had existed for four years. Under the above treatment, the tumour disappeared in a month, and there were no symptoms of a recurrence of the disease three years afterwards. In the second case, a young girl of twelve, with a large multilocular cyst, was treated on the same principle; improvement occurred in fifteen days, and a cure, which promises to remain permanent, was produced in six months. Dr Courty mentions having seen this disease in a still younger patient under the charge of Professor Simpson of Edinburgh.—*Revue de Thérap. Méd.-Chir.*

SURGERY.

SUB-PERIOSTEAL RESECTIONS.

THE subject of sub-periosteal gouging, or scraping of bones and joints, still seems to excite much attention in France; at the meeting of Academy of Sciences, of December 24th, an interesting communication on the subject was made by M. Sedillot. While we must allow a little for the enthusiasm of a partizan, we must agree with M. Sedillot, that he has made up a good case for the powers of the periosteum, with its subjacent bone, in the restoration of removed bones or portions of bones. The experience of English surgeons is as yet, at least, quite opposed to the belief in the value of any attempt to save the periosteum in resections of joints, our difficulty being, in general, rather from the formation of too much new bone after a resection than too little. The following case, by M. Laroyenne of Lyons, quoted as a favourable case of sub-periosteal resection of the elbow-joint, would be considered by no means a striking success here, simply from there being too much new bone, and, consequently, a limitation of flexion.

The elbow-joint of a man, æt. thirty-nine, had been diseased for twenty-six years. On admission, it was stiff, very painful, and showed several fistulous openings. Using the fistulous openings for his incisions, M. Laroyenne removed the bones of the articulation as far as possible without the periosteum—the tendon of the triceps was not cut, but left attached to the periosteum of the olecranon. The bones were removed apparently to sufficient extent, piecemeal. The arm was fixed in a semi-flexed position, in a starch bandage, open behind and to the inside. Eleven months after, when the patient was shown to the Society, the olecranon was found to be reproduced, the end of the humerus was filled out, the movements of flexion and extension were executed, extension being almost complete, and the flexion passing the right angle by some degrees, no lateral movement, the hinge being a true ginglymoid one, and *no* movement of rotation in the forearm.—*Gazette des Hôpitaux*, January 10, 1867.

SEDILLOT ON RESECTIONS.

M. Sedillot has written a letter to the Imperial Society of Surgery, on the regeneration of bone. It is too long for quotation entire, but of great interest. He contrasts the two principles of sub-periosteal resection in the following terms:—

One, to which Larghi has given the general name of sub-periosteal resection, is founded on the idea, that the periosteum detached and isolated in the condition of a sheath or a flap is able to renew or reproduce the subjacent bone from which it is stripped or raised.

The other principle, which I have called sub-periosteal gouging or scraping, has for its principle, that it is the periosteum, *only when attached* to the bone, that is able to renew it; and that, in consequence, the bone beneath the periosteum should be husbanded and preserved with the very greatest care.

After invoking the aid of the Baconian method of research, M. Sedillot quotes numerous series of experiments which, in his opinion, are sufficient to prove that the periosteal flaps, if completely isolated from the subjacent bone, are unable to reproduce it; "and that the so-called method of sub-periosteal resection is founded on a deplorable illusion:" and gives his opinion that the chief use of preserving the periosteum at all, is to supply a mould for the bony matter which is left in his method of periosteal gouging, and which really reproduces the bone.—*Gazette des Hôpitaux*, January 19, 1867.

ON THE DANGERS OF CHLOROFORM, HOWEVER PURE AND CAREFULLY GIVEN, AND ON THE POSSIBILITY OF REPLACING IT BY ETHER.

This subject has lately been exciting much attention in America and on the Continent, and the profession in England has also been directed to it in the work of Mr Ellis, lately noticed in these pages. The following observations are extracted from a memoir lately presented to the Academy of Sciences by Dr Petrequin, late Surgeon-in-chief to the Hôtel-Dieu at Lyons, etc.:—

Chloroform and rectified ether have been, and still are, the subjects of contradictory opinions. In a former notice laid before the Academy in Dec. 1865, I have shown that etherization has made a remarkable advance (in which the Lyons school has had a large share), and that, thanks to the simplified apparatus, the improvement in administration, and to the purity of the ether, it has now become an excellent method, efficacious, expeditious, and has remained free from all accident. This is not the case with chloroform: instruments once vaunted are now given up; no method of administration completely secures against unfortunate results; the doses and the selection of the chloroform are no sufficient guarantee. In a word, chloroform has not made notable progress, as ether has; it gives scarcely more security to the patient or the operator than at first; and every year it sacrifices a certain number of victims. In consequence, I believed myself authorised to conclude that rectified ether ought to be substituted for chloroform.

This conclusion, which seemed to me based on incontestable facts, has been challenged by a distinguished surgeon. He has affirmed that "pure chloroform, well administered, never kills." This authoritative sentence, already promulgated by him in 1852, before the Academy of Sciences, is none the truer from being repeated in 1866. It may even be said that it is less tenable now than then, and it appears to us to become more and more liable to very serious objections. In maintaining that chloroform, well administered, never kills, has M. Sedillot considered the force of this declaration? For his own sake I should prefer to doubt it; because it really is to bring a formal accusation against all those who have had the terrible misfortune to lose a patient by this agency. Unfortunately, after the following words, the reader can hesitate no more than we can; "When experience shows exemption from mortality in the practice of the professed partisans of chloroform, whilst that of inexperienced practitioners furnishes the most frequent examples, we are compelled to see in this result something beyond mere chance." Thus the Strasburg professor takes on himself the office of public accuser; but is it just to tax with ignorance and unskilfulness all our brethren who have met with misfortune? So long as these untoward accidents had only recurred in hands of dentists and "officiers de santé," the supporters of chloroform had the game to themselves, and they could, if they chose, maintain that the pretended indispensable precautions, which alone could avert death, had been neglected. But now the scene is altogether changed. These accusations must now be brought against the very princes of science, against whom, doubtless, my honourable adversary has no intention of bringing them. For myself, who am personally disinterested in the debate, having long since abandoned chloroform, I denounce with all my strength this unfortunate line of argument, for a double reason, 1. that it is not founded on truth, 2. that it throws unjustly an immense responsibility on all unfortunate operators.

A list of surgeons who have lost cases then follows, and a brief account of some fourteen fatal cases within the last few years, and a strong opinion against chloroform and for ether.—*Gazette des Hopitaux*, 5th February 1867.

[This, we need hardly add, is contrary to Edinburgh experience, where chloroform maintains its superiority unquestioned.]

INJURY OF HEAD WITHOUT APHASIA.

THE following case, communicated to the Society of Practical Medicine by Dr Piedanna, is of great interest, from the gravity of the accident, and the absence of any symptoms of aphasia. In many respects, it closely resembles that of the boy exhibited to the Medico-Chirurgical Society of Edinburgh about a year ago.

C. H., æt. nineteen, received a violent kick on the forehead from his horse, while he was endeavouring to free the reins which had got caught under the horse's tail, without leaving the box. He was thrown senseless on the road. He was there found by Dr Piedanna, bathed in blood, with a wound on the forehead, exposing an immense fracture of the frontal bone, between the superciliary ridge and the frontal eminence, and which extended towards the temporal fossa. From this, cerebral matter was oozing in some quantity. The pulse was very slow and feeble. He was taken home, and the wound being carefully cleaned, Dr P. endeavoured to raise the fragments of broken bone. The first attempt induced severe and general convulsive movements. The second succeeded in removing a large splinter of bone. After this, at least a teaspoonful of blood and brains escaped. A compress to the wound, and cold applications to the head were ordered, along with a soothing draught. In the evening, he was so feeble as to require sinapisms to the extremities.

Next day, after a purgative enema, the bodily heat improved. Pulse still only 42. Next day, he was feverish and vomited. Next day, slight general improvement; pulse 45. Vomiting had ceased, he was able to drink, and for the first time showed signs of consciousness, and *spoke quite distinctly*. From that date up to the twelfth day, improvement was gradual. From the twelfth to the nineteenth, his general condition was stationary; but the wound began to granulate, and suppuration was free. From this time onwards, improvement was rapid, till the complete cicatrization of the wound on the fortieth day. He is now perfectly well, has no pain in the head, speaks as well as ever he did, and has no defect of memory.—*Gazette des Hôpitaux*, January 3, 1867.

Part Fifth.

MEDICAL NEWS.

RETURN OF PROFESSOR LAYCOCK.

Professor Laycock, after nearly a year's absence on account of ill health, has resumed the duties of his chair. On the occasion of his introductory lecture to the course of Clinical Medicine, on 7th May, he was received by the students with marked enthusiasm. Professor Laycock has also begun his summer course of lectures on Medical Psychology in the University.

GOODSIR FELLOWSHIP IN ANATOMY AND PHYSIOLOGY.

After considerable discussion, the conditions of this Fellowship have been settled as follows:—The Fellowship is to be open to all Graduates in Medicine of the University of Edinburgh of not more than three years' standing at the date of competition, and to Licentiates of the Colleges of Physicians and Surgeons of Edinburgh who have attended within the University the same number of classes as are required for the M.B. degree. Subscriptions are now being received by the Treasurer, Dr Joseph Bell, No. 5 Castle Terrace, and the Secretary, Dr Craig Maclagan, No. 5 Coates Crescent, Edinburgh. The following members of Committee will also receive subscriptions, a list of which will be published in this Journal:—Edinburgh Committee: Principal Sir David Brewster, *President;* Sir Geo. Harvey, P.R.S.A.; Sir James Y. Simpson, Bart.; Dr Burt, V.P.R.C.P.E.; Dr Balfour; Dr Christison; Dr John Chiene; Dr Dunsmure, Pres. R.C.S.E.; Dr Matthews Duncan; Dr Handyside; Dr Maclagan; John Muir, LL.D; Dr Lyon Playfair, C.B.; Dr Smith, Pres. R.C.P.E.; Professor Syme; Dr Sanders; David Smith, Treas. Roy. Soc. of Edin.; Professor W. Turner, M.B.; Dr Andrew Wood: Dr Gairdner, Professor Lister, Professor Young, Glasgow; Dr J. Adamson, St Andrews; Dr Aitken, Inverness; Dr Borthwick, Dumfries; Dr Greig, Dundee; Dr Johnston, Stirling; Dr Mills, Thurso; Dr Stirling, Perth. London and South of England Committee (with power to add to their number): Dr T. S. Cobbold; Dr J. Soelberg Wells; Hon. Sec., Dr Dyce Duckworth, 70 Wimpole Street: Professor Acland, Oxford; Dr Bedoe, Clifton; E. R. Bickersteth, Esq., Liverpool; Dr J. Crichton Brown, Wakefield; R. Brown, Esq., Carlisle; Dr Carter, Leamington; Dr Donkin, Newcastle; Dr Falconer, Bath; Dr Fleming and Dr T. P. Heslop, Birmingham; Dr Jardine Murray, Brighton; Dr Hamilton Ramsay, Torquay; Dr Shearer, Liverpool; Dr John Thorburn, Manchester; Dr Roberts Thomson, Bournemouth; Dr T. J. Walker, Peterborough. Ireland: Professor Blythe, Cork; Professor Cleland, Galway; Professor Dickson, Dublin; Dr Moir, Belfast. Dr Mackay, R.N., Somerset House; Dr de Chaumont, Netley. Australia: Dr Cox, Sydney; Dr Paterson, Melbourne; Professor Smith, Sydney. Africa: Dr Abercrombie, Cape Town; Dr Kirk, Zanzibar. Canada: Dr John Berryman, St John's, New Brunswick; Professor G. Campbell, Montreal; Professor Maclaine, Kingston. China: Dr Dods, Hong-Kong; Dr Ivor Murray, Canton; Dr Watson, Nen-Chwang. India: Dr John Anderson, Dr Thomas Anderson, Dr Fayrer, Calcutta; John Miller, Esq., Madras; Dr James Miller, P. and O. Service, Bombay.

FRENCH INTERNATIONAL MEDICAL CONGRESS.

The following list of Corresponding Delegates for Great Britain has been published in the *Gazette Hebdomadaire* (26th April 1867):—London: Professor Thomson, Drs Beigel, Guéneau de Mussy, V. de Meric, the Editor of the Lancet, Dr Roth, Dr Wynter, Editor of the British Medical Journal, the Editor of the Medical Times and Gazette. BIRMINGHAM: Professor Foster. LIVERPOOL: Dr Jones of the Northern Hospital. SHELTON: Dr Barnard Davis. EDINBURGH: Professor Bennett, the Editor of the Edinburgh Medical Journal. DUBLIN: Professor Stokes, the Editor of the Dublin Quarterly Journal of Medical Science, the Editor of the Dublin Medical Press.

We remind our readers that this great International Congress will open on 16th August, under the auspices of His Excellency the Minister of Public Instruction. The meetings will be held morning and evening, and will be occupied by communications upon medical questions of importance. Certain of these questions have been fixed by the committee; such are:—On Tubercle and Tuberculisation; on the Causes of Death after Surgical Operations; on Measures for repressing Venereal Diseases; on the Influence of Dietary in the Production of Disease; on Conditions of Climate, Race, etc., influencing Menstruation in Different Countries; on Acclimatisation of European Races in Tropical Climates; on Entozoa and Entophytes in Man. (see No. of this Journal for November 1866, p. 476). These subjects will have the precedence at the morning sittings; but communications on any other subjects of interest may be brought before the Congress, on condition that the authors forward their manuscript to the General Secretary, Dr Jaccoud, 4 Rue Drouot, Paris, before 26th July. The papers will become the property of the Congress, and be printed subsequently by the committee at their discretion. *Any medical man wishing to attend the meetings may have his name enrolled by applying to the Editor of this Journal or any of the other Delegates.* No pecuniary contributions are required from members not resident in France.

There can be no doubt that very great benefits may arise from International medical conferences of the kind which the French Government has now initiated. The opportunities of personal acquaintance, the interchange of ideas, as well as the discussion of questions of universal or international importance, cannot fail to give an impulse to medical inquiries on an extended basis. Although as the first of the kind the Congress has many difficulties to encounter, we entertain no doubt that the meetings will be full of interest, and will obtain success. We trust that British Medicine and Surgery will be adequately represented on the occasion.

PUBLICATIONS RECEIVED.

Julius Althaus, M.D., etc.,—The Electrolytic Treatment of Tumours and other Surgical Diseases. London, 1867.
Béraud's Atlas of Anatomy: Part 11.
Bernutz and Goupil's Clinical Memoirs on the Diseases of Women. Translated for the New Sydenham Society, by Alfred Meadows, M.D., etc. Vol. ii. Lond., 1867.
Joseph P. Catlow, M.R.C.S.,—Principles of Æsthetic Medicine. London, 1867.
Th. Cocteau, M.D.,—Recherches sur les Altérations des Artères à la suite de la Ligature. Paris, 1867.
Horace Dobell, M.D.,—The True First Stage of Consumption. London, 1867.
Sir John Forbes, M.D.,—On Happiness in its relation to Work and Knowledge. 2d Edition. London, 1867.
F. James Gant, F.R.C.S.,—The Irritable Bladder. 2d Edition. London, 1867.
Thos. G. Hake, M.D.,—On Vital Force: its Pulmonic Origin, and the General Laws of its Metamorphosis. Lond., 1867.
W. H. Jones, M.D.,—The Management of Labour in Contracted Pelvis. Lond., 1867.
Journal of the Royal Agricultural Society of England. London, 1867.
G. H. Kidd, M.D.,—On Cephalotripsy. Dublin, 1867.
W. Marcet, M.D.,—A New Process for preparing Meat for Weak Stomachs. London, 1867.
Albert Mooren,—Ophthalmiatrische Beobachtungen. Berlin, 1867.
Nouveau Dictionnaire de Médecine et de Chirurgie pratiques. Vol. vi. Paris, 1867.
Proceedings of the Pathological Society of Philadelphia. Vol. ii.
Ch. Sedillot, M.D.,—De l'Evidement sous-périosté des Os. Paris, 1867.
I. Seeligmann, M.D.,—Bade: ses Eaux thermales, etc. Paris, 1867.
A. Vintras, M.D.,—The Repressive Measures in Prostitution. London, 1867.
Forbes Winslow, M.D.,—Light: its Influence on Life and Health. Lond., 1867.

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